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The impact of brief exposure and acceptance interventions on implicit verbal relations in spider-fear

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Contents

Thesis Abstract

Theories of implicit cognition suggest that behaviour is partly influenced by automatic processes of perception and memory (implicit cognition). An important implication of these theories is that patient self−report may not capture influential processes in psychological disorders (as some of these processes may not be available to self-report). For example, a patient may report that they are no longer anxious (based on their current awareness or willingness to disclose) but may retain implicit/hidden processing biases (e.g., in sensitivity to threat) that leave them vulnerable to relapse in the future. Evidence suggests that, for various psychological disorders, relapse following temporarily successful treatment is not uncommon; the literature around implicit cognition may help to improve understanding of relapse processes.

Investigation of implicit cognition has further clinical implications: for enhancing our comprehension of how existing treatment may be effective (e.g., through implicit and/or explicit processes) and of how to develop treatment that influences implicit (in addition to explicit) cognition. Researchers have now developed a number of methods for accessing/measuring implicit processes and these have been shown to predict behaviour in various psychological disorders.

An important question arising from the literature around implicit cognition and its potential role in psychopathology is: do existing treatment interventions affect implicit processes? More broadly, how malleable are implicit processes? Can implicit processes be changed in a way that supports desired functioning? Research to date is limited and contradictory in its findings. The present research contributed to knowledge by examining the effects of two treatment−analogue interventions on implicit relational processes. The two interventions (exposure and acceptance/defusion) examined in the present research were based on existing clinical treatments. Spider fear was examined as a test construct in this research.

The present research applied an implicit assessment procedure, intervention, and interpretive framework deriving from Acceptance and Commitment Therapy (and the underlying Relational Frame Theory). In this way, the present research attempted to draw together theoretically coherent aspects of basic and applied psychology to better understand the constructs of interest.

Towards the aim of testing the impact of brief exposure and acceptance interventions on implicit verbal relations in spider-fear (in addition to spider-fear-related self-report and behavioural indices), three specific objectives were identified:

1. To examine effects of exposure and acceptance interventions on implicit (and explicit) measures of spider fear
2. To test the predictive relationship between implicit (and explicit) spider fear and spider-approach behaviour
3. Combining the above, to examine intervention effects on behaviour (directly and/or via fear measures).

48 participants (from a non-clinical sample) were randomly allocated to receive one of the two interventions. Participants completed pre− and post−intervention measures of implicit (and explicit) spider fear and a post−intervention behavioural approach test.

Implicit fear incrementally predicted behaviour over explicit fear, replicating previous findings. However, neither intervention appeared to affect implicit fear. Interventions did have differential effects on explicit fear and overt behaviour; notably, defusion facilitated greater approach behaviour than exposure. Discussion centres on clinical and theoretical implications of the research, considering limitations and directions for future research.

Statement of contribution

The trainee contributed to all aspects of the research, including:

- Project design
- Application for ethical approval
- Literature reviewing
- Recruitment of participants
- Data collection
- Scoring of questionnaires and assessments
- Data entry
- Data analysis

Dr. Aidan Hart supervised the trainee throughout the research, offering guidance and advice on all aspects specified above.

The following members of course staff provided valuable feedback on aspects of the research that were submitted for formal marking (including the initial proposal, ethics application, literature review, and methodology): Dr Thomas Schröder, Dr Mark Gresswell, Dr Rachel Sabin-Farrell, Dr Dave Dawson, and Dr Roshan das Nair

Members of the ethics committees at the University of Lincoln and I-WHO reviewed the project proposal and materials. Their constructive comments facilitated clarification of the ethical justification for the present research.

The trainee would like to thank all the above-named for their contributions to the present research. Particular gratitude is reserved for Dr Aidan Hart, in recognition of his supportive and thought-provoking research supervision.

JOURNAL ARTICLE

The impact of brief exposure and acceptance interventions on implicit verbal relations in spider-fear[1]

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Abstract

Emerging research suggests that behaviour is partly influenced by automatic processes of perception and memory (implicit cognition). This research has important implications for treatment of psychological disorders. The present study aimed to test the impact of two intervention techniques on implicit (and explicit) spider fear in a non-clinical sample. The two interventions (exposure and acceptance/defusion) were based on existing clinical treatments. The study additionally examined whether implicit (and explicit) spider fear predicted behaviour towards the object of fear and whether intervention affected behaviour (either directly or through effects on implicit/explicit fear). 48 participants were randomly allocated to receive one of two interventions. Participants completed pre− and post−intervention measures of implicit (and explicit) spider fear and a post−intervention behavioural approach test. Implicit fear incrementally predicted behaviour over explicit fear, replicating previous findings. However, neither intervention appeared to affect implicit fear. Interventions did have differential effects on explicit fear and overt behaviour; notably, defusion facilitated greater approach behaviour than exposure. Results are interpreted in relation to existing literature and consideration of methodological limitations. A need for further research into the malleability of implicit cognition was identified, particularly in relation to existing clinical treatments.

Keywords: Implicit, Implicit Relational Assessment Procedure; Acceptance and Commitment Therapy; Cognitive defusion; Spider fear; Exposure

The impact of brief exposure and acceptance interventions on implicit verbal relations in spider-fear

Cognitive theories of implicit processes – processes that may be unavailable to self-report (Greenwald, et al., 2002) – have implications for clinical treatment of psychological disorders. These theories suggest that standard self-report of psychological state following treatment may not reflect latent residual relationships in memory (Teachman & Woody, 2003). For example, a patient may report that they are no longer depressed but may retain links in memory (possibly outside of 'conscious awareness') between the self and various negative evaluations. Theoretically, such links could develop from a learning history in which the self and negative evaluations have been repeatedly connected to the extent that they become relatively automatic and susceptible to uncontrollable activation (e.g., by transient emotional stimuli; Meites, Deveney, Steele, Holmes, & Pizzagalli, 2008). Residual automatic relations of this kind may leave the patient vulnerable to depressive relapse.

Evidence suggests that, for various psychological disorders (e.g., depression and anxiety), relapse following temporarily successful treatment is not uncommon (Brandon, Vidrine, & Litvin, 2007). The literature around implicit cognition may help to bolster understanding of relapse processes. Researchers have now developed a number of response-latency methods for accessing/measuring implicit processes (De Houwer, 2006) and these have been shown to have predictive validity pertaining to psychopathological behaviour: for example, in anxiety (Egloff & Schmukle, 2002), depression (Franck, De Raedt, & De Houwer, 2007), and substance use (Ostafin, Marlatt, & Greenwald, 2008). [Please see extended paper 1.1 for further discussion of relevance to clinical psychology].

An important question arising from the literature around implicit cognition and its potential role in psychopathology is: do existing treatment interventions affect implicit processes? More broadly, how malleable are implicit processes? Can implicit processes be changed in a way that supports desired functioning? Research to date is limited and contradictory in its findings (Gawronski & Bodenhausen, 2006). The present study contributes to knowledge by examining the effects of two treatment-analogue interventions on implicit relational processes. A number of recent studies have applied implicit measurement to spider fear as a test construct with potential clinical relevance (e.g., de Jong, van den Hout, Rietbroek, & Huijding, 2003; Teachman, 2007). The present study also examined spider fear, building on the findings of research to date.

Implicit cognition

There is growing evidence to suggest that, in addition to controlled/conscious processing (explicit cognition), some processing of information occurs automatically/outside of introspection (implicit cognition; Fazio & Olson, 2003; Gawronski & Bodenhausen, 2006). It may in practice be difficult to discriminate implicit cognition from particular conditions of measurement that are thought to capture implicit cognition (see section below). In the present paper, references to implicit cognition may most accurately be considered shorthand for references to a measure that is purported to reflect (the hypothetical construct of) implicit cognition.

Evidence for discrimination of implicit and explicit cognition constructs comes in part from findings of only moderate positive correlations between measures designed to tap these constructs (Nosek & Smyth, 2007). Alone, this evidence may only suggest that one or more of the measures are psychometrically weak. This is the least interesting potential interpretation regarding differences between (purported) implicit and explicit measures of cognition: that, rather than reflecting different constructs, they simply reflect a lack of overlap between measures that should pertain to the same target domain (e.g., a particular attitude or belief). Indeed, Payne, Burkley and Stokes (2008) found that increasing the structural fit (i.e., methodological similarity) of implicit and explicit measures increased correlations. Importantly, there is evidence that implicit and explicit measures differ (in consistent and meaningful ways) in their associations with other variables. A recent meta-analysis (Greenwald, Poehlman, Uhlmann, & Banaji, 2009) indicated that: (1) associations involving explicit (but not implicit) measures were moderated by (independently-rated) social desirability pressures; and (2) correlations between explicit (but not implicit) measures and behaviour were moderated by conscious controllability. Overall, implicit and explicit measures appeared to possess discriminant predictive validity: explicit (self-report) measures were better at predicting (target-relevant) behaviours that are planned/deliberate whereas implicit measures were complementarily predictive of unplanned/unintended behaviours. Taken together, it seems that 'implicit' and explicit measures tap distinct constructs that appear to be useful and influential, even though their definition is necessarily tentative and hypothetical (Greenwald & Nosek, 2009). [See extended paper 1.2 for discussion of three types of theoretical model that may account for the dissociable theoretical constructs of implicit and explicit evaluative cognition]

Measurement of implicit processes

A number of 'implicit measures' have been developed. Here, implicit refers to a particular set of conditions under which the outcome of measurement indexes the cognition being assessed. The outcome of an implicit measure indexes cognition even though the following conditions may be observed (De Houwer, 2006): (1) the participant is unaware of their cognition; (2) the participant is unaware that the outcome reflects their cognition; or (3) the participant has no control over the outcome. In contrast, outcomes of traditional explicit measures (e.g., verbal self-report) index cognition under conditions where the participant is aware of response meaning/occurrence, and the response is controllable. Arguably, an additional characteristic distinguishing implicit from explicit measures is a requirement for greater efficiency of processing (Bargh, 1994; Nosek, 2007). However, it is acknowledged that implicit measures are unlikely to be process-pure (Conrey, Sherman, Gawronski, Hugenberg, & Groom, 2005): it is unlikely that any measure will capture entirely automatic versus controlled processes (or vice-versa).

Measures of implicit processes typically use differences in response latencies as an indicator of the strength/salience of stimuli (and relationships between stimuli) in memory. The validity of these measures is supported by evidence suggesting that: (1) they tap constructs that are difficult to assess through self-reports, and (2) they reliably predict behaviour (Hofmann, Gawronski, Gschwendner, Le, & Schmitt, 2005; particularly more spontaneous behaviours and non-verbal actions). [For further discussion of findings for the predictive validity of implicit (over explicit) measures, see extended paper 1.3]

The most established of these measures is the Implicit Association Test (IAT; Greenwald, McGhee, & Schwartz, 1998). The IAT was designed to measure the relative strength of pairs of associations (e.g., snake--fear vs. spider--fear) in a computerised categorisation task. For example, relatively rapid responding to spider--fear (in comparison with snake--fear) would be considered indicative that spider and fear are more closely associated in memory than snake and fear. However, the original IAT has inherent design flaws that limit interpretation of responses (see De Houwer, 2002). Chiefly, the IAT is not informative about the independent/absolute strength of associations. For the previous example, the IAT could not indicate whether spiders are feared or not feared, only that they are *relatively* more or less feared than snakes. Subsequently developed measures – such as the Extrinsic Affective Simon Task (EAST; De Houwer, 2003) and Go/No-go Association Task (GNAT; Nosek & Banaji, 2001) – have addressed some of these limitations. However, these measures retain an important limitation: whilst they may be indicative of the strength of associations in memory, they cannot gauge the direction or nature of an association (i.e., exactly how concepts are related to each other): in actuality, human cognitions often seem to involve complicated, conditional relationships between multiple concepts (relational networks; McKenna, Barnes-Holmes, Barnes-Holmes, & Stewart, 2007).

Implicit Relational Assessment Procedure.

A recent development in implicit measurement is the Implicit Relational Assessment Procedure (IRAP; Barnes-Holmes, et al., 2006). The IRAP was developed on the basis of Relational Frame Theory (RFT; Hayes, Barnes-Holmes, & Roche, 2001): a modern behavioural theory of human language/cognition. RFT posits that core components of human cognition are relational processes rather than mere associations. These processes allow us to arbitrarily relate different stimuli to one another independently of actual relations and account for our ability to learn indirectly (in a way that a purely associative mechanism – as targeted by the IAT – could not). The IRAP involves presenting specific relational terms (e.g., similar, opposite; true, false; more, less) facilitating assessment of the properties of relations between stimuli (termed verbal relations) – in contrast to other implicit measures. The basic IRAP hypothesis is that participants will give faster responses on trials where the stimulus and required response are compatible with their private beliefs/relations (e.g., I fear-spider-true) than on belief-incompatible trials (e.g., I fear-spider-false). It is assumed that participants are slower to respond overtly when the response required goes against their more probable private relational responses (i.e., relational responses that are more readily activated because of historical and current contextual factors). Given the potential advantages of the IRAP over other implicit measures, its theoretically-grounded development, and growing empirical support (Cullen, Barnes-Holmes, Barnes-Holmes, & Stewart, 2009) the IRAP was implemented as the implicit (primary outcome) measure in the present study. [See extended paper 1.4 for discussion of the validity of the IRAP as a measure of implicit cognition].

Interpretation of implicit-explicit dissociation in terms of RFT has been considered by Barnes-Holmes, Barnes-Holmes, Stewart & Boles (2010). These authors propose the Relational Elaboration and Coherence (REC) Model. The REC model assumes that the IRAP effect reflects immediate relational responses whereas explicit measures reflect extended and coherent relational networks. Divergence between implicit and explicit cognition is assumed to result when immediate or automatic responses do not cohere with subsequent (more elaborated) relational responding. Given sufficient time, people may reject/reappraise their immediate responses on the basis of more elaborated relational processing (e.g., identifying less salient relations in a network that counter an initial response driven by more salient/immediate relations) or to cohere with other relevant relational networks (e.g., networks reflecting considerations of self-presentation or political sensitivity). If a person's immediate relational response is consistent with more extended relations and coherent with other relevant networks, implicit and explicit cognition would be expected to converge. With reference to other theoretical interpretations of the implicit-explicit dissociation (Greenwald & Nosek, 2009), the REC model posits a single representation with differences explained by level of elaborative processing [see extended paper 1.2 for discussion of other theoretical interpretations]. The REC model may be able to account for findings in implicit cognition, and to relate these findings to RFT theory/research, but further study is required to test its assumptions (Barnes-Holmes, Murphy, Barnes-Holmes, &

Stewart, 2010). [On the basis of its integration with RFT and consideration of available models in extended paper 1.2, findings of the present study were considered in relation to this model. See 3.2.1.1 for elaborated discussion].

Influencing implicit cognition

To date, there is little consistent empirical evidence to support specific methods of implicit cognitive change (Banaji, 2001; Huijding & de Jong, 2007; Rydell, McConnell, Strain, Claypool, & Hugenberg, 2007). Implicit measures are commonly conceptualised as reflecting relations that have been established slowly over time (from repeated experiences), and it has been inferred from this that changes to underlying (implicit) cognition would be slow to effect (Rydell & McConnell, 2006; Smith & DeCoster, 2000). However, there is growing evidence to suggest that implicit cognition is malleable by new learning and situational context (Blair, 2002; Plant, et al., 2009; Thush, et al., 2009). A review of available evidence (Gawronski & Bodenhausen, 2006) suggested that a general feature of procedures found to affect implicit processes is that they involve either: (1) repetitive conditioning that weakens existing implicit relationships/establishes novel relationships; or (2) changing activation of pre-existing networks by simply cueing a different pattern of relations with a target concept (e.g., changing the context within which the target is considered).

Looking across available empirical data, and considering implications of the REC model and other connectionist models (Barnes & Hampson, 1993), the present investigator posited that relational salience (or relative strength) may be more critical than relational history per se. Particular relational networks might be prominent (more automatically activated) because of early-life establishment and/or repeated experience over time (e.g., Rudman, Phelan, & Heppen, 2007), but more recent experiences (especially if novel or affectively-charged) will also affect the activation (and so prominence) of relations. Further, different contexts may come to moderate the activation of relational networks. It may be that changes introduced by recent experiences typically have a short duration and more lasting changes are brought about only by repetition-based learning (or perhaps highly significant/activating single-exposure learning). Similarly, an implication of context-moderated malleability is that it will not produce change that is stable (generalises) across situations. Questions of change stability are beyond the scope of the present study but warrant further attention.

It has been argued that it is a priority to assess the effect of existing interventions on implicit cognition (e.g., Wiers, de Jong, Havermans, & Jelicic, 2004). The present study looked at analogues of two existing intervention procedures (described in detail in the methods section); these procedures were tentatively predicted to influence implicit processes as they appear to function in ways identified by Gawronski and Bodenhausen (2006) as potentially effective. One, exposure, involves repetitive desensitisation to target stimuli and the other, cognitive defusion, involves changing the context/activation of target words.

The present study

Given present gaps in understanding around the malleability of implicit cognition (particularly with respect to psychological interventions), further investigation was considered timely. As indicated above, the present study looked at the effects of two basic treatment-analogue interventions on implicit spider-fear responses (in addition to explicit spider-fear responses and overt behaviour towards spiders[2]). [See extended paper 1.5 for rationale regarding choice of spider-fear as a test construct]

The rationale for an exposure intervention is that repeated exposure to spider stimuli could lead to habituation of emotional responses to spiders that weakens internal verbal relations (between spiders and personal fear); this may be in implicit relations, explicit relations, or both. A recent study by Veltman et al. (2004) demonstrated effects of computerised exposure to spider images on physiological responses (and physiological responses have previously been shown to be predicted by implicit versus explicit measures of cognition; Egloff, Wilhelm, Neubauer, Mauss, & Gross, 2002). Teachman and Woody (2003) demonstrated effects of cognitive-behavioural therapy (including in vivo exposure treatment) on an implicit measure, but limitations of the IAT used in this study obfuscate interpretation (Huijding & de Jong, 2007). The planned cognitive defusion intervention is derived from

[2] For purposes of definition, explicit measures are considered synonymous with self-report (interview/questionnaire) measures. Behavioural measures will generally be referred to as a separate form of measurement (although it is acknowledged that behaviour may be considered an explicit manifestation of a given construct).

Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 1999): an empirically based therapeutic approach that uses acceptance and mindfulness strategies (together with commitment and behaviour-change strategies) to increase psychological flexibility. The ACT approach is based on RFT, and so was developed within the same theoretical framework as the IRAP. In principle, defusion (a core intervention in ACT) should result in a breaking down of existing problematic verbal relations or shift in the context/activation of relations (implicit, explicit, or both). There is accumulating evidence for the general clinical utility of defusion techniques (Healy, et al., 2008). [See extended paper 1.6 for further discussion of intervention techniques].

Aims and objectives

The aim of the study was to examine the impact of brief exposure and acceptance interventions on implicit verbal relations in spider-fear.

Objectives. Relating to the aim above, three objectives were specified:

1. To examine effects of exposure and acceptance interventions on implicit (and explicit) measures of spider fear
2. To test the predictive relationship between implicit (and explicit) spider fear and spider-approach behaviour
3. Combining the above, to examine intervention effects on behaviour (directly and/or via fear measures).

Due to a lack of previous evidence and conflicting theoretical hypotheses, it was difficult to make specific predictions about how the applied interventions would affect implicit fear relations. A tentative prediction was that both interventions would reduce implicit spider-fear responses (Keogh, 2008). Explicit measures were examined as secondary outcomes in the present study.

The second objective was a test of the predictive utility of the implicit measure (over and above the explicit measure). This test was a replication of a previous study (Cochrane, Barnes-Holmes, & Barnes-Holmes, submitted for publication) which showed that implicit spider fear predicted unique variance in spider approach behaviour (see also Teachman & Woody, 2003). Replication of this finding within the present study would emphasise the relevance of testing intervention effects on implicit processes and the implications of any intervention effects.

The third objective integrates previous objectives to examine (direct or mediated) effects on behaviour. From an ACT perspective, overt behavioural tasks represent the most valid test of the effects of defusion and other interventions (Masuda, Feinstein, Wendell, & Sheehan, 2010). ACT techniques are intended to facilitate valued action that is independent of psychological discomfort (Hayes, Strosahl, et al., 1999), rather than change this discomfort directly (Hayes, Luoma, Bond, Masuda, & Lillis, 2006), so it may be that intervention facilitates approach behaviour in the absence of discernible effects on implicit or explicit spider-fear. The only other study to have examined effects of an acceptance/defusion intervention on spider avoidance (Wagener & Zettle, in press) found that the acceptance condition facilitated greater progress on a spider-approach task than other conditions (cognitive control or psychoeducation). In consideration of this, it was hypothesised that being in the defusion condition would predict less avoidance in the present study. Interestingly, Wagener and Zettle (in press) found that acceptance reduced avoidance but *not* self-reported distress, indicating that any effect in the present study might not be mediated by fear measures (although they did not use implicit measures).

Method

Participants

Forty-eight participants (14 men and 34 women) were recruited from across a University population (staff/students from the University of Nottingham) by advertisements across various media (posters around campus, email circulation, and an online message board). Age ranged between 19 and 64 with a median age of 22.5 (inter-quartile range 21-26). The majority of participants (85%) were students, rather than staff, at the University. Participants were randomly assigned to one of two intervention conditions (both n = 24).

Six prospective participants declined to follow up their initial interest in the study after reading and considering the participant information sheet. [See extended paper 1.7 for further information about recruitment and sample size calculation]

Ethical approval

The proposal was approved by Ethics Committees at the Universities of Nottingham and Lincoln (in a parallel submission). [See extended paper 3.3 for discussion of ethical issues.]

Measures

Quantitative (scaled and categorical) data was collected using a number of validated instruments and descriptive self-report items (detailed below).

Demographics. Only basic demographic information (age, gender, student/non-student status) was obtained: this information was considered useful for describing the university sample such that inferences about comparability with other university samples can be made (e.g., in making sense of obtained findings in relation to previous research); it was also considered useful to control for these basic factors in secondary analyses. Minimising collection of personally identifying information helped to protect anonymity of obtained data and reduce (unnecessary) participant burden/fatigue.

Fear of Spiders Questionnaire (FSQ). The FSQ (Szymanski & O'Donohue, 1995) was used for assessing spider fear (through explicit self-report). The FSQ is an 18-item instrument; participants rate their agreement with statements such as "If I came across a spider now I would leave the room" on a 7-point Likert-type scale (0 = strongly disagree, 6 = strongly agree). Total FSQ scores are obtained by summing ratings from all items, such that scores range from 0-108 with higher scores indicating greater spider fear. Although the total score is most commonly used, the FSQ has been shown to have a two-factor structure: assessing underlying avoidance/help-seeking and fear of harm. The FSQ has demonstrated good internal reliability (alpha=.92; Szymanski & O'Donohue, 1995). The measure can discriminate phobic from non-phobic individuals, has good test-retest reliability (alpha=.91), and is sensitive to change following both cognitive restructuring treatment and behavioural exposure treatment (Muris & Merckelbach, 1996). The sensitivity to change of the FSQ suggested that it would be an appropriate measure of pre- and post-intervention fear in the current study. With further relevance for the present study/sample, the FSQ is sensitive to low levels of self-reported spider fear, making it appropriate for use with non-phobic participants (Muris & Merckelbach, 1996). Cochrane et al. (submitted for publication) found that the FSQ demonstrated good internal consistency in a university student population (alpha=.96).

The FSQ demonstrated excellent internal consistency in the present sample (alpha=.97).

Fear and disgust ratings. Subjective affective responses to presented stimuli were assessed at various points during the experimental procedure. The rating scales used in the present study were drawn from a recent study by Gerdes, Uhl and Alpers (2009). Participants were asked to rate stimuli according to how frightening and disgusting they were perceived to be; ratings were given on a 10-point scale anchored at 0="not at all" and 9="extremely." Gerdes et al. (2009) found that ratings of fear and disgust (but not danger) in response to spider images predicted spider fear measured by a validated screening questionnaire.

Explicit fear and disgust responses to spider images were shown to have internal consistency in the present sample (.97 for fear ratings; .95 for disgust ratings).

Perceived Threat – Behavioural Approach Test (PT-BAT). The PT-BAT is an automated test of behavioural approach to spider stimuli (Cochrane, Barnes-Holmes, & Barnes-Holmes, 2008). Participants were asked to insert their hand into a series of seven opaque jars[3]. The seven jars in this series were labelled to suggest that, in sequence, the jars present incrementally more aversive tasks: (1) empty; (2) had spider inside; (3) 25% [chance of spider inside]; (4) 50%; (5) 75%; (6) 100%; (7) big spider.

Two studies have demonstrated that the PT-BAT can discriminate between low- and high-fear groups (significant differences in number of steps completed; Cochrane, et al., 2008, submitted for publication).

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[3] These were adapted from the original eight jars used in the PT-BAT to minimise response burden. Cochrane et al. (2008) argue that a strength of the PT-BAT is that its 'contents' can be readily adjusted, reducing or increasing the number of steps as required. In line with research indicating that individuals heuristically reduce judgment of probabilities to quarters (Konold, 1995) jars for 20, 40, 60, and 80 percent probabilities were adapted to 25, 50, and 75 percent.

Wagener and Zettle (in press) report a positive correlation between FSQ score and PT-BAT performance.

The rationale for development of the PT-BAT came in part from a study demonstrating that an 'unseen' spider stimulus (an opaque container that was reported to contain a spider) provoked greater physiological reactivity than a live tarantula in a transparent container (Castaneda & Segerstrom, 2004). This suggested that it was possible to elicit spider fear, and potentially gauge related behaviour, without exposing participants to real spiders (or even visual cues). Given additional ethical issues (including the use of animals in research), and the fact that access to and management of live spiders can be difficult and resource-intensive (Meng, Kirkby, Martin, Gilroy, & Daniels, 2004), the development of alternative 'spiderless' behavioural measures seemed potentially advantageous.

Implicit Relational Assessment Procedure (IRAP). The IRAP software was used to present stimuli and record participant responses. On each trial, one of two label stimuli ("I fear" or "I do NOT fear") and a colour image of either a spider or a snake were presented. Two response options ("True" and "False") were also presented on each IRAP trial. Spider and snake images were identified from an online searchable database of images available for use under a creative commons license (http://creativecommons.org). Snake images were included as a naturally primed fear stimulus comparable to spider images (Teachman & Woody, 2003); inclusion of the snake images allowed checking of the (spider) specificity of intervention procedures but matched 'control' stimuli were not necessary: IRAP scores for the different image types are calculated independently. Similarly, although images were standardised in size for presentation, controlling for image properties (e.g., matching attributes of spider and snake images) was not required as each target image acts as its own control in the IRAP procedure. IRAP was scored in terms of differences in response latency between consistent and inconsistent trials (individual effect sizes; discussed further in the data processing section). Split-half reliability of the spider-fear IRAP is adequate (.60) and slightly better than reliability in comparable implicit measures (Cochrane, et al., submitted for publication).

Because the present study was designed to examine effects on implicit processes, and the IRAP was used as the primary outcome measure, it is important to consider available evidence for the validity of the IRAP as an implicit assessment tool. [Section 1.2 of the extended paper examines the validity of the IRAP in detail – and in relation to alternative measures of implicit cognition]. The reader is referred to the background section for elaborated discussion of the mechanics of the IRAP and its practical advantages over other available implicit measures.

Procedure

The experiment consisted of three phases (see Figure 1). In Phase 1, participants completed implicit and explicit measures of spider fear. In Phase 2, participants were exposed to one of two automated intervention tasks (exposure or cognitive defusion): these tasks were basic analogues of clinical interventions, administered to a non-clinical sample for the purposes of the research (examining possible mechanisms of influence through effects on implicit cognition/relations). In Phase 3, participants again completed the measures administered in Phase 1 and they were also asked to perform a behavioural approach task (the PT-BAT). In total, the procedure took approximately one hour to complete (in both intervention conditions). The researcher (a trainee clinical psychologist) was present to guide/assist the participant through all stages of the procedure, although most instructions/tasks were automated and presented on computer screen. The experiment was carried out in an experimental cubicle at the University of Nottingham/Lincoln (as appropriate); experimental sessions were run serially. The experimental procedure was administered to each participant on a PC using the PsychoPy software (Peirce, 2007) to present instructions, stimuli, and record responses for most components of the procedure. The IRAP task was run as a stand-alone software program on the same PC.

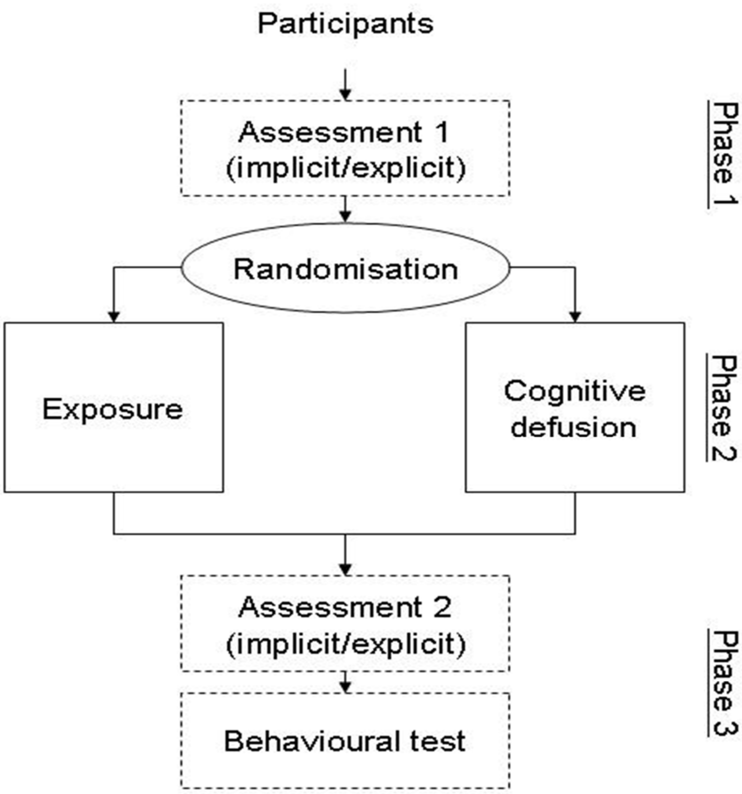

Figure 1. Flowchart representing the 3-phase study procedure.

Phase 1. *Implicit measure.* The IRAP computer program included standardised onscreen instructions. Participants were able to read the instructions in their own time, pressing the space bar to advance. The instructions described the IRAP process, how to complete the task, and emphasised that both accuracy and speed would be required. Participants were not informed as to which tasks would be deemed to be consistent or inconsistent.

On each IRAP trial, four stimuli were presented concurrently. The label stimulus (either "I fear" or "I do not fear") appeared at the top of the screen, the target picture (either spider or snake) appeared at the centre of the screen, and the two response options ("True" and "False") appeared in the bottom corners of the screen. All four stimuli remained visible on screen until a participant chose one of the two response options: pressing the "D" key to select the left option or pressing the "K" key to select the right option. The left/right positioning of "True" and "False" responses was alternated randomly across trials.

If a participant gave the correct response for a given trial, all four stimuli disappeared from the screen and there was a 400ms inter-trial interval (blank screen). If a participant gave an incorrect response, a red X appeared immediately below the target picture and remained onscreen (with the other four stimuli) until the correct response is given.

The IRAP consisted of a minimum of two practice blocks plus a fixed set of six test blocks; each block contained 24 trials. Only data from test blocks was used for the purposes of analysis. Within each block, the six target pictures were presented in a quasi-random sequence such that each picture was presented four times – twice with each label stimulus ("I fear" and "I do not fear").

The initial (practice) trial-block required participants to produce responses consistent with spider-fear (see Figure 2). For example, if the label "I fear" and any of the spider images appeared concurrently on screen, the defined correct response would be "True"; if "I fear" and any of the snake images appeared concurrently on screen, the correct response would be "False." After a participant completed 24 trials in the first practice block they were presented with feedback indicating the percentage of correct responses and median response time (across the 24 trials). Subsequently, between-block instructions were presented informing the participant that the previous correct/incorrect responses would be reversed in the next block.

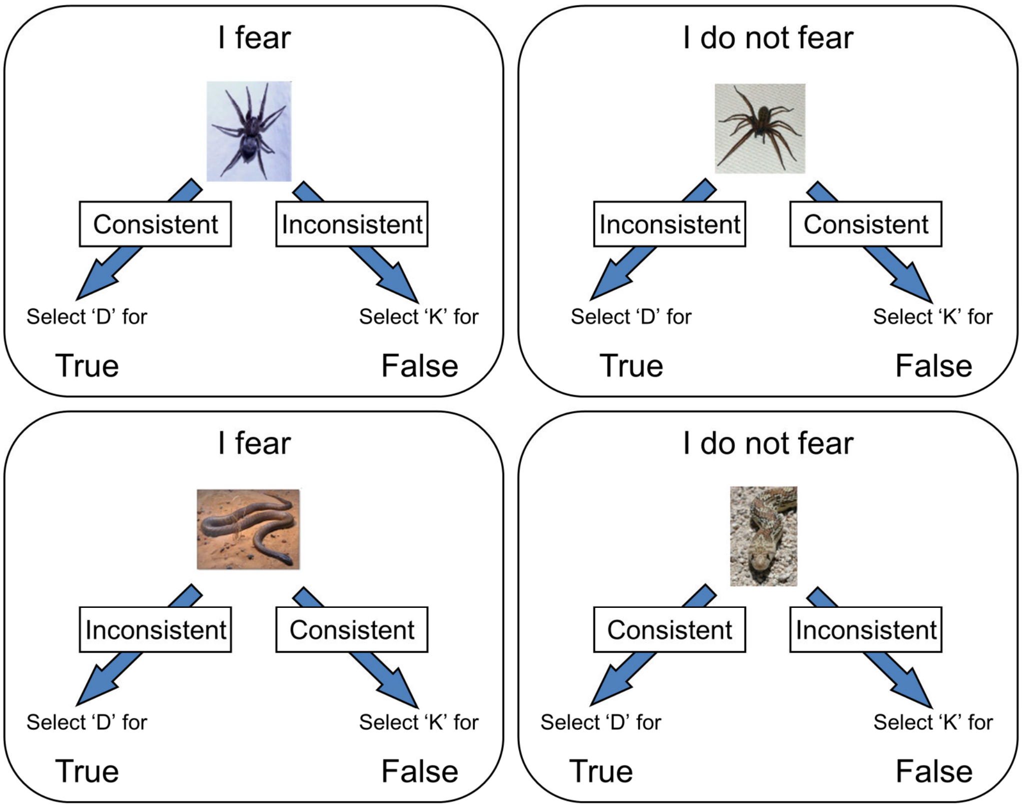

Figure 2. Examples of the four IRAP trial-types. The arrows with text boxes show responses consistent/inconsistent with spider-fear (arrows and boxes did not appear onscreen). The defined 'correct' response varied by block: consistent response options were correct in spider-fear consistent blocks and inconsistent response options were correct in spider-fear inconsistent blocks.

The second (24-trial) block required participants to produce responses inconsistent with spider-fear: by providing a response pattern opposite to the pattern described for the first practice block. Participants who met practice criteria (>80% correct and median latency <3000ms) during the first, second, third, or fourth exposure to the practice-block pairs continued with the six test blocks (i.e., if a participant met the practice criteria during the first pair of practice blocks they moved on to the test blocks without being presented with further practice-block pairs). Each successive pair of test blocks was identical to the previous practice-block pairs, except that participants were instructed "This is a test. Go fast. Making a few errors is okay."

Explicit measures. After completing the IRAP, participants were asked to complete the Fear of Spiders Questionnaire and rate the spider stimuli (three images) presented in the IRAP for fear and disgust.

Phase 2.

Exposure intervention. Half of the participant sample was (randomly) allocated to receive a computerised exposure intervention[4]. This intervention involved two steps (sessions) of graded exposure to spider stimuli (colour still images of spiders followed by colour videos of moving spiders). The components of the exposure/habituation intervention were drawn from two recent studies demonstrating effects of brief exposure to spider stimuli on physiological responses and explicit self-reports (Tabibnia, Lieberman, & Craske, 2008; Vansteenwegen, Vervliet, Hermans, Thewissen, & Eelen, 2007b).

Participants were first exposed to a series of spider pictures (Tabibnia, et al., 2008). This exposure session consisted of 72 trials: 12 spider pictures were presented six times each (picture stimuli did not replicate spider stimuli used in the IRAP; images were obtained from an online creative commons resource, discussed above in relation to the sourcing of IRAP snake images). Each trial began with the presentation of a spider picture for 3500ms; subsequently, an unrelated neutral text stimulus was presented for 2500ms, and this was followed by 6000ms of a blank screen. Presentation of neutral words following each exposure was found to augment exposure effects in the study by Tabibnia et al. (2008) and is theoretically relevant for the present study in that the activation of unrelated words following exposure to spider stimuli may interfere with more established verbal relations around spiders (potentially weakening previously learned implicit/explicit networks). Lagged presentation should allow full attention to the spider stimulus whilst present; presenting stimuli simultaneously would likely increase demands on attention and may consequently interfere with exposure effects (Parrish, Radomsky, & Dugas, 2008).

As each trial lasted for 12000ms, the first exposure session was 14.5 minutes in duration. Before the exposure session began, participants were informed (onscreen) that they would see a number of spider images, and that each image would be shown several times. They were instructed that, although the images may be difficult to look at, they should try to fixate on

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[4] Randomisation was achieved using a restricted random allocation rule to obtain equal groups (equivalent to single permuted-block randomisation). Selection was made using the true random number generator at www.random.org

the pictures whilst presented and try to remember the features of each image (no recall task was actually presented, but this was not clear from the instructions provided). Participants were also informed that they would occasionally see single words presented on screen and instructed that they should read these words silently to themselves.

A second exposure session involved exposure to videos of spiders in different contexts (Vansteenwegen, et al., 2007b; video stimuli were obtained from the first author). This session consisted of eight video presentation trials: four one-minute video clips were shown two times each. Participants were instructed to carefully view each video; to try to imagine that they are in the room shown onscreen; to focus on the spider; and not to suppress their emotional response. During inter-trial intervals, participants were asked to rate the fear and disgust that they experienced during the preceding video presentation. As each trial plus rating interval lasted for approximately 1.5 minutes, the second exposure session took around 12 minutes to complete.

Cognitive defusion intervention. The other half of the participant sample was (randomly) allocated to receive a computerised defusion intervention. This intervention was adapted from exercises presented in Hayes and Smith (2005) – and empirically supported by Masuda and colleagues (Masuda, Hayes, Sackett, & Twohig, 2004; Masuda, et al., 2008) – that are designed to weaken problematic relations among private events (i.e., implicit and explicit verbal networks) by teaching the reader to see thoughts and feelings for what they are (a verbally enmeshed process) rather than what they seem to be (literal reality; Hayes, Strosahl, et al., 1999).

At the start of the cognitive defusion session, a rationale was presented for reading onscreen (approximately five minutes of reading time). The rationale (similar to a brief rationale used in Masuda, et al., 2004) highlighted the benefits of literal language and thinking (including the capacity for logical problem-solving and resultant management of the environment), but also stressed the contribution of language/thought to suffering. Participants were informed that negative thoughts may be relatively automatic but that people can become "fused" with the literal content of thoughts: compare "I am anxious" with "I am having the feeling that I am anxious"; the former inflexibly fuses self with thought. To demonstrate the notion of fusing with literal content, participants were asked to think about the word "milk" (what it is like; what it looks like/feels like) and type a few attributes of milk that come to mind (e.g., white, liquid, cool). The participant was then asked to say "milk" repeatedly out loud for 30 seconds (speaking as fast as possible while clearly pronouncing the word), notice what happens, and record their experiences (typing in a response box). People completing this task tend to find that the meaning of the word falls away and more formal properties come to the fore (e.g., the sound of the word; Hayes & Smith, 2005). The participant was then asked to apply the same procedure to the words "spider" and "terrified" (with relevance to spider fear), drawing attention to the fact that potentially aversive thoughts are also just words/images and thus changing the relationship to thoughts (in a way that, theoretically, may weaken or redefine existing implicit or explicit relations, such as relations between the self, spiders, and fear).

A second defusion exercise – described in Hayes and Smith (2005) and implemented (in a different form) in previous experimental interventions (Gutiérrez, Luciano, Rodríguez, & Fink, 2004; McMullen, Barnes-Holmes, Barnes-Holmes, Stewart, Luciano, & Cochrane, 2008) – was used to promote defusion by demonstrating that thoughts are not causes. Participants were asked to repeatedly type the phrase 'I cannot type' until they had filled a text box onscreen. Again, the principle is to deliteralise thinking in a way that may facilitate more flexible behaviour (e.g., responding in a manner inconsistent with previously learned relations in IRAP tasks).

The final intervention was matched for duration with the exposure intervention, following piloting.

Phase 3. Participants again completed the implicit measure (IRAP) and explicit measures (FSQ, subjective ratings) as in Phase 1 (described above). Participants were subsequently asked to complete the PT-BAT as a check of their actual behaviour towards (the perceived threat of) spiders.

Instructions relating to the PT-BAT were presented onscreen. During this test, each participant was asked if they would be willing to put their hand into a series of opaque jars, keeping their hand in each jar for 30 seconds. Participants were able to discontinue the test at any stage. The test was terminated if the participant indicated unwillingness to place their hand in the next jar (by pressing the key assigned to a "no" response). If the "yes" key was pressed, instructions asked the participant to put their hand in the next jar in the series; an onscreen message indicated when 30 seconds had elapsed (and the researcher observed compliance). The participant was then instructed to make subjective ratings with respect to the preceding step in the test using mouse-operated sliding scales (presented onscreen): rating (a) unpleasantness, (b) emotional intensity, and (c) unwillingness to put their hand in the jar. After rating the preceding step, participants were presented with instructions for the next jar; this process continued until either the participant terminated the test or they completed all seven steps in the series.

Preparation of the IRAP data

Raw IRAP response latency data (time in milliseconds between trial onset and participant response) was transformed into *D*-IRAP scores using procedures outlined by Barnes-Holmes and colleagues (Barnes-Holmes, Murtagh, Barnes-Holmes, & Stewart, 2010; Vahey, Barnes-Holmes, Barnes-Holmes, & Stewart, 2009) – and response outliers were handled accordingly. Transformation to *D*-IRAP scores – normalised indices of response-latency differences between consistent and inconsistent blocks of IRAP tasks – controls for individual variability in response speed relating to extraneous factors (such as differences in cognitive ability). The following steps were used to *D*-transform raw response-latency data for each participant:

1. Only data from test blocks will be used;
2. Latencies above 10,000 ms will be removed from the dataset;
3. Data will be removed for a participant if more than ten percent of test-block trials have latencies <300 ms;
4. 12 standard deviations will be computed for the four trial types: 4 for the response latencies from across test blocks 1 and 2, 4 from test blocks 3 and 4, and a further 4 from test blocks 5 and 6;
5. 24 mean latencies will be computed: one for each of the four trial types in each test block;

6. For each pair of test blocks, the mean latency of each trial type's consistent test trials will be subtracted from the mean latency of their corresponding inconsistent test trials, computing difference scores;

7. Each difference score will be divided by its corresponding standard deviation (from step four), producing 12 *D*-IRAP scores (i.e., one score for each trial type for each test-block pair);

8. Four overall trial-type *D*-IRAP scores will be calculated by averaging the three scores for each trial type across the three test-block pairs; and

9. Two compound DIRAP scores, one for spider target images (spider D-IRAP) and one for snake target images (snake D-IRAP), will then be calculated by averaging the two spider and then the two snake trial-type D-IRAP scores from step eight.

D-IRAP scores and all other data collected were entered into SPSS for analysis.

Results

The first two sections of the results detail initial analyses of the pre-intervention explicit and implicit measures: it was necessary to establish that these measures performed in the expected way (e.g., basic IRAP effects and implicit-explicit correlation) to be able to interpret analyses pertaining to the main study objectives. ANOVAs examining changes from pre- to post-intervention addressed objective one, and a regression analysis addressed objectives two and three. [See extended paper section 2 for additional analyses and details of relevant assumption tests: the extended results follow the same sequence as the present journal results]

Preliminary analysis of explicit measures

Randomisation successfully produced intervention groups that were similar with respect to baseline spider fear, as scored on the Fear of Spiders Questionnaire (FSQ). A between-participants t-test demonstrated that there was no significant difference in FSQ score between the exposure (*M* = 37.50, *SD* = 29.56) and defusion (*M* = 35.17, *SD* = 33.53) conditions (*p* = .80).

To replicate previous analyses by Cochrane et al. (submitted for publication), and explore the discriminative validity of the IRAP, the sample was further grouped with respect to level of spider fear. Thus, participants were divided into low- and high-fear groups according to a median-split of scores on the FSQ. The mean score for the low-fear group (n = 24) was 11.5 (*SD* = 9.17); for the high–fear group (n = 24) it was 61.2 (*SD* = 25.09).

Planned comparisons demonstrated that explicit fear ratings (in response to spider images) were significantly greater in the defined high-fear group (*M* = 39.79, *SD* = 12.36) relative to the low-fear group (*M* = 21.50, *SD* = 11.82), $t(46) = 5.24$, $p < .001$, $d = 1.51$, $CI_{.95} = 0.86, 2.15$. Similarly, explicit spider disgust ratings were significantly greater in the high-fear group (*M* = 37.04, *SD* = 14.22) relative to the low-fear group (*M* = 19.13, *SD* = 12.29), $t(46) = 4.67$, $p < .001$, $d = 1.35$, $CI_{.95} = 0.71, 1.97$. Thus grouping on the basis of FSQ scores differentiated participants with respect to other explicit measures of spider aversion (spider fear and disgust ratings).

Pre-intervention IRAP analyses

For spider stimuli, positive D-scores reflect shorter response latencies on spider-fear (versus non-spider-fear) blocks. Similarly, D-scores for snake stimuli have been scored so that positive D-scores reflect shorter response latencies on snake-fear relative to non-snake-fear blocks. In this way, positive D-scores reflect relatively faster responding to fear-consistent relations for both spider and snake stimuli. Figure 1 presents the D-IRAP scores for low- and high-spider-fear groups, showing a positive IRAP effect in each case.

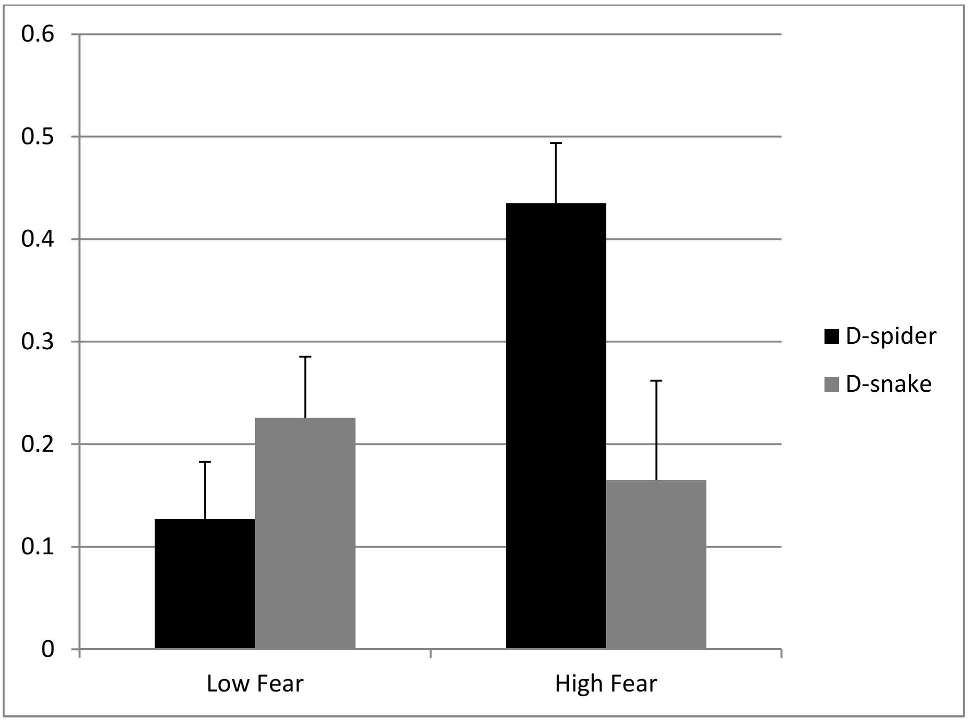

Figure 1. Pre-intervention D-IRAP scores by group (low-/high-fear). Figure shows means and standard error bars.

Four one-sample t-tests were conducted to determine whether each of the D-scores differed significantly from zero. Spider D-scores were significantly above zero (indicating spider fear) in both high- and low-fear groups, and the effect size for the high-fear group was more than three times that of the low-fear group (high-fear, $t(23) = 7.43$, $p < .001$, $d = 1.52$, $CI_{.95} = 0.92, 2.10$; low-fear, $t(23)=2.27$, $p = .003$, $d = .46$, $CI_{.95} = 0.03, 0.88$). The snake D-score was significantly greater than zero (indicating snake fear) in the low-fear group ($t(23) = 3.81$, $p = .001$, $d = 0.78$, $CI_{.95} = 0.31, 1.23$) but not in the high-fear group ($p = .10$).

A 2 x 2 mixed ANOVA yielded non-significant main effects for trial-type ($F(1, 46) = 1.39$, $p = .25$) and group ($F(1, 46) = 3.39$, $p = .072$), although the latter contrast approached significance. This indicated that snake and spider trial responses did not differ within subjects (when averaged across groups) and that overall responding (averaged across snake and spider trials) did not differ between groups. As expected however, the interaction between group and trial-type was significant ($F(1, 46) = 6.47$, $p = .014$, $\eta^2 = .16$), indicating that low- and high-fear groups responded differently by trial-type. Two between-groups t-tests were used to conduct planned comparisons for each trial-type. There was a significant difference between

groups on spider trials: spider D-scores were greater in the high-fear group (*M* = 0.44, *SD* = 0.29) than the low-fear group (*M* = 0.13, *SD* = 0.27), *t*(46) = 3.80, *p* < .001, *d* = 1.10, $CI_{.95}$ = 0.48, 1.70. For snake trials, the difference in D-score between high- (*M* = 0.17, *SD* = 0.48) and low-fear (*M* = 0.23, *SD* = 0.29) was not significant (*p* = .54).
These findings indicate that the high-fear group produced a stronger IRAP effect for spider trials, relative to the low-fear group, in the expected direction (response bias towards spider fear). Only the low-fear group produced a significant IRAP fear-effect for snake stimuli.

Prediction of group membership. The IRAP data indicated that participants in the high-fear group produced significantly larger D-IRAP scores than those in the low-fear group for the spider trial-type. To determine the predictive validity of this D-score, a discriminant function analysis was carried out. The value of this function differed significantly between groups (X^2 (1, 46) = 12.44, *p* < .001), and the discriminant function successfully classified 70.8% of cases overall, with equivalent predictive accuracy (70.8%) for both low- and high-fear groups. This indicated a 29.2% 'false-negative' misclassification of the high-fear group (seven high-fear participants were predicted to be in the low-fear group) and a 29.2% 'false-positive' classification of the low-fear group (seven low-fear participants were predicted to be in the high-fear group).

IRAP-explicit correlation. Table 1 presents inter-correlations among study variables, including Pearson correlations between IRAP scores and explicit self-report measures. The correlation between FSQ and spider D-IRAP score was moderately positive and significant. Spider D-IRAP score also correlated significantly with explicit ratings of disgust, but not fear (*p* = .11), in response to spider images. Explicit measures of spider fear demonstrated correlation in the expected direction. Snake D-IRAP score was not associated with any of the spider measures.

Table 1

Descriptive statistics, reliabilities, and inter-correlations among variables (reliabilities in parentheses)

Measure	*M*	*SD*	1	2	3	4	5	6
1. D-Spider	0.28	0.32	(.48)	.47**	.18	.32*	-.11	-.39**
2. FSQ	36.3	31.3		(.97)	.62**	.55**	.08	-.40**
3. Spider fear	30.6	15.1			(.97)	.87**	-.01	-.32*
4. Spider disgust	28.1	16.0				(.95)	-.04	-.44**
5. D-Snake	0.20	0.39					(.61)	.16
6. PT-BAT steps	5.31	2.17						-

Note: Pearson product-moment correlations were used for all relationships tested. Expected relationships between spider measures were tested at the one-tailed level; relationships with D-Snake were tested at the two-tailed level.
For D-scores, reliability was estimated using the Spearman-Brown coefficient; Cronbach's alpha coefficients were used for other estimates of internal reliability.
FSQ = Fear of Spiders Questionnaire; D-Spider = IRAP effect for spider trials; D-Snake = IRAP effect for snake trials; PT-BAT = Perceived Threat – Behavioural Approach Test.
* p = .013, ** p < .001

Reliability of the IRAP. An odd-even split-half procedure (applying the Spearman-Brown formula) was used to assess the reliability of the IRAP (Barnes-Holmes et al., 2009). Split-half reliability was .48 and .61 for the spider and snake D-IRAP scores respectively. These values are modest but comparable to previously reported IRAP reliability (ranging from .34 to .60 for spider trials; Cochrane et al., submitted) and to reported reliability for alternative implicit measures – such as the GNAT (.46; Teachman, 2007) and the EAST (.56; Huijding & de Jong, 2005).

Changes from pre- to post-intervention

A 2 x 2 x 2 mixed ANOVA was conducted, with intervention condition[5] (exposure and defusion) as the between-participants variable, and trial-type (spider and snake) and time (pre- and post-intervention) as within-participants variables. Figure 2 shows the relevant D-scores for this model. There was a significant main effect of time ($F(1, 46) = 8.83$, $p = .005$, $\eta^2 = .16$), indicating that, averaging across trials and intervention conditions, D-scores increased from pre- ($M = 0.24$, $SD = 0.24$) to post-intervention ($M =$

[5] [See section 2.2.3.1 of the extended paper for manipulation checks relating to the intervention conditions]

0.35, *SD* = 0.25). The ANOVA yielded no other significant main or interaction effects (*p*s > .12)[6]. The absence of interaction effects suggested that the increase in D-scores did not differ by trial-type or intervention condition. The lack of spider-specificity, and parity of intervention conditions, may suggest that the increase reflected a general practice effect. Neither intervention – nor the spider-specific context of the experiment – was shown to affect changes in responding on the IRAP.

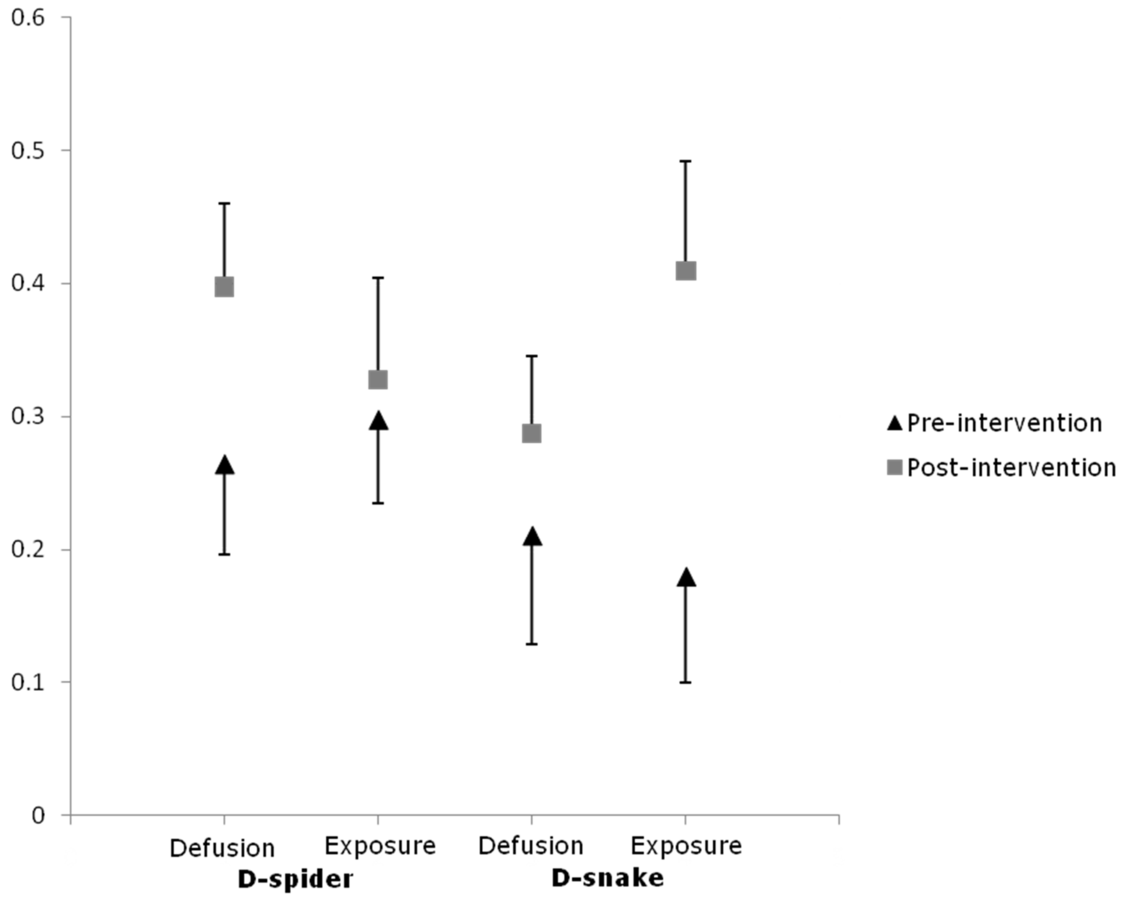

Figure 2. Pre- and post-intervention D-IRAP scores by intervention condition (defusion and exposure). Figure shows means and standard error bars.

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[6] An extended model, including group (low-fear; high-fear) as an additional between-participants factor, was carried out to test for possible interactions with level of pre-intervention spider-fear. The only additional significant effect was the interaction between group and trial-type, F(1, 44) = 6.33, *p* = .016 (all other *p*s > .107). Follow-up one-way ANOVAs confirmed that the previously observed intergroup difference on spider trials was also observable when averaging across time (pre- and post-intervention assessments), *F*(1, 46) = 9.14, *p* = .003. High-fear participants produced higher D-spider scores than low-fear participants, indicating relatively greater fear-consistent responding across pre- and post-intervention spider trials. The groups did not differ significantly on snake trials (*p* = .534).

Explicit measures. Parallel mixed 2 x 2 ANOVAs[7] were conducted for the explicit self-report measures, with separate models for FSQ score, spider fear ratings, and spider disgust ratings. Time (pre and post-intervention) was the within-participants variable and condition (exposure and defusion) was the between-participants variable in these models. There were no significant main or interaction effects in the model for FSQ score (all *p*s > .133). Explicit spider fear as measured by the FSQ did not demonstrate sensitivity to intervention conditions, showing no significant change between administrations.

In the model for spider fear ratings, there was a significant within-participants main effect ($F(1, 46) = 13.83$, $p = .001$, = .23). The main effect for intervention condition and interaction term did not reach significance (*p*s > .28). Spider fear ratings decreased from pre-intervention ($M = 30.65$, $SD = 15.12$) to post-intervention ($M = 27.27$, $SD = 16.31$), but this decrease did not differ significantly between conditions. A statistically equivalent decrease in fear ratings was shown between exposure and defusion conditions.

In the model for spider disgust ratings, there was a significant within-participants main effect ($F(1, 46) = 11.54$, $p = .001$, $\eta^2 = .20$) and a significant interaction with intervention condition ($F(1, 46) = 5.42$, $p = .024$, $\eta^2 = .11$). The main effect for intervention condition did not reach significance ($p = .43$). Spider disgust ratings decreased from pre-intervention ($M = 28.08$, $SD = 15.96$) to post-intervention ($M = 25.04$, $SD = 17.22$). This decrease was more pronounced in the exposure condition (mean change from 27.25 to 22.13) than the defusion condition (from 28.92 to 27.95), indicating that the exposure condition was relatively more effective in reducing disgust responses to spider images.

Test-retest reliability. Correlations revealed that participants' responses on explicit measures were consistent across the two administrations: FSQ, $r = .89$, $p < .001$; fear ratings, $r = .92$, $p < .001$; disgust ratings, $r = .93$, $p <.001$. The IRAP was relatively less stable across administrations ($r = .41$, $p = .004$), although reliability was comparable

[7] MANOVA/adjustment for multiple testing was not carried out. Tests were selected according to a priori interest in effects on a limited number of secondary outcomes (Cook & Farewell, 1996).

with previous findings for the IRAP (Cullen, et al., 2009) and for other implicit measures (Nosek, Greenwald, & Banaji, 2007).

Prediction of avoidance behaviour

A hierarchical regression analysis was carried out to determine whether implicit and explicit measures each explained unique variance in subsequent avoidant behaviour. This replicated the analytical approach applied by Cochrane et al (submitted for publication) and Teachman and Woody (2003). Intervention condition was additionally entered into the model to test the hypothesis that defusion (negatively) would predict avoidance over and above pre-intervention indicators of (implicit and explicit) spider fear. An equation was calculated with number of steps completed on the PT-BAT as the dependent variable (fewer steps indicated greater avoidance). The primary explicit measure, pre-intervention FSQ score, was entered as a predictor at Step 1; the implicit measure (pre-intervention spider D-score) and intervention condition (0 = exposure, 1 = defusion) were entered at Step 2. Table 2 presents the results of this regression analysis.

Table 2

Predicting avoidance behaviour

	B	*SE B*	β
Step 1			
Constant	6.31	0.45	
FSQ	-0.03	0.01	-.40**
Step 2			
Constant	6.01	0.53	
FSQ	-0.02	0.01	-.27*
D-Spider	-1.76	1.01	-.26*
Condition	0.94	0.56	.22*

Note: R^2 = .16 for Step 1, ΔR^2 = .10 for Step 2 (p = .028)

* $p < .05$, ** $p < .01$

As expected, FSQ score significantly predicted steps completed at Step 1. Variables at Step 2 added significantly to the equation: both D-spider and condition emerged as significant predictors of steps completed, and FSQ score remained significant. This suggested that implicit fear predicted additional avoidance behaviour beyond the FSQ. Furthermore, intervention condition was predictive of avoidance behaviour. Controlling for explicit and implicit fear, participants in the defusion condition were less avoidant (completed more spider-approach steps) than those in the exposure condition.

Post-intervention model. The non-significance of ANOVAs examining (within-participants) change from pre- to post-intervention and the (between participants) influence of condition indicated that post-intervention implicit and explicit measures would not mediate any effect of intervention condition – or supersede pre-intervention measures in predictive utility. Given this, and collinearity concerns, selection of predictors in the primary regression analysis (reported above) was limited to pre-intervention measures, plus the intervention condition. However, a parallel regression model, with post-intervention FSQ and D-spider scores as alternative predictors, was examined in a secondary analysis. Interestingly, the post-intervention D-spider score was not a significant predictor in this model (p = .46); in contrast, post-intervention FSQ score was significant (p = .001). The D-spider measure appeared to lose predictive utility at the second administration-point, potentially reflecting loss of reliability with retest and/or intervention effects.

Discussion

The current study replicated previous research in demonstrating relationships between implicit, explicit, and behavioural indices of spider-fear (Cochrane, et al., submitted for publication; Teachman & Woody, 2003). More specifically, an implicit (response latency) measure of spider fear was shown to be related to explicit (self-reported) spider fear – discriminating low- versus high-fear participants – but was also independently predictive of actual spider-approach behaviour (over and above the explicit measure). Thus, the implicit measure of spider-fear applied in the present study demonstrated both convergent and discriminant validity in relation to explicit spider-fear; crucially, implicit spider-fear showed predictive validity, accounting for unique variance in actual behaviour. The present study uniquely expanded on previous research by examining the effects of two treatment-analogue interventions on implicit, explicit, and behavioural indices of spider-fear. The intervention conditions applied in the present study appeared to differentially affect spider-approach behaviour, and seemed to influence some indices of explicit spider fear, but were not shown to affect implicit spider-fear.

Returning to the specific objectives of the current study, the research aimed to: (1) examine the effects of two interventions on implicit and explicit fear measures, (2) test whether implicit and explicit fear measures predict unique variance in relevant behaviour, and (3), combining the above, examine intervention effects on behaviour (directly and/or via fear measures). These objectives are considered in more detail below.

Objective one: Intervention effects on implicit and explicit fear

The first of these objectives was necessarily exploratory in nature, given the lack of previous evidence and conflicting theoretical arguments.

Implicit fear. Within the present study, neither exposure nor defusion interventions significantly modified implicit spider-fear. In both conditions, participants appeared to show a slight positive trend in their scores from pre- to post-intervention (suggesting a tendency towards inflated spider-fear, irrespective of intervention). In the absence of a control (no-intervention) condition, the equivalence between conditions is difficult to interpret. Possibilities include that: (1) neither intervention had any discernible effect; (2) practice effects reduced response biases (i.e., with IRAP repetition, participants get faster at responding in accordance with IRAP rules, even if these are inconsistent with their own beliefs), but both interventions increased implicit spider-fear (activating thoughts of fear and spiders), such that the net effect is neutral; or (3) both interventions decreased implicit spider-fear, but practice effects potentiated responding on bias-consistent blocks, producing a neutral net effect. It might be considered more likely that practice (repeat administrations of the IRAP) would reduce response biases, in line with the second possibility considered above. Also consistent with this possibility, it has been shown that implicit responding is highly sensitive to the context of administration (with implications for test-retest reliability; Ellwart, Becker, & Rinck, 2005; Ellwart, Rinck, & Becker, 2006): it may be that the study context activated and temporarily strengthened relations between spiders and fear (particularly at retest, when the spider-approach task was imminent).

However, Keogh (2008) reported IRAP effects showing more rapid responding on bias-consistent blocks following a placebo intervention, suggesting that the third possibility should not be ruled out. In the present study, snake stimuli served as parallel IRAP stimuli that were not directly targeted in either intervention (although both procedures, the defusion rationale in particular, could conceivably generalise to snake stimuli). Interestingly, overall implicit fear (averaged across snake and spider stimuli) increased significantly between pre- and post-intervention. Again, this may support the suggestion that repeat administration (within a single testing session) increased bias-consistent responding. This could reflect a *general* bias in practice effects and/or contextual activation of fear relations.

In the absence of a control condition, further interpretive discussion would be highly speculative. It does seem that second-administration implicit measures were less reliable (e.g., avoidant behaviour was predicted by pre-intervention D-spider score but not by the more temporally contiguous post-intervention D-spider score) and it may well be that repeat testing within such a short timeframe introduced test-retest artefacts that obfuscate intervention effects. Some researchers (Huijding & de Jong, 2007; Wiers, Van De Luitgaarden, Van Den Wildenberg, & Smulders, 2005) have suggested that implicit response-latency measures may be less suited to repeated measures designs, questioning their treatment sensitivity and raising concerns regarding their susceptibility to test-retest effects. The present findings for sensitivity to intervention effects most resemble those of Huijding and de Jong (2007) who found that a single-session exposure intervention reduced explicit ratings of spider threat but did not affect an implicit measure. Similar to the study by Huijding and de Jong, the present study used single-session interventions (one of which was exposure-based) and demonstrated effects on explicit (fear/disgust) but not implicit measures of spider fear.

Explicit fear. In terms of effects on explicit measures, the defusion and exposure conditions differentially affected spider disgust reports. Relative to participants in the defusion intervention, those in the exposure condition reported a greater decrease in disgust responses to spider stimuli[8]. Fear responses to spider stimuli were shown to decrease in both intervention conditions, with no statistical difference between conditions. Whilst the exposure condition involved repeat exposure to spider images/videos[9], the defusion exercise focussed on verbal relations between spiders and fear (but not disgust). The difference between conditions may reflect the specificity of the defusion exercise in comparison with a more general exposure effect. To some extent, both defusion and exposure conditions involved exposure to spider images (within the repeat administrations of both implicit and explicit measures). It could be that the decrease in fear ratings reflects this exposure/familiarisation (rather than an equal outcome from distinct exposure and defusion processes), but the differential effect for disgust suggests that the interventions were not wholly equivalent (and, if exposure is a common factor, the exposure condition at least provided more of it). Although a control condition could have aided interpretation, outcomes in a control condition would still reflect possible exposure processes (through repeat administration of implicit and explicit stimuli), as in the defusion condition. The apparent effect of the exposure analogue on both fear and disgust is consistent with studies of exposure effects in spider phobics (Choplin & Carter, 2010). Similarly, the more specific effect of the defusion analogue resembles the finding that defusion techniques can reduce the impact of targeted evaluative language (Blackledge, 2007). In the present instance, targeted disruption of verbal stimulus functions for 'fear' and 'spiders' may have decreased fear ratings of spider stimuli.

There was no overall difference in FSQ score from pre- to post-intervention and neither condition was shown to significantly affect this score. Although the FSQ has demonstrated treatment sensitivity in previous intervention studies (e.g., Muris, Mayer, & Merckelbach, 1998) this measure did not show change in the present study. It may be that the other explicit

[8] Again, although exposure and defusion conditions were shown to have distinct effects on explicit disgust reports, it is not clear how these effects would compare to repeat administrations without intervention. Exposure appeared to outperform defusion on this measure, but it cannot be assumed that exposure would outperform a no-intervention condition.

[9] [Analyses reported in the extended paper (2.2.3.1) do show reduction of both fear and disgust during the exposure condition, suggesting general desensitisation to these attributes.]

measures are more sensitive to brief interventions as they capture immediate responses to spider images[10]. Although most items of the FSQ are worded to suggest a state-dependent focus (using the terms 'currently' or 'now'), some statements appear to gauge more stable traits (e.g., 'Spiders are one of my worst fears') and temporal specificity is often unclear (e.g., 'Currently, I am sometimes on the lookout for spiders'). FSQ items likely require more abstract conditional thinking than the other explicit measures applied in the study: compare 'If I encountered a spider now, I would have images of it trying to get me' with 'How frightening is this [spider] image?'. The sensitivity of the FSQ to change following a brief intervention may further be compromised by items that are likely to be affected by the study context. For example, responses to the statement 'I now think a lot about spiders' are apt to be elevated immediately after interventions that focus on spider stimuli. Because the FSQ was completed immediately prior to the spider-approach task, some items may have seemed more salient and relatable than usual, and indeed than they had been when first administered (pre-intervention). Examples of such items include statements regarding being on the lookout for spiders, concern about being bitten, and expected nervousness if confronted by a spider. In the first published study to administer the PT-BAT, Cullen and colleagues (2008) also identified possible anticipatory effects on pre-task self-report (in terms of high state anxiety)[11]. Of course, the FSQ is a commonly-used measure and its reliability and validity have been supported across a number of studies. Demonstration of effects on this measure would arguably be more convincing – and relatable to the wider literature – than the observed effects on fear and disgust ratings. However, the fear and disgust responses showed good internal and test-retest reliability, and were significantly positively correlated with the FSQ (as a well-established and

[10] Greater sensitivity to brief interventions may be misleading if it means that the measures detect transient effects. Without long-term follow-up, it is difficult to demonstrate the potential clinical significance of changes. It may be that the FSQ is robust to temporary effects, making it a better indicator of more profound change.

[11] Within the single-session design, one alternative would have been to measure FSQ score after the PT-BAT. However, completion of the PT-BAT (which could be considered a form of exposure) would have contaminated subsequent reports. Future designs could be enhanced by carrying out procedures over a number of sessions, with the approach task taking place on a separate (later) occasion to the intervention and (first) administration of post-intervention self-report measures.

validated self-report measure). Furthermore, unreported analyses showed that all ratings of fear and disgust (pre- and post-intervention) were correlated with behaviour on the subsequent spider-approach task (with coefficients ranging from -.33 to -.42). These measures likely tap important variance in an underlying construct reflecting spider-aversion.

Future research may aid interpretation of intervention effects on explicit measures by carrying out follow-up assessments over time. This would help to establish whether present findings for FSQ insensitivity reflected the contiguity of assessment to the intervention and approach tasks (i.e., context effects). Such follow-up measurement would also be informative about the duration of observed effects on explicit fear and disgust responses, helping to establish whether brief exposure and defusion interventions have only temporary or more durable impact. It would be important to consider the timing of behavioural approach tasks in any longitudinal design, as performance of such tasks may additionally or multiplicatively influence subsequent self-report outcomes.

Objective two: Prediction of avoidance behaviour

With respect to the second objective, regression analysis showed that pre-intervention explicit fear (as measured by the FSQ) and implicit fear (D-spider) each predicted unique variance in avoidance behaviour, replicating the finding of Cochrane et al. (submitted for publication). The explicit and implicit measures were both correlated with spider-approach behaviour such that higher scores on either measure predicted fewer steps completed (i.e., greater avoidance). Crucially, although explicit and implicit spider-fear measures were correlated with each other, implicit spider-fear showed some incremental validity over explicit self-report for approach-task performance. This finding bolsters the suggestion that implicit cognition (as measured by response-latency paradigms) may be uniquely informative about clinically-relevant behaviour, and represent an important target for measurement and modification. Exploration of intervention effects in the present study indicated that the exposure and defusion treatment analogues did not modify implicit cognition (caveats around interpretability aside). However, the observed relationship between implicit cognition and behaviour suggests that the measurement and malleability of implicit cognition should be investigated further (Nock, et al., 2010; Wiers, Teachman, & De Houwer, 2007).

Objective three: Intervention effects on avoidance behaviour

The third objective was partly met within the regression analysis answering objective two. Over and above explicit and implicit fear, intervention condition influenced spider-approach behaviour. Individuals in the defusion condition completed more spider-approach steps (i.e., demonstrated less avoidance) relative to those in the exposure condition. This finding was consistent with preliminary research reported by Wagener and Zettle (in press), which found that participants in an acceptance-based intervention condition completed more steps on the PT-BAT than those in another (distraction) condition. Preceding ANOVA analyses had shown there to be no significant effect of intervention condition on repeated measures of D-spider or FSQ scores, suggesting that this effect of defusion was not mediated by implicit or explicit fear. Regression analyses further supported this: condition remained significant (predicted unique variance) in models incorporating both pre- and post-intervention measures of implicit and explicit fear. Thus, the present data suggests that defusion reduced avoidance behaviour more directly (perhaps through unmeasured variables reflecting deliteralisation and behavioural flexibility). This effect was in the predicted direction and appears consistent with conceptualisation of defusion: although defusion techniques may (secondarily) ameliorate fear by disrupting identification with relevant constructs, their primary theoretical function is to draw attention to the distinction between thought and action, thereby facilitating behavioural flexibility (Hayes, Strosahl, et al., 1999; Wilson & Murrell, 2004). Thus defusion should promote behaviour that is independent of fear cognitions, suggesting that one can have fearful thoughts and feelings yet act in spite of these experiences. In the present study, participants exposed to defusion exercises appeared able to complete more spider-approach steps than would be predicted by their explicit and implicit fear responses alone.

Implications for clinical malleability of implicit relations

The present study failed to support defusion and exposure interventions as techniques for modifying implicit spider fear. Returning to the original hypotheses, there was little empirical evidence to suggest that these interventions might alter implicit processes, but it was possible to identify a theoretical basis for predicting ameliorative effects (as described in the introduction). It may be that the brief interventions employed in this study did not capture important aspects of analogous clinical interventions; aspects that would modulate implicit cognitions in actual practice. However, effects (in the expected direction) on explicit self-report and actual behaviour give some indication of validity, suggesting that the intervention conditions likely captured aspects of the practices from which they were derived. Given evidence supporting the clinical significance of implicit responding, it would seem important to identify procedures that can modify implicit/automatic cognitions, and to improve understanding of the mechanisms by which existing treatments do or do not affect this construct (Nock, et al., 2010; Phillips, Hine, & Thorsteinsson, 2010). One implication of the present study might be that exposure and defusion treatments could be usefully supplemented by techniques that reliably modulate implicit processes. [See extended paper section 3.1. for elaborated discussion of clinical implications of the present study]

Limitations and contribution to knowledge

Consideration of study limitations has been partly integrated into previous discussion [See also extended discussion]: a number of issues are evident.

In terms of design and methodology, interpretability of study effects was limited by the omission of a control group and lack of follow-up assessment: it was not clear whether observed changes reflected test-retest artefacts, and the durability of changes could not be assessed. The moderate reliability of the implicit measure used in the present research, whilst comparable with reliabilities reported for other implicit measures, represents a limitation: the study aimed to test effects on this measure so it was important to maximise its precision. The IRAP did not seem to perform well as a repeated measure in the present study: future testing in suitably controlled studies would help to discern practice (versus treatment) effects.

The applicability of present findings to clinical practice was potentially limited by the use of a non-clinical sample. Further, the use of treatment-analogue interventions may fail to capture important aspects of corresponding real-world practices. However, the integration of applied and basic experimental research is consistent with the functional contextualist philosophy that underpins the present investigation (towards the scientific goal of prediction-and-influence).

The present study added to the evidence-base suggesting that implicit cognition may have discriminant and predictive validity for understanding clinically-relevant behaviour. The implications of this for investigating malleability of implicit cognition, particularly by existing treatments, seem clear. However, few studies have addressed this important question. The present study uniquely explored the effects of exposure and acceptance/defusion interventions on an implicit construct (and related explicit and behavioural indices). Findings were informative about the selective effects of these interventions, with implications for future research into how implicit cognition might be targeted. If implicit cognition is relatively insensitive to standard techniques, there may be a need to develop novel interventions that can affect implicit processes and thereby augment existing practices. This would seem to represent an important focus for further investigation. [Please see section 3 of the extended paper for extended reflective discussion on clinical and theoretical implications in addition to scientific and ethical issues]

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EXTENDED PAPER

Rationale for journal choice

The impact factor of *Behaviour Research and Therapy* (2.995) compares favourably with the median impact factor for clinical psychology journals (1.465). By this metric, *Behaviour Research and Therapy* is ranked 12th of 93 clinical psychology journals listed in ISI Web of Knowledge.

The scope of *Behaviour Research and Therapy* includes experimental analyses of behaviour change and processes of relevance to psychopathology. The journal has previously published experimental research testing analogues of clinical interventions with student volunteer samples, including a recent study testing an acceptance/defusion-based procedure (McMullen, et al., 2008b). Published articles also include a number investigating implicit cognition (e.g., Teachman, Woody, & Magee, 2006). The most cited article published by Behaviour Research and Therapy in the last 5 years pertains to Acceptance and Commitment Therapy (Hayes, et al., 2006; 221 citations).

Taken together the above information suggested that *Behaviour Research and Therapy* is a relatively highly-cited journal, with implications for effective dissemination, and that the present study might be considered suitable for consideration by its editors.

The journal does not impose a word limit on submissions (apart from 'shorter communications', which have an upper limit of 5000 words). Author guidelines are not included in the appendices (due to their length) but are available from:
http://www.elsevier.com/wps/find/journaldescription.cws_home/265/authorinstructions

1. Extended introduction

1.1. Relevance to clinical psychology

As indicated in the introductory paragraph of the journal paper, the construct of implicit cognition may have important implications for clinical practice. Empirical evidence is gathering to support the suggestion that investigation of implicit cognitions may bolster our understanding of processes central to psychopathology (Wiers, et al., 2007).

Measures of implicit cognition have been shown to distinguish clinical from non-clinical populations. For example, measures of implicit self-esteem and attractiveness-competence association differentiated individuals diagnosed with body dysmorphic disorder (BDD) from individuals with subclinical BDD symptoms and healthy control participants (Buhlmann, Teachman, Naumann, Fehlinger, & Rief, 2009). Implicit self-esteem has been shown to differentiate suicidal from non-suicidal individuals with depression (Franck, De Raedt, Dereu, & Van den Abbeele, 2007). McKay, Langdon and Coltheart (2007) found that implicit (but not explicit) self-esteem discriminated patients with persecutory delusions from healthy controls and patients with remitted persecutory delusions – in line with theoretical predictions (Bentall & Kaney, 1996).

Further, implicit cognition has been shown to be predictive of future clinically-relevant behaviour. For example, a recent study by Nock and colleagues (2010) found that implicit responding was prospectively predictive of attempted suicide, exceeding the predictive utility of known risk factors, self-reports, and clinical judgements. Implicit anxiety has been found to predict behavioural indicators of anxiety whilst delivering a stressful speech and to predict changes in observer-rated anxiety and performance decrements after failure – above and beyond explicit (questionnaire) measures of anxiety (Egloff & Schmukle, 2002). Haeffel et al. (2007) tested the role of implicit and explicit cognition in cognitive vulnerability to depression. In line with theories of implicit cognition driving immediate reactions and explicit cognition being more involved in long-term responding, implicit (but not explicit) cognition predicted acute affective reactions to a presented stressor whereas explicit cognition (interacting with life stress) predicted depressive symptoms over five weeks.

Implicit measures have been particularly useful in predicting risky health behaviours. This seems reasonable theoretically: such behaviours may be more impulsive (automatically activated) and subject to self-presentation concerns (i.e., even if relevant attitudes and behavioural intentions are available to introspection, individuals may choose not to report them). A recent study (Kahler, Daughters, Leventhal, Gwaltney, & Palfai, 2007) found that implicit smoking attitudes predicted abstinence during smoking cessation treatment over and above explicit measures. Wiers, Van Woerden, Smulders and de Jong (2002) found that implicit alcohol associations prospectively predicted alcohol-use over a one-month period (improving prediction from explicit measures alone). Stacy et al. (2000) found that implicit cognition independently predicted unprotected sex in a (judged) high HIV-risk community sample.

Implicit cognition has been shown to be sensitive to treatment interventions and predictive of outcomes. For example, a recent study (Gamer, Schmukle, Luka-Krausgrill, & Egloff, 2008) found that implicit anxiety was reduced (along with an explicit self-report indicator of social anxiety) following treatment for social anxiety (implicit anxiety had discriminated socially anxious from control participants prior to the intervention). Similarly, Grumm, Erbe, von Collani and Nestler (2008) found that implicit pain was sensitive to a 4-week course of cognitive-behavioural treatment in a patient group with chronic pain (pre-intervention implicit pain had differentiated the patient group from controls). In an important early examination of treatment-sensitivity that informs the proposed study, Teachman and Woody (2003) found that a course of cognitive-behavioural therapy reduced implicit spider fear in spider phobic participants; however, Huijding and de Jong (2007) failed to replicate this treatment-sensitivity finding.

1.2. Models of implicit cognition

How might evidence for dissociable implicit and explicit measures be interpreted? What does dissociation tell us about the nature of implicit (versus explicit) cognition – or whatever it is that causes the scores on implicit (versus explicit) measures? Three general interpretations have been put forward in the literature (Greenwald & Nosek, 2009); these may be labelled: (1) single representation, (2) dual representation, and (3) influences of person versus culture.

Single-representation advocates (e.g., Fazio & Olson, 2003) suggest that there is (only) one representation (of relations/associations for a target cognitive object), accounting for implicit/explicit contrasts in terms of discrete processes or levels of processing. According to this argument, it is a distinction between automatic and deliberative processing that accounts for implicit versus explicit constructs. More deliberative processing may allow for other (e.g., motivational) influences to affect processing outcomes (and influence behaviour). Nosek and Smyth (2007) observe that a single structure may be operated on by different processes to produce empirically distinct phenomena: for example, in physics, the single molecular structure of H_2O is operated on by different environmental processes (temperature/pressure) to produce distinct phases of ice, water, and steam. Although H_2O is represented by a single structure, its distinct (process-driven) phases may usefully be conceptualised as separate constructs.

Dual-representation views (e.g., T. D. Wilson, Lindsey, & Schooler, 2000) hold that implicit and explicit measures tap structurally distinct mental representations. In this view, a person may have two cognitive representations of the same object: one (explicit) that is expressed at a conscious level and one (implicit) that operates outside of awareness. Either structure may be activated: implicit structures are characterised as automatically activated (default) representations whereas explicit structures are seen to be activated only when a person has sufficient capacity/motivation to over-ride activation of the default structure. These separate structures may represent different relations (e.g., positive versus negative evaluations) for the same object and may be independently changed (e.g., a person may change their explicit attitude but retain their old implicit attitude towards the same object).

A third interpretation is that implicit and explicit measures capture different categories of influences: with implicit measures tapping cultural/environmental influences and explicit measures tapping within-person influences (i.e., an individual's evaluations; Karpinski & Hilton, 2001). This interpretation may be considered a variant of the dual-representation account, with distinct cultural and personal representations of an object.

Overall, although the underlying structure or mechanism of empirically dissociated object-evaluative cognition is not apparent (and may not be determinable by behavioural data), empirical patterns of discriminant and convergent validity seem to establish that two distinct theoretical constructs are necessary to capture the implicit-explicit cognitive domain (Greenwald & Nosek, 2009) – whether these are accounted for as different levels of processing or separable representational structures. On the basis of parsimony, a single-representational model with different levels of processing-depth may be favoured (but other representational models cannot presently be ruled out).

1.3. Prediction of behaviour from implicit measurement

A recent meta-analysis of 122 research reports (involving 184 independent samples and 14, 900 participants) found that a measure of implicit cognition (the Implicit Association Test) predicted behavioural, judgement, and physiological measures with an average *r* of .27 (Greenwald, et al., 2009). In (156) samples where parallel explicit (self-report) measures were used, explicit and implicit measures were found to have incremental predictive validity: each predicted unique variability in the criterion behaviour. Furthermore, in studies examining socially sensitive topics, the predictive validity of explicit measures was assuaged to a far greater extent than validity of the implicit measure (consistent with the argument that impression management may undermine the validity of self-report in certain contexts; Nosek, Greenwald, & Banaji, 2007). Findings of incremental validity and dissociable moderation (by social sensitivity) may be interpreted as evidence for separate implicit and explicit cognitive constructs.

A general finding has been that measures of implicit cognition are more predictive of spontaneous behaviours, consistent with the theoretical automaticity of implicit processes. When more time for deliberation is afforded, automatic tendencies may be moderated by other (less immediately salient) considerations (e.g., long-term consequences). Elaborated reasoning may facilitate inhibition of automatic tendencies. Measures of implicit cognition typically use response time to make inferences about the automaticity of processing: they are informative about immediate response propensities and such information is likely useful for understanding more reactive (versus planned) behaviour.

1.4. Validity of the IRAP as a measure of implicit cognition

Acceptance of the construct validity of an implicit measure is dependent on acceptance of the (theoretical) underlying construct of implicit cognition. It is difficult to demonstrate the reality of such underlying psychological constructs beyond the specifications of their definitions and proposed operationalisations for measurement (Sechrest, 2005). Some may not accept the underlying construct, making arguments for the validity of a specific instrument (as a measure of the construct) redundant. In the absence of hard evidence, the construct validity of a measure may only be supported by the gradual accumulation of information about the measure and the theoretical coherence of its relationships to an array of other measures and phenomena. It has been argued that the overall process of validating a psychological measure is inseparable from delineation and validation of its underlying construct. Theory and measurement are entwined such that attempts to measure constructs facilitate improved understanding and revision of constructs (Sechrest, 2005).

Irrespective of whether construct validity of a measure is accepted, the measure may have value in terms of criterion-related validity: it may be useful as an instrument that predicts variables of more fundamental theoretical or practical interest. The sub-sections below examine the IRAP in terms of validity indicators, drawing on research literature to date. Although a conventional multifaceted test-theory approach to validity (Messick, 1989) has been used in the present review, it has been argued that validity should be considered simply in terms of whether (1) a construct exists and (2) the construct causes scores on the test (Borsboom, Mellenbergh, & Van Heerden, 2004). Such a conception simplifies and focuses the question of validity, but loses information about the overall quality and implications of a test (now commonly included under the umbrella term of validity).

The following sub-sections examine the IRAP in terms of construct, criterion-related, content, and face validity. The section addressing construct validity is sub-divided into discussion of (1) convergent validity (subsuming correlational, contrasted groups, and experimental evidence) and (2) discriminant validity. The section addressing criterion-related validity is separated into (1) concurrent validity and (2) predictive validity.

1.4.1. Construct validity.

1.4.1.1. Convergent validity.

1.4.1.1.1. Correlational. High Inter-correlation of tests designed to measure the same construct (implicit cognition) would be indicative of validity.

IRAP and IAT measures of cultural preferences in the same sample were not found to be significantly correlated (Barnes-Holmes, Waldron, Barnes-Holmes, & Stewart, 2009). Given that stimuli were consistent between measures, and both measures purport to assess implicit cognition (on the basis of stimulus-response latencies), this may be taken as evidence against convergence of the IRAP with other implicit measures. It may be that differing features of the IAT and IRAP (e.g., relativistic versus absolute measurement; indirect-associative versus direct-relational responding) capture different aspects of the target construct. However, comparisons *across studies* show similar patterns of findings between implicit measures: the IRAP appears to operate like the IAT and other implicit measures (Barnes-Holmes, Barnes-Holmes, Power, Hayden, Milne, & Stewart, 2006). Correlations may be attenuated by the limited reliability of compared measures; limited (internal and test-retest) reliability has been a concern for all implicit measures to date (Nosek, Greenwald, & Banaji, 2007). The IAT and IRAP appear to compare well with other implicit measures such as the GNAT, EAST, and evaluative priming measures (which have been found to have split-half reliabilities as low as -.05; Nosek et al., 2007). More direct comparisons would bolster this suggestion.

The IRAP is a recently developed measure and more research is required to examine overlap with other implicit measures (for matched targets/stimuli). Evidence for convergent validity among other response-latency implicit measures is mixed. Bosson, Swann, and Pennebaker (2000) examined relationships among seven implicit measures (of self-esteem): of 21 possible zero-order correlations between these measures only two reached significance. Two of the most established measures – the IAT and evaluative priming – have failed to converge in a number of studies (Fazio & Olson, 2003). However, Cunningham, Preacher, and Banaji (2001) found that correcting for inter-item inconsistencies improved convergence between implicit (IAT and priming) measures, revealing a single latent factor (implicit construct).

In future assessment of IRAP convergence with other implicit measures, precision of correlational analyses may be enhanced by: (i) maximising reliability within measures; (ii) correcting for remaining measurement error (low reliability) using latent variable analysis (following Cunningham et al., 2001) to circumvent impact on inter-measure correlations; (iii) increasing the similarity of stimuli/task demands between measures (Olson & Fazio, 2003); and (iv) using large samples (Lane, Banaji, Nosek, & Greenwald, 2007).

Convergent validity may also be examined in terms of particular target cognitions (e.g., spider fear). Such examination might look at correlations between multiple measures of the specific target cognition (e.g., explicit and implicit measures of spider fear): the point being to establish target-specific convergence (e.g., does this measure tap a common spider fear construct?) rather than convergence supporting a general implicit cognition construct and its accessibility (operationalisation) by the IRAP. The present focus is on the notion of a general implicit cognition construct and the validity of the IRAP as a tool for measuring implicit cognition.

1.4.1.1.2. Contrasted groups. Another approach to measuring convergent validity is to examine differences in test scores between groups of people who would be expected to score differently on the test.

Barnes-Holmes et al (2009) found that IRAP effects distinguished known social groups (based on cultural preferences), outperforming the IAT with matched stimuli. The IRAP has also been found to distinguish between self-reported meat-eaters and vegetarians (based on food preference), matching IAT performance (Barnes-Holmes, Murtagh, Barnes-Holmes, & Stewart, 2010). Notably, the IRAP was more informative about the nature of contrasts: whereas the IAT (as a relativistic measure) could not distinguish pro-vegetable from anti-meat preferences, the IRAP assessed values for each target separately. Other studies have found that the IRAP distinguished between: prisoner and undergraduate groups on the basis of self-esteem (in accordance with known group differences; Vahey, Barnes-

Holmes, Barnes-Holmes, & Stewart, 2009); high- and low-spider fear groups (Cochrane, Barnes-Holmes, & Barnes-Holmes, submitted for publication); and heterosexual and non-heterosexual groups on the basis of homonegativity (Cullen & Barnes-Holmes, 2008).

In a study conducted by Dawson and colleagues (Dawson, Barnes-Holmes, Gresswell, Hart, & Gore, 2009), the IRAP distinguished between child sex-offender and non-offender groups (based on child-sexual classifications). However, sensitivity was moderate (68.8%) and specificity low (56.3%). Gray et al. found higher sensitivity (78%) and specificity (58%) in a comparable IAT study (Gray, Brown, MacCulloch, Smith, & Snowden, 2005). This study compared child sex offenders and other (sexual/violent) offenders, so discriminant findings may be considered more impressive: the control group may have matched the experimental group more closely than in the IRAP study (where a university-based control group was used). However, the IAT and IRAP studies used different stimuli, obfuscating comparison of contrasted-groups validity.

1.4.1.1.3. Experimental. Experimental construct validity is evident when manipulation of relevant variables produces theoretically consistent changes in the measures that should be influenced. For example, effects of a self-esteem intervention on an implicit measure of self-esteem may provide evidence of construct validity – especially if the intervention has specificity and does not simultaneously affect theoretically unrelated outcomes. Because less is known about influencing implicit versus explicit cognition – and changes in these constructs have been dissociated – interventions that have theoretically/empirically been shown to influence explicit outcomes may not affect implicit outcomes in the same way.

Cullen, Barnes-Holmes, Barnes-Holmes, and Stewart (2009) showed experimentally-manipulated malleability of IRAP effects (indexing ageist attitudes) between groups. A general anti-old IRAP bias was completely reversed in a group that was exposed to pro-old exemplars prior to testing. Effects were specific to the implicit measure (explicit attitude measures were unaffected – supporting discriminant validity, as discussed below).

1.4.1.2. Discriminant validity. Discriminant validity may be looked at in terms of non-correlation with theoretically distinct explicit (versus implicit) constructs. Evidence below suggests that the IRAP taps variance that is not captured by explicit measures: given the theoretical underpinnings of the IRAP, this may be interpreted more generally as supporting the validity of a distinct implicit construct. At minimum, it indicates that the IRAP is not a redundant measure when used alongside traditional questionnaire items relevant to a given target cognition.

Power, Barnes-Holmes, Barnes-Holmes, and Stewart (2009) reported discriminant implicit versus explicit preferences for nationalities. IRAP responses were found to diverge from explicit responses in a theoretically coherent way. In one experiment with a group of Irish participants, IRAP responses indicated a strong preference for Irish over Scottish and American over African whereas explicit measures indicated Irish equally likeable to Scottish and African and more likeable than American. Implicit preferences were consistent with predictions from in-group theories of perceived social similarity, whereas explicit preferences were considered to reflect 'socially desirable' (politically sensitive) responding.

Barnes-Holmes et al. (2006) reported distinct explicit (positive) versus implicit (negative) attitudes towards individuals with autism in professionals working with this population.

Discriminant validity may also be assessed in terms of dissociation between IRAPs with theoretically distinct target cognitions. Cochrane et al. (submitted) found IRAP effects for spider fear but not weapon fear in a high-spider fear sample; such a difference suggests that the IRAP has target specificity and does not simply pick up on a propensity to show IRAP (i.e., response-time bias) effects (Lane et al., 2007). Arguably, any convergence of spider and weapon fear could have been accounted for theoretically in terms of generalised threat-sensitivity; the study was not designed to demonstrate dissociation between IRAPs, but the results provide some preliminary evidence.

1.4.2. Criterion validity. Criterion validity refers to how strongly IRAP scores are related to other behaviours and constructs.

1.4.2.2. Concurrent validity. Here, concurrent validity is considered in terms of the relationship of the IRAP to established indicators of target cognitions/domains (examined at the same time). These will often be established explicit measures. Thus, a valid implicit measure should assess the same domain as an explicit measure whilst also demonstrating dissociation: referring back to considerations of convergent and discriminant validity, it is evident that implicit measures must demonstrate an unusual balance of shared and unique variability (Greenwald & Nosek, 2009).

A number of findings in the available literature support concurrent validity of the IRAP. Speed and flexibility of IRAP (relational) responding was positively related to general IQ (as theoretically predicted, O'Toole, Barnes-Holmes, Murphy, O'Connor, & Barnes-Holmes, 2009)[12]. Domain-relevant IRAP responses were found to correlate in the expected direction with (concurrently administered) established measures of spider fear (Cochrane et al., submitted) and self-esteem (Vahey et al., 2009). In a preliminary study (Barnes-Holmes et al., 2006), IRAP performance was found to be correlated with concurrent event-related-potentials (ERP) measures: belief-inconsistent trials produced a more negative ERP waveform than belief-consistent trials. Stimuli and response actions were equivalent across trials so differences may reflect automatic (well-established/high-probability) response processing versus low-probability response processing.

1.4.2.3. Predictive validity. Cochrane et al. (submitted for publication) showed that IRAP-assessed spider fear predicted subsequent spider approach behaviour (over and above explicit measures). The proposed study will provide a replication test of this effect and thus contribute to evidence around predictive validity of the IRAP.

1.4.3. Content validity. Content validity is a qualitative type of validity (although quantitative approaches have been proposed; Haynes, Richard, & Kubany, 1995) concerned with the extent to which an instrument measures the important aspects of the concept under assessment. Judgement of content validity is made (by an analyst) with reference to a theoretical definition of the concept to be assessed. Content validity could be judged for specific IRAPs in terms of the stimuli used (e.g., do IRAP-presented spider-fear stimuli adequately capture the concept of spider fear?). More generally, content validity of the IRAP can be examined in terms of the extent to which the IRAP possesses the functional properties of a measure of implicit cognition (De Houwer, 2006; Power et al., 2009).

[12] Note that this relationship was found with raw IRAP responses and can be controlled for by using the *D*-IRAP transformation (Vahey et al., 2009) as applied in the current study. In this way, the current study controls for possible effects of individual differences in cognitive ability on speed/flexibility of responding.

In the present discussion, construct validity pertains to inferences drawn from IRAP scores (thus extending beyond the IRAP itself, with implications for the construct of implicit cognition). Content validity is more limited in that it specifically looks at the appropriateness of the IRAP for measuring implicit cognition (as currently understood). Content validity may be informative about the quality of the IRAP as an instrument but not the implicit construct it was developed to measure (Sireci, 1998).

Drawing on available theoretical and empirical literature, De Houwer (2006) argued that a measure can be considered implicit if it meets one or more of the following criteria: (1) the participant is unaware of their cognition; (2) the participant is unaware that the outcome reflects their cognition; or (3) the participant has no control over the outcome. These criteria are considered below.

The IRAP is a relatively direct measure of cognition; the relations between presented stimuli are made clear: as a relational statement (e.g., I do not fear [the spider]). This means that, in contrast to disguised priming measures, and basic stimulus-pairing (associative) measures (such as the IAT), IRAP respondents are likely to be aware of the target cognition being assessed. That is, IRAP respondents will probably be aware of what the IRAP outcome is supposed to reflect. Their insight into the target cognition itself is more questionable: there may be processing of the target cognition that they are unaware (unconscious) of, and this processing may diverge from their conscious processing of the same target (e.g., attitude towards a particular racial group). Because the criterion of cognitive unawareness (criterion 1) is difficult to assess/demonstrate and the criterion of outcome naivety (criterion 2) is likely not met for the IRAP, the remaining criterion (criterion 3) may be considered a critical test of content validity (according to current understanding of this construct in the field of implicit cognition, as articulated by De Houwer, 2006).

McKenna, Barnes-Holmes, Barnes-Holmes, and Stewart (2007) studied the effects of instructing participants to ‘fake’ performance on the IRAP (having explained how the measure operates). Results showed no evidence of faking, suggesting that the outcome of the IRAP cannot be easily controlled. IRAP responses may be harder to control than IAT responses: a study by Kim (2003) found that participants could fake the IAT when given explicit instructions. It appears that the IRAP meets criterion 3 of implicit measurement, although further empirical inquiry is merited.

1.4.4. Face validity. Face validity is considered a less important aspect of validity, indicative of whether a measure looks like it will measure the thing it purports to.

Considered from the perspective of the participant, implicit measures may have little face validity – in fact, as discussed above, participant naivety to the purpose of measurement is one criterion for considering a measure to be 'implicit.' The IRAP is exceptional among implicit measures in the directness of its stimulus presentations, so the participant may be relatively clear about the cognitions/attitudes under examination (although they may not see how their responses will be measured).

From the perspective of experts in the field, the IRAP has face validity as an implicit measure. It resembles established implicit measures (such as the IAT) in its basic structure and response-latency-based scoring.

1.5. Rationale for use of spider-fear as a test construct

Spider fear was chosen as a test construct in the present study. Spider fear is a construct with clinical applicability (Teachman & Woody, 2003), but which appears to be best represented as dimensional rather than categorical (Olatunji, et al., 2009; Szymanski & O'Donohue, 1995), suggesting that research with non-clinical samples may inform (and generalise to) clinical populations. A number of studies have examined spider fear in relation to implicit cognition (since Teachman, et al., 2001), and this bolstered the rationale for selection of spider fear as a test construct: permitting interpretation in relation to existing literature.

The present research was not intended to investigate spider phobia *per se*. It was hoped that findings would be principally informative about the malleability of implicit responding and the functioning of the treatment-analogue interventions. However it should be acknowledged that spider fear is a common concern, with 20% of men and 30% of women reporting fear/anxiety when faced with a spider (Davey, 1994). Spider phobia represents the most common animal phobia, with a point-prevalence of 3.5%. Individuals with spider phobia report acquisition of spider fear through conditioning, parental instruction, or vicarious learning (Ost & Hugdahl, 1981); fear is likely to be maintained by negative reinforcement (i.e., reduction in distress from avoiding or escaping phobogenic stimuli) (Tryon, 2005).

1.6. Supplementary information regarding treatment analogues

1.6.1. Exposure. Years of research into systematic desensitisation supported the conclusion that mere exposure to an aversive cue (e.g., a real or imagined spider) can be sufficient to achieve desensitisation (Marks, 1975). Although exposure procedures are widely used in clinical practice for specific phobias (Vansteenwegen, Vervliet, Hermans, Thewissen, & Eelen, 2007a), and have demonstrable therapeutic efficacy (Götestam, 2002), the explanation of exposure effects remains an issue of debate (Tryon, 2005). Explanations in terms of habituation, extinction, and counter-conditioning have been formulated, but support for traditional conceptualisations of these mechanisms is limited (Tryon, 2005). More recent research suggests that the crucial effect of exposure may be in new learning (Moscovitch, Antony, & Swinson, 2009), particularly in terms of expectancy violation (Bouton, 2004): finding that the direct contingencies of a stimulus (e.g., spider) are not as anticipated (i.e., feared).

Of relevance to the present study, exposure is used within Acceptance and Commitment Therapy (ACT) (Orsillo, Roemer, Block Lerner, LeJeune, & Herbert, 2004): from an ACT perspective, defusion and exposure are complementary techniques (see 3.2.2 for further discussion). A limitation of traditional concepts of exposure may be that they fail to reflect the likely role of verbal contingencies (language and cognition) in learning and behaviour change (Tryon, 2005). Possible effects of exposure on verbal relations may be accounted for within ACT (and more specifically, relational frame theory), and this is discussed further in 3.2.2.2.

Although there is research to suggest that graduated exposure over multiple sessions is likely to be most effective (Butler, 1989), one-session treatments have been shown to be effective (Thorpe & Salkovskis, 1997). Furthermore, use of computerised exposure can have similar effects to standard in vivo exposure protocols (Marks, Shaw, & Parkin, 1998; Michaliszyn, Marchand, Bouchard, Martel, & Poirier-Bisson, 2010) – although findings are not consistent (Nelissen, Muris, & Merckelbach, 1995). These studies offered some support for the use of a computerised single-session exposure task in the present research (other supportive studies, which more directly influenced the specific procedure applied, are referred to in the journal paper).

1.6.2. Cognitive defusion. From an ACT perspective, experiential avoidance is a key target of clinical intervention (Hayes, Strosahl, & Wilson, 1999). Experiential avoidance has been defined as unwillingness to experience private events (thoughts, feelings, and bodily sensations), and efforts to modulate these experiences (and eliciting contexts) through control, prediction, or avoidance (Fledderus, Bohlmeijer, & Pieterse, 2010). Boulanger, Hayes, & Pistorello (2010) identify experiential avoidance across a range of psychological disorders and argue that it represents a transdiagnostic vulnerability factor (see also Fledderus et al., 2010). Theoretically, experiential avoidance is considered to derive from cognitive *fusion* (Orsillo et al., 2004): wherein verbally-labelled private events are taken to be literally true and come to have direct functions. Thus, the fused spider phobic may try to avoid even thinking the word *spider* because it may seem to bring all its negative connotations (feelings of fear, past memories, 'crawling' sensations, and related thoughts of personal vulnerability) into immediate experience. In a context of fusion and experiential avoidance, behaviour can often seem constrained because we are engulfed by our thoughts and feelings (and attempts to control them). Defusion creates a change in context: the thoughts and feelings that a person has do not change (and are not inherently positive or negative), but these private experiences have less of an impact in a defused context, facilitating flexible behaviour.

Thus, promotion of cognitive defusion – as a way of enabling valued actions when negative psychological content is present – is one of the overarching therapeutic principles in Acceptance and Commitment Therapy (Ruiz, 2010), equated with promotion of 'acceptance' or willingness (Orsillo et al., 2004) . The second overarching principle is about clarifying personal values and promoting 'commitment' to values-consistent behaviour.

Reviews of available empirical research suggest that ACT is efficacious for a wide range of psychological problems (Gaudiano, 2009; Ruiz, 2010). However, this evidence may be undermined by methodological weaknesses in trials conducted to date (Öst, 2008) and further research is needed to compare ACT with established treatments, such as cognitive-behavioural therapy (Powers, Zum Vörde Sive Vörding, & Emmelkamp, 2009).

The use of ACT in relation to spider phobia has only been reported in one previous study (Wagener & Zettle, in press), but this study found that participants in an acceptance-based condition were able to progress further on a spider approach task than participants in control or CBT conditions. Interestingly, participants in the acceptance condition did not report less distress than those in other conditions: they seemed able to progress further in spite of psychological discomfort. Similarly, three studies of pain tolerance have found that acceptance-based interventions can increase behavioural tolerance independently of effects on subjective discomfort (Gutiérrez, et al., 2004; Hayes, Bissett, et al., 1999; Takahashi, Muto, Tada, & Sugiyama, 2002). Such effects are consistent with ACT theory (Hayes et al., 2006), although effects on distress have been reported (Masuda, et al., 2010), and could also be accommodated theoretically (see discussion in 3.2.2).

The journal paper discusses the rationale for specific defusion techniques used in the present research (with reference to previous defusion/acceptance studies) and sets out evidence supporting initial study hypotheses. Detailed discussion of study findings for defusion effects – in relation to ACT/Relational Frame Theory postulates and previous research – is provided in 3.2)

1.7. Recruitment

Interested individuals were asked to contact the researcher by phone or email and the researcher provided further information about the study (including an electronic version of the information sheet for the potential participant to consider) and arranged a mutually convenient time to meet. On meeting, the potential participant was provided with a hard copy of the information sheet to read and a consent form; the researcher was available to answer any questions and obtain signed informed consent as appropriate. All participants who consented to take part in the study received £5 for their time/travel ('inconvenience allowance'); they were free to withdraw at any time after consent and still receive £5.

1.7.1. Inclusion/exclusion criteria. Prospective participants were considered eligible for inclusion in the study if they met the following criteria: (1) able to provide written, informed consent; (2) English as first language; and (3) 16 years of age or older.

Individuals without English as their first language were excluded due to the likely demands of procedural instructions on comprehension; materials were not translated into other languages due to resource limitations and difficulties ensuring cross-language comparability.

No specific criteria were set for inclusion/exclusion of participants on the basis of spider fear. Spider fear was treated as a continuous dimensional construct (Muris & Merckelbach, 1996), and the focus of interest was in terms of relative changes on this construct. The nature of (spider/snake) stimuli/tasks to be used in the experiment was explained to potential participants and they were informed that they could walk away from the study at any time (pre- or post-consent signature, reflecting the ongoing nature of informed consent).

1.7.2. Sample size. The primary objective of the proposed study was tested by a 2 x 2 ANOVA with time (pre-test vs. post-test) as a within-participants factor and intervention (exposure vs. defusion) as a between participants factor[13]. The primary outcome measure was implicit spider fear as measured by the IRAP. The information described below was used to find (in an a priori power analysis) that the proposed study needed to recruit at least 46 participants to have sufficient power (.90) to detect relevant differences.

G*Power 3.0 software (Faul, Erdfelder, Lang, & Buchner, 2007) was used to calculate sample size based on the following:

alpha=.05

m=2 (number of levels in repeated measures factor: 2 time-points)

f (effect size)=.43 [based on previously found effect size for change in implicit spider fear following intervention (Teachman & Woody, 2003)]

[13] For the purposes of analysis, this model was extended to include implicit snake fear as measured by the IRAP (i.e., trial-type – spider vs. snake – formed an additional within-participants factor). However, the component of this model testing objective one was as described here: time and intervention at the level of spider trials. Analysis of the spider-specific 2 x 2model (upon which power calculation was based) produced equivalent results.

rho (population correlations between different levels/times)=.49 [test-retest reliability from a previous IRAP study (Cullen, et al., 2009) was used to estimate this].

G*Power shows that a sample size of 46 should provide power >.90 (.911).

The sample size of 46 was considered realistically obtainable, representing a small fraction of eligible individuals across the two targeted university populations. It was anticipated that some prospective participants would not consent to participation after being informed of potential spider exposure; recruitment continued until a sufficient sample was obtained. Drop-out following consent had not been found in similar procedures (e.g., Cochrane, et al., submitted for publication) and was considered less likely in a single-session experiment than a study involving sessions spread over days.

Ideally, a no-intervention group would have been obtained to control for practice effects on repeated IRAP assessments, but inflation of required sample size could not be accommodated within available time/resources.

2. Extended results

This part of the extended paper details supplementary tests that were not provided in the journal paper. These tests support reported results by examining data integrity and checking assumptions underlying the analyses conducted. All of the data considerations and testing procedures described below were derived with reference to the following texts: Field (2009), Howell (2002), and Tabachnick and Fiddell (2001).

2.1. Preliminary data considerations

There was no missing data. Most tasks required participants to respond via computer input, with progression dependent on complete responding. This also limited the potential for errors in data entry. However, data integrity was further assessed by checking the compiled file against raw data outputs and testing for out-of-range values.

Box-plots were examined for all variables to check for outliers (which might unduly influence estimates in subsequent analyses). The only outliers[14] were for the variable of age (two outlier cases, participants aged 37 and 64). These cases were retained but robust descriptive statistics were reported for age (median and inter-quartile range). The only other analyses conducted with age were preliminary checks for relationship with variables of interest and for success of randomisation to conditions (reported below). Non-parametric tests were used in these analyses to limit the influence of outlying values.

2.1.1. Checking outcome of randomisation and influence of demographic variables. Analyses were carried out to test: (1) whether randomisation achieved balanced groups in terms of basic demographic factors (analyses in the journal paper demonstrated that randomisation successfully produced parity with regard to fear of spiders), and (2) whether demographic variables related to the variables of interest in planned analyses. No specific hypotheses had been considered with regard to demographic variables, but relationships with other variables of interest may suggest that they be included as covariates/control variables in subsequent analyses. Collection of personal information was minimised in

[14] Note that a *within-participants* transformation for extreme values is conducted in calculation of D-IRAP scores. This may reduce the likelihood of outlying cases for D-IRAP measures.

the present study and the only demographic variables available for analysis pertained to gender, age, and student/staff status.

There was no significant association between condition and gender (X^2 (1) = .40, *p* = .75), suggesting that randomisation produced equivalent groups with regard to gender. Pearson point-biserial correlations indicated that gender was not related to any of the other variables examined in analyses addressing study objectives (all *r*s < .17, *p*s > .26).

Non-parametric tests indicated that age was not related to any of the variables examined in analyses addressing study objectives[15] (Spearman Rho; all *r*s < .11, *p*s > .46) and that age did not differ significantly between conditions (Mann-Whitney *U* = 238, *z* = -1.04, *p* = .30).

The final demographic variable pertained to student status (0 = non-student, 1 = student), broadly differentiating student from staff members of the University population. Student status did not differ between conditions (Fisher's exact test[16], *p* = .42). Pearson point-biserial correlations indicated that gender was not related to any of the other variables examined in analyses addressing study objectives (all *r*s < .23, *p*s > .12).

Following these checks, it could be concluded that there were no systematic differences in gender, age, or student status between allocated conditions. This indicated that random allocation had achieved some success in producing balanced groups. Furthermore, the absence of relationships[17] between demographic variables and the main variables of interest supported the plan to exclude demographic variables from subsequent analyses.

2.2. Supplementary testing for reported analyses

The following sections are organised to parallel results subsections from the journal paper. Each section details assumption testing and

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[15] Variables examined in analyses addressing study objectives were pre- and post-intervention implicit (D-IRAP scores) and explicit responses (FSQ scores, Fear and Disgust ratings) and PT-BAT performance (number of steps completed).

[16] Chi-square was not used as cell sizes were inadequate (two cells had < 5 cases).

[17] Even without adjustment for multiple testing, none of the correlations involving demographic variables approached significance (i.e., the unadjusted .05 significance level). Adjustment for multiple testing (e.g., Bonferroni correction) would have reduced the chance of finding spurious relationships (i.e., Type I error) but at the potential cost of reducing power to detect significant relationships (i.e., Type II error). In the event, even with liberal (unadjusted) testing criteria, demographic variables did not appear to be related to the variables of interest.

supplementary observations pertaining to analyses reported in the corresponding subsection of the journal results. Assumption testing was carried out to examine the appropriateness, accuracy, and potential generalisability of analyses.

2.2.1. Preliminary analysis of explicit measures

2.2.1.1. Differences in fear ratings between conditions (exposure, defusion). Assumptions of this between-participants *t*-test, with FSQ score as the dependent variable and intervention condition (exposure, defusion) as the independent variable, were tested as follows:

- The DV (FSQ score) was measured at the interval level, satisfying the requirement for level of measurement.
- The distribution of scores appeared approximately normal (with slight tendency towards positive skew) on inspection of generated histogram and probability plots. Significance tests on *z*-scores for skew ($z = 1.64$) and kurtosis ($z = -0.89$) were non-significant ($ps > .05$), supporting the assumption of normality.
- The groups had approximately equal variance, as indicated by the non-significant Levene's Test, $F(1, 46) = 0.28$, $p = .60$. This suggested that the assumption of homogeneity was met.
- Independence of observations was assumed, as scores came from different participants.

Testing indicated that the assumptions of the *t*-test were met, suggesting that this parametric analysis was appropriate for the data observed.

The above-described procedure for assumption-testing was carried out for all succeeding between-participants t-tests. Aspects of the procedure were also replicated in testing assumptions for other parametric tests. Subsequently, specific values are only reported in cases where testing showed that an assumption was violated.

2.2.1.2. Differences in fear ratings between groups (low-fear, high-fear). Testing indicated that all assumptions of the *t*-test were met for this analysis (assumptions were tested used the procedure described above).

2.2.1.3. Differences in disgust ratings between groups (low-fear, high-fear). *T*esting indicated that all assumptions of the *t*-test were met for this analysis (assumptions were tested used the procedure described above).

2.2.2. Pre-intervention IRAP analyses. *One-sample t-tests* For each test, values were independent (taken from different participants) and interval-level. The sample distribution was tested as described previously (2.2.1.1), and the assumption of normality was supported for all four D-scores.

Mixed ANOVA The mixed ANOVA combined repeated measures and factorial ANOVA, requiring that the assumptions of both are satisfied. These are considered below:

- The repeated measures variables (D-spider and D-snake scores) were at the interval level and the between-participants factor (fear group) was suitable for defining groups (1 = low-fear, 2 = high-fear). Thus, requirements for levels of measurement were satisfied.
- The repeated measures variables were normally distributed at each level of the between-participants factor (i.e., in both low- and high-fear groups). This was checked (as in 2.2.1.1) by inspection of plots and significance testing of skew and kurtosis *z*-scores (all *p*s > .05).
- Each participant's responses were assumed to be independent of each other participant's responses (the repeated measures came from different people, randomly allocated to separate groups) .
- The groups had approximately equal variance for both repeated measures, as indicated by the non-significant Levene's Test (*p*s > .12). This suggested that the assumption of homogeneity was met for each cell of the design.
- Sphericity was assumed as there were no more than two levels of the repeated measures factor (D-spider and D-snake). The assumption of sphericity is met when the variances of differences are equal – and there is only one difference in a two-level factor (which is logically equal to itself).
- The assumption of homogeneity of inter-correlations was supported by a non-significant Box's M statistic (*p* = .07). The covariance between D-spider and D-snake scores appeared equal across groups. Box's M approached significance, however some have cautioned that this statistic is unstable (Tabachnick & Fidell, 2001) and argued that

it is better to disregard Box's M in cases where group sizes are roughly equal (such that robustness of test statistics can be assumed). All of the mixed ANOVAs examined in the present study had equal group sizes (i.e., 24 participants at each level of the between-participants factor).

In practice, ANOVA models are relatively robust against reasonable violations of assumptions (Howell, 2002) – at least when group sizes are roughly equal, as in the present study. However, the above-described procedure was applied for all mixed ANOVAs reported in the journal paper. Subsequently, specific values are only reported in cases where testing showed that an assumption was violated.

2.2.2.1. Prediction of group membership. The simple case of a univariate discriminant function analysis (with a single predictor variable and dichotomous dependent variable) is computationally similar to ANOVA, and the same assumptions apply. Most assumptions had been checked as part of assumption-testing for the preceding mixed ANOVA (reported above), specifically those pertaining to the between-participants part of the model. Supplementary checks confirmed normality, homogeneity, independence, and appropriate measurement.

An additional assumption of the discriminant function analysis is that dependent variable grouping represents a true dichotomy. In the present instance, a median-split was used to define groups: this practice threatens the assumption of a true dichotomy, and is often inadvisable due to potential loss of variability (Cohen, 1996). However, the applied group-split replicated Cochrane et al (submitted), facilitating cross-validating comparisons with this study, and further allowed for rough estimates of discriminant validity. Future investigation of meaningful cut-off scores for the Fear of Spiders Questionnaire would permit more acceptable groupings (Szymanski & Donohue, 1995).

2.2.2.2. IRAP-explicit correlation. All variables examined in Pearson correlations were interval-level and normally distributed, as appropriate. Normality was tested as described in 2.2.1.1.

2.2.3. Changes from pre- to post-intervention. The primary 2 x 2 x 2 Mixed ANOVA was tested against assumptions as described in 2.2.2. All assumptions were found to have been met.

Similarly, these assumptions were met for the parallel 2 x 2 ANOVAs examining changes in explicit measures.

2.2.3.1. Manipulation checks. Both intervention conditions (exposure and defusion) incorporated self-reports that permitted some indication of their effects.

2.2.3.1.1. Exposure. In the exposure condition, participants completed fear and disgust ratings of video stimuli immediately after each presentation. There were four video stimuli and each video stimulus was presented twice. From this data it was possible to gauge change from first to second presentation of stimuli. Significant reduction in fear and disgust would suggest that the intervention was having intended effects in terms of habituation/desensitisation.

Within-participants Wilcoxon tests were conducted for both fear and disgust ratings[18]. On average, participants reported less fear at second presentation (*Mdn* = 11.5) than they did at first presentation (*Mdn* = 13.5), z = -2.91, p = .002, r = -.42. Similarly, they reported less disgust at second presentation (*Mdn* = 11.0) than they did at first presentation (*Mdn* = 14.0), z = -2.21, p = .024, r = -.32. These results indicated that fear and disgust reduced over the course of the (video) exposure section, providing some evidence to suggest that the exposure intervention functioned as intended.

2.2.3.1.2. Defusion. In the defusion condition participants provided text responses describing the effects they experienced after a defusion exercise (the 'Milk' exercise, see Appendix C). This permitted a check that the reported experience of this exercise matched with the intended experience. Similarly, text input could be used to demonstrate successful engagement in another defusion exercise (a paradox exercise wherein the participant was instructed to type 'I cannot type' repeatedly).

Responses to the defusion exercise were consistent across the 24 participants and were congruent with expectations of the exercise (Masuda et al., 2009). Participants typically reported that the meaning of the word temporarily fell away and that formal properties (e.g., the sound of the word) became more prominent (in ACT conceptualisation, they seemed able

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[18] The sampling distribution of the differences between scores was non-normal, so the Wilcoxon test was used as a non-parametric alternative to the dependent *t*-test.

to experience the process of language-based thought rather than 'fusing' with its contents). Example responses are provided below:

- "The word seemed to lose meaning as I said it; it just became a repeated word rather than an image." [Case 9]
- "I forgot all the good things about milk; I just focused on how weird it sounds to be honest." [Case 15]
- "At the moment I cannot help but think of the word itself instead of the drink itself. It was indeed very strange to say it many times but I think any other word would give the same effect if I were to say it repeatedly as I did with the 'milk'." [Case 33]

Text data from the paradox exercise showed that all 24 participants acted as instructed (repeatedly typing 'I cannot type'), indicating experiential participation as intended.

2.2.4. Prediction of avoidance behaviour. To assess the accuracy and generalisability of the regression model, two sets of tests were carried out. Firstly, diagnostics were conducted to identify the possible influence of multivariate outliers and influential cases. Secondly, underlying assumptions were checked to determine whether population-based conclusions could be supported.

2.2.4.1. Diagnostic statistics. Diagnostic testing was carried out to identify cases that may be unduly influencing the regression model (Field, 2009):

- Less than 5% (4.2%) of cases had standardised residuals > 2 and none had absolute values > 2.5
- Values of Cook's distance were all < 1
- No leverage values were greater than twice the average leverage value
- Values of Mahalanobis distance were all < 15
- No values of DFBeta were greater than 1
- All cases had covariance ratio values within acceptable limits (0.75-1.25 for the present data)

From this testing it was possible to conclude that the model had adequate reliability for fitting the observed data and was not overly influenced by a small number of cases.

2.2.4.2. Testing of assumptions. Underlying assumptions were checked to establish potential generalisability of the regression model. These assumptions and relevant tests are considered below (Field, 2009):

2.2.4.2.1. Variable types. All predictor variables were quantitative (FSQ, D-spider) or dichotomous (condition; 0 = exposure, 1 = defusion). The dependent variable (PT-BAT steps completed) was quantitative, continuous, and unbounded (with participants scoring across the range of possible responses). Variables appeared appropriate for regression modelling.

2.2.4.2.2. Independence and non-zero variance. All values of the dependent variable (PT-BAT steps) came from separate participants, supporting the assumption of independent observations. All predictors demonstrated non-zero variation in value.

2.2.4.2.3. Multicollinearity. Table 3 shows inter-correlation of predictor variables. The highest correlation was in the 'moderate' range (.47), substantially below values suggesting problematic collinearity (.80 and above). Inspection of collinearity diagnostics showed that variance inflation factor (VIF) values were small (1.003, 1.284, 1.286) and within the suggested range of acceptability (i.e., <10), with an average VIF (1.191) that was close to 1. Reciprocal tolerance values were all above the recommended lower bound of .20. There did not appear to be problematic collinearity in the data.

Table 3

Inter-correlation of predictor variables

Measure	(1)	(2)
D-Spider	.47**	-.05
FSQ (1)	-	-.04
Condition (2)		-

Note: Pearson correlations were used for all relationships tested (point-biserial coefficients were used for relationships with Condition).

FSQ = Fear of Spiders Questionnaire; D-Spider = IRAP effect for spider trials.

** p < .001 (one-tailed)

As previously discussed, there were no outlier values for any of the variables submitted to regression modelling (the only outliers identified were for the uncorrelated variable of age). It could be concluded that the accuracy of the regression model was not compromised by outlying values on variables included in the equation.

2.2.4.2.4. Homoscedasticity and linearity. A scatter-plot of standardised residuals against standardised predicted values was generated. Inspection revealed a random array of points, evenly spread around the zero line. There was no apparent funnelling or curvature (indicative of heteroscedasticity or non-linearity respectively). It could be concluded that the assumptions of homoscedasticity and linearity were met. Partial plots supported similar conclusions with respect to each of the three predictor variables.

2.2.4.2.5. Normally distributed errors. Inspection of a histogram and normal probability plot of the residuals indicated that the residuals were roughly normal: fitting a bell-shaped curve (histogram) and showing little deviation from the line of normality (probability plot). This was supported by testing of skew and kurtosis for standardised residuals, using procedures previously reported.

2.2.4.2.6. Independent errors. The Durbin-Watson statistic for the regression model was 1.74. This was greater than the upper bound of the critical value (1.47) for 3 predictor variables and n = 48, indicating that there was no significant autocorrelation among residuals. It could be concluded that the assumption of independent errors was met.

Testing indicated that all the assumptions of regression were met. This increased confidence in the possible generalisability of findings beyond the present sample. Validation in other samples would bolster applicability to the population model.

3. Extended discussion

3.1. Clinical implications

The following sections will consider the clinical implications of the present study in terms of (1) the utility of implicit measurement and (2) connotations for clinical intervention.

3.1.1. Utility of implicit measurement.

3.1.1.1. Predictive utility. The finding that performance on an implicit fear measure predicted avoidance (beyond explicit measures alone) suggests that measurement of implicit processes could have some utility in measuring and predicting clinical behaviour. This finding contributes to a growing evidence base for the predictive validity of implicit measures in spider phobia (e.g., Ellwart, Rinck, & Becker, 2006; Teachman, 2007) and other clinically-relevant phenomena (Wiers et al., 2007). How might the incremental validity of implicit (response latency) over explicit (self-report) measurement be understood? Returning to the REC model discussed in the introduction (Barnes-Holmes et al., 2010), one possibility is that implicit measures capture a distinct level of processing that is relatively automatic and implicated in more spontaneous (unplanned) action. In support of this, Huijding and de Jong (2006) previously found that scores on the FSQ best predicted strategic avoidance behaviour whereas a response-latency measure of implicit spider-fear predicted automatic fear responses. This finding bolsters the suggestion that implicit measures have specific predictive utility for automatic fear responses, and may facilitate interpretation of results in the present study.

Transposing to the present study, it might be that implicit responding uniquely predicted performance on the spider-approach task that was more reactive, whereas explicit self-report captured deliberative behaviour. Participants were expecting the spider-approach task and had time to consider and plan their behaviour; they also had opportunity to deliberate between each step, before agreeing to continue. However, some participants were observed to spontaneously discontinue a step (showing unanticipated escape behaviour) whilst others reported progressing further than they had planned (appearing to spontaneously respond against expectations during the task). Future research may be able to examine this possibility more precisely by recording each participant’s expectation for progress on the behavioural task before they undertake it (and perhaps again after each step). It might be hypothesised that any discrepancy between expected and actual behaviour would be best predicted by performance on implicit measures.

Clinically, a possible implication of the present research (considered in the context of available theoretical and empirical literature) is that implicit measures may have utility in understanding susceptibility to unanticipated lapses, and broader situational reactivity, in treatment and follow-up. With respect to spider fear, implicit measures could be informative about how a phobic client may react in an unexpected spider encounter. Whilst treatment may be shown to positively affect self-report and promote planned approach behaviour (e.g., going into a cellar, expecting to see a spider), residual implicit fear could predict adverse reactions to *unanticipated* spider-exposure or intrusive thoughts (which might undermine previous treatment gains). The predictive function of implicit assessment could be used to guide the focus and duration of treatment. More generally, it is possible to see how the predictive function of implicit assessment could inform relapse prevention work in treatment of a variety of clinical problems (Stacy, Aimes, & Leigh, 2004).

3.1.1.2. Discriminative utility. In the present study, implicit performance differentiated self-reported low- and high-fear individuals. There is little basis to suggest that study participants would wilfully disguise the extent of their spider fear, and so the observed correlation between implicit and explicit measures of spider fear was as expected (replicating previous findings by Cochrane et al., submitted; Teachman, Gregg, & Woody, 2001). However, the apparent discriminative validity of implicit measures could be clinically useful in contexts where individuals may be unwilling or unable to provide an accurate self-report. For example, at outcome assessment, a patient may be inclined to over-report improvement following treatment for a clinical condition (e.g., spider phobia), or may be unaware of residual negative associations/relations (e.g., implicit spider fear). In such contexts, explicit self-reports may be misleading but responses on implicit tasks would likely remain informative. Previous studies have shown that implicit tasks such as the IRAP are difficult to fake (McKenna et al., 2007; Langner et al., 2010) and can reveal biases that may not be reported explicitly due to social desirability or other demand characteristics (e.g., Dawson et al., 2009). Even if such factors are not influential, and the participant believes that their explicit self-reports are completely accurate, assessment of residual implicit relations could bolster understanding of treatment outcome and projected progress. Given that implicit responses can predict behaviour (as discussed above) it may be important to demonstrate change in both self-reported and implicit responses.

3.1.1.3. Limitations of utility. Although implicit responses showed some discriminant and predictive validity, building on previous findings (Wiers et al., 2007), there is a need to improve measurement of implicit processes and their effects before it can be argued that relevant measures be applied in clinical practice. In the present study, the implicit measure incorrectly classified 29.2% of participants, suggesting that discriminant validity could be substantially improved. Further, although statistically significant, the predictive relationship between implicit spider-fear and avoidance was of small effect-size[19] (incrementally explaining just 5% of the variance in avoidance beyond self-reported fear). A significant proportion of variability in avoidance behaviour could be understood without reference to implicit responses (through self-report alone). The argument for clinical application would be strengthened if it was possible to show that implicit processes predict behaviours that are not understandable through explicit self-reports (Ellwart et al., 2006). Promisingly, recent research by Nock et al (2010) indicates that implicit measures may be predictive of suicidal behaviour, which has proved difficult to estimate from self-report or other indicators.

The possible unsuitability of implicit assessments for repeated measurement (Huijding & De Jong, 2009) is a measurement concern that potentially limits their practical usability: in the present study, implicit measures showed only moderate test-retest reliability (.49) and did not demonstrate sensitivity to intervention conditions. However, some studies have shown expected treatment sensitivity, particularly over longer-term interventions with greater separation between repeat test administrations

[19] The effect size (f^2) was 0.07; which was between the conventional small (0.02) and moderate (0.15) effect sizes for f^2 (Cohen, 1988)

(Teachman & Woody, 2003). Work is ongoing to refine the measurement of implicit processes (Krause et al., 2010). Beyond the question of measurement, more fundamental questions remain about the meaning of the processes tapped by implicit measurement techniques. Responses on implicit measurement tools may have some pragmatic validity (e.g., in their apparent relationship to clinically-important behaviour), but, in Lewin's phrase, "there is nothing so practical as a good theory" (Lewin, 1951, p. 159). Better theoretical understanding of the implicit construct would likely improve specification of measurement approaches and point to effective ways of modifying implicit processes. The present study failed to demonstrate malleability of implicit processes, but this remains an important focus for investigation, with manifest clinical implications.

3.1.2. Implications for intervention techniques. The present study applied two brief treatment-analogue interventions in order to examine the effects of these techniques on explicit, implicit, and behavioural indices of spider-fear (as a test construct). To the extent that these analogue interventions capture processes in the clinical treatments from which they are derived, results have implications for understanding the mechanisms and likely effectiveness of these clinical treatments. Findings suggested that both exposure and defusion techniques could reduce self-reported spider fear, and that exposure in particular could additionally reduce self-reported spider disgust. The defusion task specifically involved verbal relations with fear (rather than disgust), and it may be that defusion tasks including disgust-specific terms would have produced outcome-parity with exposure. However, it seemed that mere exposure to visual spider-stimuli (without specific verbal terms) reduced both fear and disgust. It was notable that these changes occurred in the absence of discernible effects on implicit spider-fear responses, suggesting that treatments may have independent effects on implicit and explicit fear.

An interpretation of present findings may be that the techniques examined in the present research do not affect implicit cognition. This might imply that the techniques examined here could be augmented in practice by interventions that do seem to influence implicit cognition. Some of the more consistent evidence for controlled malleability of implicit cognition is around the use of evaluative conditioning techniques (Olson & Fazio, 2006;

Hofmann et al., 2010). The rationale for these techniques (De Houwer, 2009) suggest that evaluative implicit representations (e.g., spiders as negative) can be modified by repeatedly pairing a target stimulus (e.g., spiders) with a second stimulus (e.g., positive images or words). These techniques have shown similar effects for self-report (Houben, Schoenmakers, & Wiers, 2010). The fact that evaluative conditioning effects have been shown for implicit responding is in itself more supportive of a relational versus purely associative account of implicit cognition (Hofmann et al., 2010) – and this is discussed further in the section considering theoretical implications (3.2.1.1). Another technique that has been shown to be effective involves evaluative information: simple statements or narratives that reframe a target stimulus (e.g., describing positive attributes of spiders) can produce change in implicit responding (Gregg, Seibt, & Banaji, 2006). It seems that both conditioning procedures and evaluative communication can modify implicit cognitions, with potential for clinical applications. Further research is required to demonstrate use and effectiveness in clinical practice.

3.1.3. Limitations for clinical relevance. The clinical implications of the present study are limited by the use of a non-clinical sample. However, analyses incorporating level of spider fear (low-fear and high-fear) did not suggest that, for example, there was any difference in treatment sensitivity at different levels of fear. Participants in the present study reported relatively high spider-fear in comparison to similar university volunteer samples. For example, the means for low- and high-fear groups in the study by Cochrane and colleagues (submitted) were 3.6 and 45.5 respectively; equivalent groups in the present study had means of 10.6 and 65.4. Thus, although not selected on the basis of spider fear, participants showed a range of spider fear and included individuals with relatively high (subclinical) levels of fear[20]. Working with non-clinical participants was arguably more appropriate given the exploratory nature of study objectives

[20] It might have been expected that individuals with more pronounced spider fear would not volunteer for the study (and certainly, a number of individuals did not follow up their interest in participation after receiving study information). Although it is possible that some phobic individuals participate in spider-fear studies expecting therapeutic gains, information around the present study stressed that the study was experimental and that procedures may cue distress.

pertaining to the malleability of implicit cognition and the experimental use of treatment-analogue interventions. However, it is acknowledged that the external validity of results obtained with university population samples is questionable (Sears, 1986), and that work with relevant clinical populations will ultimately provide the most compelling and practical answers to the research questions that prompted the present study.

Although chosen to capture techniques used in clinical practice, treatment-analogue interventions in the present study may have limited clinical relevance. Investigation of techniques in isolation can be informative about the specific mechanisms by which therapeutic changes occur. However, such investigations fail to capture the multifarious factors that may influence (and even override) effects of specific techniques in practice (Wampold, 1997; Oltmanns & Klonsky, 2007). Influential factors may include unknown interaction effects with other component procedures of the broader therapy. For example, in ACT, the effects of exposure and defusion may vary according to the effects of values work. Common therapeutic factors – such as placebo effects, therapeutic alliance, and therapist competence – may have a strong influence on the translation of techniques into practice (Messer & Wampold, 2002). Indeed, a meta-analysis by Ahn and Wampold (2001) found that psychotherapeutic interventions without the presumed active component technique were at least as effective as those including the technique. The debate around the importance of specific techniques versus common factors is beyond the scope of the present paper (the interested reader is referred to Lundh, 2009). However, this literature raises questions as to the applicability of experimental component research (such as the present study) to clinical practice. Putting aside the general question of whether experimental component research can have implications for clinical practice, it cannot be assumed that conditions in the present study adequately captured the techniques that they were designed to analogise. These procedures were developed on the basis of previous research, as described in the study methods, and the results of the study indicated that they behaved (broadly) in the manner expected. However, the brief computerised analogues used in the present study may not be adequately representative of actual clinical procedures. For example, varied as exposure protocols might be (e.g., Mohlman & Zinbarg, 2001), they tend to occur over longer time periods (even in one-session treatments; Öst, Brandberg, & Alm, 1997), and to be calibrated to the individual – although generic computerised formats have been used for specific phobias (Bornas et al., 2006). Potential clinical relevance may have been improved by more precisely replicating manualised treatment protocols (although issues of working with a non-clinical sample, in a non-therapeutic context, would remain).

3.2. Theoretical implications

The following sections will consider implications of the present study for (1) theoretical models of implicit evaluative cognition and (2) theory underpinning Acceptance and Commitment Therapy

3.2.1. Models of implicit cognition. Considering the findings of the present study in relation to available models of implicit cognition, there is support for dissociable implicit and explicit constructs (Greenwald & Nosek, 2009). Implicit and explicit measures in the present study were correlated but had incremental predictive validity and were differentially affected by intervention. However, present findings do not provide a basis for distinguishing available models. The theoretical constructs of explicit and implicit object-evaluative cognition could be explained in terms of different categories of influence, dual representations, or a single representation[21] – it may be difficult to empirically discriminate these accounts (Greenwald & Nosek, 2009). Given this, we made a case for adopting a single-process model of interpretation, on the basis that this is a more parsimonious explanation of available data (requiring fewer assumptions). We introduced the REC model as a single-process model[22] developed from an ACT/RFT perspective, with specific relevance to the use of a relational implicit measure (the IRAP) and ACT-based intervention (defusion) in the present study. The following section considers study findings in terms of this model.

***3.2.1.1. Relational Elaboration and Coherence (REC) Model*.** Interpreted in terms of the REC model, the correlation between implicit and explicit measures of spider fear indicates that immediate relational responses (e.g., I fear-Spider-True) cohered with extended relational

[21] And within the single-representation account, constructs may be accounted for by discrete processes or different levels of a single process (Fazio & Olson, 2003; Hughes et al., 2010).
[22] As it pertains to implicit cognition.

networks. Less formally, spontaneous responses were consistent with more carefully considered responses. Given time to consider their response to spiders, some participants may have drawn on more elaborated thoughts/verbal relations that contradicted their immediate relational response (e.g., 'I fear spiders, but I know that most spiders are not dangerous to humans, and I've actually never been hurt by one'). However, in general, participants in the present study showed convergence on implicit and explicit measures which indicated that their more elaborated networks supported their immediate relational responses. It is possible to see how the divergence of implicit and explicit responses towards more socially-sensitive stimuli could be accounted for by the REC model (Barnes-Holmes et al., 2010). An immediate evaluation, reflecting more historically and/or contextually salient relations (e.g., 'Muslims are dangerous'), may not find expression in deliberative self-report, because deliberation enables access to other relational networks (e.g., 'It is wrong to discriminate on the basis of religion'). The unique predictive validity of implicit and explicit measures may be interpreted in terms of brief relational responses predicting more reactive behaviour in the approach task (e.g., to an intrusive thought or sensation), whereas explicit responses predicted more elaborated and relationally coherent (i.e., deliberative) behaviour.

Finally, in terms of sensitivity to intervention, the REC model suggests that implicit (immediate) relational responses are likely to be more sensitive to current contextual variables. Whereas brief responding did not show sensitivity to spider-specific interventions in the expected way, there may have been a general context effect on brief fear responding (the IRAP fear effect increased across conditions). Design issues partly obfuscate interpretation, but it is possible that the general context of taking part in a fear experiment (with an imminent approach task) could have influenced immediate relational responses over and above more targeted effects of the intervention contexts. The REC model suggests that explicit responses are likely to reflect more coherent relational responding, such that current contextual variables may have a lesser impact (unless they cohere with other relational networks, which reflect the individual's verbal and non-verbal history). In the present study, some explicit measures were sensitive to intervention (fear and disgust responses) but the FSQ was not.

Reconsidering the response demands of these measures, the fear and disgust measures were relatively specific (eliciting responses to particular spider images) in contrast to the FSQ (see discussion in journal paper). According to the REC model the three measures of spider responses in the present study likely lie along a continuum in the extent of elaboration that they prompt. The IRAP permits only brief relational responding, the FSQ demands extended relational responding, but the fear/disgust ratings lie between these other measures (they are not time pressured but do not require extensively elaborated relational responding). What follows is a speculative interpretation of the present pattern of intervention effects in terms of the REC model: (1) IRAP responding may have been overly sensitive to general context; (2) FSQ responses may have been insensitive to brief contextual interventions; whereas (3) fear and disgust responses perhaps reflected the level of network elaboration that was sensitive to intervention.

The fact that the present study demonstrated effects from a relational procedure that are consistent with findings for associative implicit procedures has potential theoretical implications. Dominant accounts of implicit cognition (Gawronski & Bodenhausen, 2006) assume that implicit evaluations reflect the strength of linkage between two concepts in memory (associations), whereas explicit evaluations reflect propositional validation processes. The dominance of this account reflects and perpetuates the common use of purely *associative* procedures (such as the IAT) in implicit measurement (Hughes et al., 2010). The associative assumption suggests that only associative measures would show typical 'implicit effects' (Gawronski & Sritharan, 2010). However, the present study found that a time-pressured relational (i.e., propositional) response task (the IRAP) produced the same implicit effect as an associative response task (e.g., the IAT; Teachman, Gregg, & Woody, 2001). In effect, participants gave an immediate propositional response (essentially 'it is true/false that I fear spiders') that operated in the same way as an immediate associative response ('fear-spiders')[23]. The present study adds to a growing evidence

[23] Relational responding may also be more informative about the nature of stimulus relations than an association, which only indicates that stimuli are closely linked in memory. For example, IAT responses show that fear and spiders are generally

base for IRAP effects (see 1.4). Although it may be possible to develop an explanation of the IRAP effect in associative terms (Hughes et al., 2010), these findings suggest that the associative assumption may have limitations, and that a relational account of implicit cognition could facilitate some improvements in understanding.

Although results can be explained in terms of the REC model, the study was not designed as a test of this model. Further research is required to systematically investigate the REC model (Hughes et al., 2010).

3.2.2. Implications for the model of Acceptance and Commitment Therapy. The apparent effect of defusion on behaviour in the absence of unique effects on implicit or explicit relational responses was consistent with ACT postulates (Hayes et al., 2006; Wilson & Murrell, 2004). Within the ACT framework, defusion is not assumed to reduce discomfort directly (this may well occur, but is seen as secondary and not necessary). In a defused state a person may notice that they feel frightened or disgusted by a spider (and could self-report this). However, they are able to disentangle the *self* from these feelings, and can choose to approach the spider in spite of inhibitory feelings (if such an action is valued). The awareness that one is not entirely bound by feelings or thoughts may well diminish the impact of these private events. Further, the act of approaching feared contexts may secondarily function as a form of desensitising exposure. Thus, experiences of fear and distress might diminish as an indirect (and perhaps temporally distal) consequence of defusion, but these experiences are not primary targets.

In the present study, based on the ACT assumption that defusion serves to change the context of verbal relations (increasing distance between the self and any negative thoughts/feelings[24] towards spiders), it

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correlated (have been commonly paired in past learning experiences), whereas IRAP responses indicated whether spider fear was experienced as personally true/false. The informativeness of IRAP responses could be further extended by manipulating the relational terms (e.g., similar, opposite; more, less) and other presentation stimuli.

[24] From an ACT perspective, the verbal interpretation of feelings is the problem here (Hayes et al., 1999). Theoretically, when in a state of cognitive fusion, labelling an inner experience as 'fear' brings all the negative events (connotations) of this word into immediate experience. The labelled experience of fear may thus be accompanied by verbal arguments that inhibit direct experience and approach

was tentatively hypothesised that defusion might weaken fear-consistent responses to both IRAP and self-report items. That is, although the principal function of defusion is to break the control of overt behaviour by internal private events (Hayes et al., 1999), it was considered that the effects of defusion might be detectable in post-intervention relational responses. Previous research has been inconsistent with respect to this hypothesis. Masuda and colleagues (2004) found that use of a defusion technique reduced self-reported discomfort in the case of self-evaluative negative thoughts; Bassett and Blackledge (2006) found similar effects (reduction of distress associated with negative self-evaluations). However, the only other study to have examined an ACT intervention in the context of spider fear resembled the present study in finding that an acceptance/defusion condition reduced avoidance behaviour without affecting self-reported distress (Wagener & Zettle, in press). A similar pattern of results, behavioural change independent of self-reported distress, has been found in three studies of pain tolerance (Gutiérrez et al., 2004; Hayes et al., 1999; Takahashi et al., 2002). The differences between empirical studies may be accounted for by their different foci: those involving overt behavioural challenges (pain tolerance or spider avoidance) did not show effects on distress whereas those involving verbal challenges (negative self-evaluations) did. If defusion is thought to partly involve a distancing perspective-shift (from *I-Here-Now* to *I-There-Then*; Barnes-Holmes, Hayes, Dymond, & O'Hora, 2001), this shift may have been easier to accomplish when faced with a temporally non-specific verbal statement (e.g., "I'm a bad person"; Bassett & Blackledge, 2006) than when confronted with a behavioural task[25]. Irrespective of available empirical literature, it is important to stress that the present finding was not incongruent with ACT assumptions. Although we predicted effects on implicit/explicit measures, the ACT model accommodates a range of possible outcomes. Indeed, from an ACT perspective, it may have been hypothesised that defusion would increase fear responses (Keogh, 2008),

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behaviour (simply, fear becomes a literal fact and causal reason for avoiding action).

[25] Although, within ACT, both types of stimulus (verbal statement and approach task) are contexts for behaviour, it is argued that the here-and-now experiential relatability of these stimuli may differ, with implications for defusion effects.

because defusion is thought to promote acceptance of psychological content and willingness to make experiential contact with this. Although there is evidence for practical utility of defusion, and it builds on principles from an underlying theory of human language and cognition (RFT), understanding of how this intervention works in therapeutic practice is limited (Blackledge, 2007).

It should be acknowledged that the intervention conditions compared in the present study would likely be combined and complementary in an ACT treatment of spider phobia or other anxiety disorders (Orsillo et al., 2004). Defusion is considered to be a useful precursor to exposure therapy, facilitating willingness to approach feared stimuli in spite of inhibitory verbal experiences of thoughts and feelings. Indeed, there is evidence to suggest that exposure therapy can be undermined by experiential (cognitive) avoidance (Powers, Smits, & Telch, 2004; cf. Rachman, Radomsky, & Shafran, 2008). Defusion would theoretically augment subsequent exposure by facilitating experiential contact with the exposed stimuli (allowing individuals to simply notice stimulus-cued negative thoughts and feelings). Thus, although the present study looked at defusion and exposure interventions in isolation, future research might usefully examine the additive effects of these techniques, with applicability to ACT practice. As discussed in the section pertaining to clinical implications, the study of component interventions is useful (and encouraged within the scientific philosophy of functional contextualism; Gifford & Hayes, 1999), but may discount a number of important variables that would operate in practice[26]. With respect to the ACT model, values work is considered to be a central process that provides a rationale for other interventions (Wilson & Murrell, 2004): engagement in therapeutic tasks, such as defusion and behavioural exposure, is justified as a means to valued living. The present study did not directly appeal to participant values in presenting interventions, potentially limiting extrapolation to (values-driven) therapeutic work, although it does contribute to understanding of the defusion process itself – with potential generalisability beyond the ACT model (Blackledge, 2007).

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[26] This could present a potential difficulty with understanding any applied theory or ‘model’ of therapy.

3.2.2.1. Relational Frame Theory (RFT). By extension, implications for ACT have implications for Relation Frame Theory (RFT). These basic (RFT) and applied (ACT) theories are considered mutually supportive within a functional contextualist approach to prediction-and-influence (Hayes et al., 1999; see section on philosophy of science). Both the IRAP response task and defusion intervention follow from an RFT-based understanding of language and cognition. As discussed in the section on the REC model, the finding that the relational IRAP can perform in a similar way to associative measures of implicit cognition may challenge the associative assumption underlying most implicit research to date (Hughes et al., 2010). The REC model may usefully extend RFT to account for present findings (and other reported implicit effects), just as ACT extends RFT to applied behaviour analysis and clinical practice. However, it is noteworthy that, although a potential strength of ACT is its foundation in a programme of basic RFT research, understanding of the nature and effects of defusion (and related ACT procedures) remains imprecise (Blackledge, 2007; Masuda et al., 2009; Keogh, 2008). More co-ordinated programmes of basic and applied research may be required to realise the promise of theoretical integration.

3.2.2.2. A note on exposure as it relates to RFT. RFT provides a behavioural account of language and cognition, but additionally holds that direct operant or classical conditioning can establish and maintain behaviour (Anderson, Hawkins, & Scotti, 1997). Thus, theoretical and empirical support for exposure intervention is accommodated within RFT (Hayes et al., 2006). However, RFT further offers an explanation for the effectiveness of exposure in terms of verbally mediated experience. This is important as an RFT account suggests that verbal networks may dominate behaviour over and above direct environmental contingencies (Hayes et al., 1999). Exposure theoretically counteracts this by bringing individuals into contact with the stimuli that their verbal networks suggest would be intolerable (e.g., 'I can't stand to be near spiders'). Exposure circumvents verbal controls, allowing direct stimulus functions to operate. Individuals may find that the 'reality' of being near a spider (direct stimulus function) is not as awful as their verbal networks suggested, thereby loosening aversive verbal relations[27]. Exposure and defusion are both theorised to undermine verbal dominance because they are experiential exercises that do not merely engage with verbal content (from an RFT perspective, verbal reasoning only elaborates relational networks) but rather change the context of verbal relations so that individuals are more aware of natural environmental contingencies (exposure) and can see thoughts for what they are (defusion).

3.3. Ethical issues

3.3.1. Potential distress. It was considered that the (snake/spider-related) stimuli and procedures could be anxiety-provoking for some participants (although no live snakes or spiders were presented) and the risk of discomfort/anxiety was made clear in the participant information sheet (see Appendix B). The researcher (a trainee clinical psychologist) was present during procedures to discuss matters arising and provide immediate support as necessary. The researcher had experience of supporting participants through anxiety-provoking study procedures (e.g., Ferguson, Moghaddam, & Bibby, 2007) and was able to draw on basic clinical skills in providing immediate assistance for participants. Participants were able to stop the procedures at any time and withdraw participation. If participants required further support they were invited to contact their GP or other resource of their choosing. A leaflet with details of relevant sources of information/support was provided to all participants.

In practice, the researcher regularly checked that participants were happy to continue and reminded them that they could stop procedures at any time. At certain points in the procedure (particularly during the behavioural approach task), some participants did express discomfort. However, when asked if they would like to stop, most participants continued. The only procedure that some participants discontinued was the behavioural task, and this task was of course designed to capture variability in progression. Twenty-three participants (48%) chose to stop the behavioural task before the final step, and this might be considered supportive of ethical conduct, suggesting that the potential influence of

[27] This is how habituation/desensitisation may affect 'cognition', from an RFT perspective.

perceived demand characteristics or coercion was limited[28], and that participants felt able to stop when they wanted to (and thus regulate their distress).

3.3.2. Potential therapeutic misconception. Although the study was not advertised as having potential therapeutic effects, one potential concern was that individuals might volunteer for the study in the hope of ameliorating pre-existing spider fear. To counter this possibility, the possible risks/benefits of participation were clearly stated in the participant information sheet and potential participants were asked to consider this information for at least 24 hours before providing consent. It was stressed that there would likely be no immediate benefits for study participants – and that some participants might be made more aware of their fear. In the event, some participants may have found that the study had beneficial effects in reducing their fear of spiders (as reflected by decreases in explicit fear and disgust ratings). Reports from individuals following the approach task suggested that this task likely had a beneficial effect for individuals who were able to complete the procedure or who made greater progress than they had expected.

3.3.3. Confidentiality. Efforts were made to limit the use of personally-identifying information: only contact details necessary for arranging participation were required. Interested participants had the opportunity to additionally provide an address for the purpose of receiving study results feedback (anonymisation of data meant that it was not possible to feed back individual scores). This information was held in a separate secure database, was not linkable to any study data, and was destroyed following feedback of results.

3.3.4. Use of deception in the PT-BAT. One of the tasks used in the present research involved the provision of misleading information to participants (i.e., deception). Information in the PT-BAT implied that there was a greater probability of encountering a spider than was the case: in actuality, no spiders were present in this task.

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[28] Whilst early-stopping suggested that participants felt able to discontinue participation if they wanted to (i.e., that they did not perceive a demand to complete the task), it is possible that other demands were perceived (involving interpretation of the researcher's expectations) and influenced behaviour in the study.

BPS guidance (2009) and APA rules of conduct (2002) suggest that the use of deceptive techniques in research requires strong scientific rationalisation and is only justifiable if alternative procedures are not feasible. Analyses of the experimental psychological literature suggest that deception is more commonly used than would be expected if this guidance was routinely observed (Hertwig & Ortmann, 2008a). In the present instance, resource limitations and competing ethical considerations restricted the choice of behavioural approach tasks, such that use of live spiders lacked feasibility. The PT-BAT represented a validated alternative to live-spider approach tasks (Cochrane et al., 2008). It was methodologically important to index actual behaviour in the context of perceived spider threat, and it has been argued that compromised research designs can be inherently unethical (Rosenthal, 1994). Informing participants of the true odds of encountering a spider would have invalidated the behavioural task, undermined the usefulness of the research, and thereby weakened the justification for allocating resources to the research (at cost to society) and inconveniencing volunteers (who participated, in spite of advertised risks, to support potential gains for future understanding). Thus, a deceptive method (the PT-BAT) was employed to preserve the integrity of the experimental design and thereby facilitate findings with greater potential benefits (contributing to scientific knowledge with clinical implications).

Although we have attempted to justify use of the PT-BAT (at some cost to participant autonomy) in terms of the impracticality of alternatives and beneficence for future understanding, it is important to consider what harm may have been caused by the use of a deceptive method in the present study. The principle of non-maleficence is considered below, with respect to the individual, the profession, and wider society.

Exposure to real spiders would likely have been at least as harmful to participants in terms of eliciting anxiety (Olatunji et al., 2009), in addition to putting animals at risk and exposing participants to the possibility of physical harm (receiving a spider bite)[29]. However, participants in the present study may have experienced harm beyond the potential anxiety

[29] Thus, study information arguably implied a more risky situation than was the case – and avoided the more questionable practice of implying a more benign situation (BPS, 2009)

that they had been prepared to expect (by the participant information sheet), such as shame at being 'duped' by the experimenter (Baumrind, 1985). Against this, reviews of available evidence suggest that participants do not generally perceive harm or feel resentful after participation in deceptive procedures (Kimmel 1998; Christensen, 1988). Thus available evidence largely supports the argument that possible costs to participant autonomy (in providing misleading information) do not necessarily equate to harm (Herrera, 2001). However, some contradictory findings exist (Hertwig & Ortmann, 2008b), with the 'severity' of deception likely to be an important (if difficult to define) factor (Rosnow & Rosenthal, 1997). In the present study, no objections were raised by participants regarding the deceptive PT-BAT (those who commented indicated that they found the procedure interesting and revealing). According to BPS guidance (2009) this may be taken to suggest that the deception was not inappropriate (the BPS considered the reaction of participants when debriefed to be the central principle in deciding ethicality). However, there are reasons to question the validity of participant self-report following deception (Baumrind, 1985). To compensate for perceptions of compromised autonomy, and in the context of perceived demand characteristics, participants may assert that they were glad to participate and endorse the scientific value of the deception (rather than admit to harm and resentment).

The use of deceptive methods may accrue harm to the wider profession, principally by increasing suspicion towards research psychologists[30] and 'contaminating' the participant pool for others (Baumrind, 1985). To date, there is little evidence to support feared cumulative effects of deceptive methods on attitudes towards psychological research (Sharpe, Adair, & Roese, 1992) or on participation/performance in future studies (Bonetti, 1998). Nonetheless it is conceivable that participants from the present study may be more alert to potential deception in future studies. Long-term effects might be less evident in the

[30] Although discussion here centres on psychological research, the possibility of harm to the profession could extend to other forms and practitioners of both psychology and research. In particular, the present research was carried out in the context of doctoral training in clinical psychology, and the conduct and dissemination of the present study may have implications for perceptions of clinical psychologists.

student volunteer participant pool – which is regularly regenerated (Hertwig & Ortmann, 2008b) – but it is possible to see how the use of a deceptive method by one researcher could impact on the work of others. In more rarefied populations, such a contaminating effect could have marked implications for future research. One specific implication of the knowledge gained by debriefed participants in the present research is that they could not now be recruited to another study using the PT-BAT (without confounding the results). Deceptive methods such as the PT-BAT may represent a limited resource: use by one researcher can be considered to be at the cost of another. With repeat use and dissemination, awareness of these methods will likely become more widespread, reducing the numbers of procedure-naïve prospective participants available to other researchers.

Baumrind (1985) wrote of the potential harm done to society by deceptive practices. She argues that the suspicion that may be engendered by deceptive research could have wider implications for increasing mistrust and cynicism in society (particularly towards people in positions of authority). Although it may be difficult to measure such effects, it seems careless to assume that any costs of deceptive research would be limited to the participant (or even the profession).

As Hertwig and Ortmann (2008b) conclude, the contentious arguments, limited empirical evidence, and vague guidance in this area mean that it is difficult to draw firm conclusions about the acceptability of specific deceptive methods. Much is left to the interpretation of individual researchers and their ethical reviewers.

3.4. Scientific issues

The following sections summarise issues pertaining to design/methodology and elucidate the philosophy of science underpinning the present study.

3.4.1. Design and methodology. The present findings are subject to a number of methodological concerns.

As already discussed, one limitation of the present study design was the omission of a control group. Participants in a control (non-intervention) condition could have provided valuable data to aid interpretation of apparent effects in other conditions (for example, permitting identification of intervention effects over and above practice effects). For the present study, it was considered that inflation of required sample size could not be accommodated within available time/resources. However, future research would help to elucidate and build on present findings by implementing a controlled design.

The use of the PT-BAT presented potential methodological issues (in addition to ethical concerns discussed in 3.3). Although the PT-BAT seemed to function as intended in the present study, indexing overt avoidance behaviour that was consistent with spider fear, there are potential issues with the perceived credibility of this measure (e.g., Wagener & Zettle, in press) and it is not clear how repeatable the measure would be. These considerations may limit use of the PT-BAT in future studies, in addition to potential concerns that the unusualness of the jar task (in comparison with natural spider encounters) may produce artefactual results. Work showing that avoidance of the (unseen) PT-BAT spider threat predicts real-world spider avoidance would strengthen the external validity of this measure.

Related to the above, understanding of intervention effects on behaviour could potentially have been strengthened by incorporating a pre-intervention (baseline) administration of the PT-BAT. However, it was considered that this might introduce additional problems: behavioural approach tasks constitute a form of exposure (complicating interpretation of subsequent exposure and defusion intervention effects) and, in the case of the PT-BAT, test-retest properties are unknown. Again, the addition of a control group would aid interpretation of a design with pre- and post-intervention administrations of the PT-BAT, but specific interactions within conditions (e.g., unique effects of PT-BAT performance on a subsequent exposure procedure) would not be discernible.

As discussed in the journal paper (in consideration of findings in relation to study objectives), the relatively poor reliability of the IRAP may have undermined examination of implicit effects in the present study. There is emerging evidence to suggest that IRAP effects can be more reliably detected by reducing the permitted response window from 3000 milliseconds to 2000 (Barnes-Holmes et al., 2010). Theoretically, faster responding should allow for less elaboration, thereby giving a more accurate indication of immediate/automatic relations (with implications for the discriminant validity of implicit versus explicit measures). Future studies might improve precision by reducing response windows and further adapting implicit measurement protocols according to ongoing psychometric work (Krause et al., 2010).

The lack of follow-up measurement is a limitation of the present design. It is not clear how observed effects would endure (and whether other effects might emerge) in the longer term. It may be that the brief exposure and defusion interventions had only temporary effects (e.g., Rachman, 1989). However, it should be acknowledged that defusion exercises, in particular, are not intended to have permanent effects (i.e., produce a sustained state of defusion; Blackledge, 2007). Rather, defusion exercises are intended to provide experiential learning with future applicability (Masuda et al., 2010). The intention is for an individual to learn that they can bring thoughts/experiences under contextual control, shifting their perception to notice the process of thinking (rather than the content) as needed. Of course, follow-up testing could be useful to identify whether, in appropriate contexts, participants can and do implement defusion strategies (after experiential learning). The generalisability of learning to novel situations may be crucial for maintenance of treatment effects in specific phobias (Mystkowski et al., 2002) and other disorders (Bouton, 2002). It would be interesting to examine whether adoption of defusion strategies facilitates more generalisable effects than specific instances of stimulus-exposure learning: this could be tested in future research by carrying out follow-up assessments in novel external contexts.

Although we have discussed potential difficulties relating component or analogue experimental findings to practice, because of additional factors that are introduced in the clinic, a converse issue is that intervention procedures in the present study were unlikely to be process-pure (e.g., Masuda et al., 2010). In attempting to balance clinical applicability and technical specificity, there is a danger that the present design underperformed in both respects (i.e., was neither generalisable nor specific enough to be informative). Further basic and applied research will aid interpretation of the present study and elucidate the link between theory, specific techniques, and clinical effects.

3.4.2. Underpinning philosophy of science. Research and practice around Acceptance and Commitment Therapy (as an extension of behaviourism) has been located within a philosophical paradigm of functional contextualism (Hayes et al., 1999; Hayes, 1993; Biglan & Hayes, 1996). Functional contextualism represents a form of the pragmatic and contextualist philosophies of science described by Pepper (1942, as cited in Biglan & Hayes, 1996). In common with other contextualist approaches, functional contextualism analyses phenomena in terms of situated action[31] (acts in current and historical context) and adopts a pragmatic truth criterion (Hayes, 2004). By this criterion, an analysis is valid to the extent that it supports successful working (i.e., accomplishes a particular goal).

Forms of contextualism are differentiated by their goals, and functional contextualism is distinct from other (more descriptive) forms of contextualism in that it aims to achieve prediction-and-influence (Hayes, 1993). Hayes et al (1999) posit that functional contextualism has two important implications for analysis (in both research and clinical practice): (1) phenomena must be traced back to aspects of the external context; and (2) prediction is not sufficient. The goals of prediction and influence are integrated, such that analyses must support both: aspects of external context that are identified as predictive must also be (in principle) manipulable.

Functional contextualism is promoted as an explicitly empirical pragmatic philosophy (Gifford & Hayes, 1999). The aim here is not to uncover fundamental principles – this may or may not be obtainable (no ontological reality is assumed) – rather it is a practical goal of finding generally applicable principles (to guide other analysts). Thus, functional contextualism differs from other contextualist approaches – which tend to support descriptive, qualitative approaches to phenomena – in linking pragmatic ontological assumptions to concrete scientific practices. By this reading, it is possible to have empirical knowledge without truth (Gifford & Hayes, 1999).

The goal of prediction-and-influence has consequences for the types of research that would be most effective (Biglan & Hayes, 1996). Specifically, the use of experimental methods is considered most useful for

[31] In terms of behaviour analysis this means that understanding the function of behaviour requires understanding of its context. The same form of behaviour may serve multiple functions – and multiple forms of behaviour may serve the same function – depending on context.

identifying variables that can both predict and influence behaviour. From the perspective of functional contextualism, this is the rationale for use of an experimental design in the present study. Such a design allows the manipulation of external contexts (e.g., intervention conditions) and examination of effects on behaviour.

The functional contextualist's pragmatic approach to truth means that different views may be truer (more workable) depending on the context. As an analogy, consider three representations of a web page: its http address, source code, and user interface (browser view). If asked to choose the 'correct' view of the web page, the answer would likely depend on the required function. The http address would be most useful for the purpose of navigating to the page, the source code would be most useful for modifying the page, and the user interface would be most useful for appraising its contents.

As suggested by Hayes et al. (1999), it is important to elucidate the postulates that underlie any scientific inquiry. The goal of the above discussion was not to justify the adopted philosophy (or undermine the assumptions and values of others), but to clarify and accept responsibility for the assumptions that have been made. Given that all forms of analysis assume a philosophical position (implicitly or otherwise) analysis of any one position from another arguably reduces to a clash of distinct standards. This is especially problematic from a contextualist standpoint, which acknowledges the contextual relativism of standards (i.e., there is no absolute truth against which to decide such a contest).

However, it remains important to consider (1) the consequences of a philosophical position relative to its purported purposes and (2) whether its standards have been applied consistently (Hayes et al., 2004). In this way, a paradigm can be critiqued against its own postulates. Similarly, the present study can be considered in relation to its underlying philosophy. Whilst a purported strength of functional contextualism is its integration of "afoundational assumptions with an explicitly scientific epistemology" (p.313, Gifford & Hayes, 1999), the tension in this integration (of pragmatic ontology and concrete epistemology) threaten its philosophical coherence. Functional contextualists appeal to pragmatism to resolve this tension: scientific epistemology is embraced because it is considered functional (towards the goal of prediction-and-influence); similarly, foundations are disregarded because they are not functionally useful (Gifford & Hayes, 1999). Another danger pertaining to the paradigm itself is that its truth criterion may become tautological (Jonassen, 2006). If an analysis fulfils its goal (of prediction and influence), it is considered true (i.e., an analysis was true if it was found to be true). Similarly, the logic of accepting observed predictive and manipulative relationships as 'true' could lead to overvaluation of spurious results (and undervaluation of apparently non-significant results). In practice, these concerns are likely to be mitigated by embracing scientific practices of a priori hypothesising, replication, and cross-validation.

In congruence with the goal of functional contextualism, the present study set out to predict and influence the phenomena of interest. The study examined how responses in different response-contexts (implicit versus explicit) predicted subsequent behaviour; the study further attempted to directly manipulate contexts (intervention conditions) to test effects on later responding. Even the correlational aspects of the research are potentially useful from a functional contextualist perspective (Biglan & Hayes, 1996): learning about relationships between events may inform subsequent attempts to manipulate the probability of an event (directly or indirectly). In the present case, knowledge about how responses in implicit and explicit response contexts predict target behaviour (spider avoidance) could lead to the development of interventions that change the predictive responses (with potential consequences for the correlated target behaviour). In common with underlying principles, it is not suggested that uncovered relationships reflect an absolute truth, but it is considered that findings from the present research context may have some practical generalisability and contribute to functional theories. An implication of the functional contextualist emphasis on practical knowledge is that basic and applied research within this paradigm is complementary (Gifford & Hayes, 1999). Although the present study used a non-clinical sample, its focus on prediction-and-influence does not undermine its potential clinical relevance from a functional contextualist perspective.

3.4.4. A note on the use of language. One area of apparent internal inconsistency is in the language used throughout the present thesis.

To integrate available research on implicit, explicit, and behavioural constructs, terms of reference have been used that extend beyond the formal behaviour-analytic terms of functional contextualist theories (ACT and RFT). Within these conceptualisations it would be more precise to refer to different classes of behavioural response rather than make reference to intrapsychic processes. However, use of language within the present thesis was pragmatic and contextually sensitive in attempting to link mainstream (cognitive) implicit research with RFT-based procedures whilst limiting the use of theory-esoteric technical language. There is a danger of satisfying neither the purist nor the general reader in this approach, but the flexible use of language is consistent with other works communicating RFT principles (e.g., Hayes & Smith, 2005; Hayes et al., 1999).

3.5. Future research

Although specific recommendations for future research and methodological refinement have been discussed within relevant parts of the discussion, a number of general suggestions are presented below.

Controlled studies looking at malleability of implicit cognition (and concurrent effects on self-report and behavioural responses) would usefully inform understanding of how to influence this construct – with implications for theory and clinical practice. Evidently, there is scope to improve the measurement of implicit cognition (given the limited internal and test-retest reliability of measures in the present study and more generally). Research improving the precision of implicit measurement would strengthen subsequent (basic and applied) investigations into the predictive/discriminative utility of implicit cognition, and its malleability.

Further integration of basic and applied research will contribute to our understanding of the processes by which specific treatment techniques might operate – and how these may be consequently refined for future practice. The relatability of basic and applied research may be complicated by the addition/subtraction of influential real-world variables, but the potential benefits for deriving theoretically-informed practice and practically-informed theory are considerable.

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Appendix A

Ethical approval: from University of Lincoln and I-WHO ethics committees.

Ethical approval from the University of Lincoln:

Moghaddam Nima
From: Emile van der Zee [evanderzee@lincoln.ac.uk]
Sent: 15 November 2009 21:45
To: Moghaddam Nima_Golijani
Subject: RE: Application for ethical approval
Dear Nima, this is to confirm that you have ethical approval for your project from today.

Good luck with your project (and as indicated before, it would be great to hear about the results; potentially this could be a paper in a journal like 'Psychological Science' (with a very high impact factor)), all my best,
- Emile
Emile van der Zee PhD
Principal Lecturer in Psychology
Programme director of the MSc in Child Studies University of Lincoln
Lincoln LN6 7TS
evanderzee@lincoln.ac.uk
http://www.lincoln.ac.uk/psychology/staff/683.asp
<https://email.lincoln.ac.uk/exchweb/bin/redir.asp?URL=http://www.lincoln.ac.uk/psychology/staff/683.asp>

Institute of Work, Health & Organisations
http://www.i-who.org

The University of Nottingham

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18/01/2010

Dear Nima

I-WHO Ethics Committee Review

Thank you for submitting your proposal on "The impact of brief exposure and acceptance interventions on implicit verbal relations in spider-fear". This proposal has now been reviewed by I-WHO's Ethics Committee to the extent that it is described in your submission.

I am happy to tell you that the Committee has found no problems with your proposal. If there are any significant changes or developments in the methods, treatment of data or debriefing of participants, then you are obliged to seek further ethical approval for these changes.

We would remind all researchers of their ethical responsibilities to research participants. The Codes of Practice setting out these responsibilities have been published by the British Psychological Society. If you have any concerns whatsoever during the conduct of your research then you should consult those Codes of Practice and contact the Ethics Committee.

You should also take note of issues relating to safety. Some information can be found in the Safety Office pages of the University web site. Particularly relevant may be:

The *Safety Handbook*, which deal with working away from the University.
http://www.nottingham.ac.uk/safety/
Safety circulars: Fieldwork P5/99A on
http://www.nottingham.ac.uk/safety/publications/circulars/fieldwk.html
Overseas travel/work P4/97A on http://www.nottingham.ac.uk/safety/publications/circulars/overseas.html
Risk assessment on http://www.nottingham.ac.uk/safety/publications/circulars/risk-assessment.html

Responsibility for compliance with the University Data Protection Policy and Guidance lies with all researchers.

Ethics Committee approval does not alter, replace or remove those responsibilities, nor does it certify that they have been met.

We would remind all researchers of their responsibilities:

- to provide feedback to participants and participant organisations whenever appropriate, and
- to publish research for which ethical approval is given in appropriate academic and professional journals.

Yours sincerely

Nadina Lincoln

Professor Nadina Lincoln
Chair IWHO Ethics Committee

Appendix B

Participant information sheet and consent form

The impact of brief exposure and acceptance interventions on implicit verbal relations in spider-fear

Name of Investigators: *Dr. Nima Moghaddam, Dr. Aidan Hart, Dr. Mark Gresswell*

Participant Information Sheet

You are being invited to take part in a research study. Before you decide whether to take part it is important for you to 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 whoever you wish. Ask us if there is anything that is not clear or if you would like more information. Take time to decide whether or not you wish to take part.

Background

This project is being carried out by a student at the University of Nottingham toward a postgraduate qualification in clinical psychology. Researchers in psychology are looking to understand how different psychological intervention/therapy techniques might influence thinking processes. Research suggests that some thinking processes occur outside of conscious awareness: these processes have been termed implicit cognition. However, little is known about implicit cognition and we do not know how psychological intervention techniques might influence implicit cognition. The present project is looking at implicit processes in fear of spiders and how these processes might be affected by two different intervention tasks.

What does the study involve?

The study will consist of three phases. In Phase 1, you will be asked to complete implicit and explicit measures of spider fear; these measures will involve responding to pictures of spiders and snakes. In Phase 2, you will be randomly allocated to one of two computerised tasks (exposure or cognitive defusion): these tasks will be basic analogues of clinical interventions. Exposure will involve looking at repeated images/videos of spiders and defusion will involve completing word-based tasks. In Phase 3, you will again be asked to complete the measures from Phase 1 and will also be asked to perform a spider approach task. The approach task will ask you to put your hand into jars with increasing likelihood of containing a spider; you can stop this task (and any other procedure) at any time.

The study will take place in an experimental cubicle at the University of Nottingham. In total, the procedure should take approximately one hour to complete. The researcher (a trainee clinical psychologist) will be present to guide/assist you through all stages of the procedure, although most instructions/tasks will be automated and presented on computer screen. At the end of the study, the researcher will be available to debrief you and answer any further questions.

Why have I been chosen?

You have been chosen because you have responded to a study advertisement.

Do I have to take part?
It is up to you to decide whether or not to take part. If you do decide to take part you will be asked for your written consent at the start of the study. However, you are still free to withdraw at any time and without giving any reasons. If you decide to withdraw partway through the study we would like to keep and use data collected from you before withdrawal (in anonymous form) – but we will destroy this data if you ask us to.

What are the possible disadvantages and risks of taking part?
Study procedures may cause discomfort/anxiety for participants, especially for those who are highly fearful of spiders or snakes. However, you will not be under any pressure to complete procedures that you prefer not to.

What are the possible benefits of taking part?
The greatest benefit is likely to come in the future, because the findings should help to improve our understanding of implicit cognition and how best to design psychological interventions (towards improvement of care services). You will receive an inconvenience allowance of £5 (compensation for time and effort) if you decide to take part in this research.

Will my taking part in this study be kept confidential?
All information which is collected from you as part of this research will be kept on a password protected database and is strictly confidential. Any information about you which leaves the research unit will have your name and address removed so that you cannot be recognised from it.

What will happen to the results of the research study?
The information from the study will be written up as a doctoral thesis (you will not be identifiable). We hope the findings will be used to further understanding of implicit cognition and appropriate interventions. We will send you a summary of our results at the end of the study if you would like.

Who is organising and funding the research?
This study is being carried out by a postgraduate student and his supervisors at the University of Nottingham; the research is funded as part of the lead researcher’s course of study.

Who has reviewed the study?
This study has been reviewed and approved by assessors on the lead researcher’s course of study and by the I-WHO Research Ethics Committee.

Contact for Further Information
Dr Nima Moghaddam
DClinPsy student
I-WHO, International House
University of Nottingham
Jubilee Campus, Nottingham NG8 1BB
Tel: 07866516646
E-mail: lwxngm@nottingham.ac.uk

Please direct any complaints to:
Dr Nadina Lincoln
I-WHO Ethics Committee
I-WHO, International House
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Jubilee Campus, Nottingham NG8 1BB
0115 9515315
nadina.lincoln@nottingham.ac.uk

Participant Consent Form

Title of Project: The impact of brief exposure and acceptance interventions on implicit verbal relations in spider-fear

Name of Researcher: Nima Moghaddam

	Please initial box
1. I confirm that I have read and understand the information sheet dated 09/09/09 version 1 for the above study and have had the opportunity to ask questions.	
2. I understand that my participation is voluntary and that I am free to withdraw at any time, without giving any reason.	
3. I give permission for the information I provide to be used for research purposes (including reports, publications and presentations), with strict preservation of anonymity.	
4. I agree to take part in the above study	

	________________ Date	____________________ Participant Signature
________________________ Researcher name	________________ Date	____________________ Researcher Signature

1 copy for respondent; 1 copy for researcher

Appendix C

Study materials

Participant ID: ☐

Demographics

To begin with, we would like to ask a few basic questions about you:

1. Are you male or female?

(a) Male ☐

(b) Female ☐

2. Are you:

(a) Student ☐

(b) Non-student ☐

3. How old are you?

………………………………………….........

Instructions for IRAP procedures

INSTRUCTIONS

Shown below are illustrations of the four different types of task that will be presented repeatedly in this part of the experiment. To help you understand the tasks each of the four illustrations is explained immediately underneath. Please examine each illustration and then read carefully the explanation attached to it. Please make sure that you understand each task before continuing with the experiment.

Illustration 1

I fear

Select 'D' for **True**

Select 'K' for **False**

Explanation for Illustration 1

If you select "True" by pressing the 'D' key, you are stating that "I fear the spider."

If you select "False" by pressing the 'K' key, you are stating that "I do NOT fear the spider."

Illustration 2

I do NOT fear

Select 'D' for	Select 'K' for
True	**False**

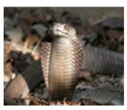
<u>Explanation for Illustration 2</u>

If you select "True" by pressing the 'D' key, you are stating that "I do NOT fear the spider."

If you select "False" by pressing the 'K' key, you are stating that "I fear the spider."

Illustration 3

I fear



Select 'D' for	Select 'K' for
True	**False**

Explanation for Illustration 3

If you select "True" by pressing the 'D' key, you are stating that "I fear the snake."

If you select "False" by pressing the 'K' key, you are stating that "I do NOT fear the snake."

Illustration 4

I do NOT fear

Select 'D' for	Select 'K' for
True	**False**

Explanation for Illustration 4

If you select "True" by pressing the 'D' key, you are stating that "I do NOT fear the snake."

If you select "False" by pressing the 'K' key, you are stating that "I fear the snake."

NOTE: During the experiment other pictures of snakes and spiders will also be presented.

FINAL INSTRUCTIONS

During the experiment you will be asked to respond as **quickly <u>and</u> accurately** as you can across all trials.

It is very important to understand that sometimes you will be required to respond to the tasks in a way that ***agrees*** with what you believe and at other times you will be required to respond in a way that ***disagrees*** with what you believe. **This is part of the experiment**.

When you make an incorrect response for a task it is signalled by the appearance of a red 'X' in the centre of the screen. To remove the red 'X' and continue please make the correct response quickly.

If you do not understand something about the foregoing instructions or have any further questions please talk to the researcher before clicking on the blue button.

Acceptance (cognitive defusion) task instructions

Please read and consider the following information:

Language and cognition have allowed humans to be enormously successful in an evolutionary sense, and people who are good at them generally do well in many areas, especially in their professions. Our problem-solving skills have allowed us to reshape the world we live in.

However, problems arise when we can look only "from our thoughts" rather than "at our thoughts." That narrowness and rigidity can be costly because in some areas of life taking literally what your mind tells you is not the best approach. This is particularly true in regard to our own internal, emotional pain.

<Click here when ready to continue>

Your mind is not your friend.

Minds evolved to give us a more elaborate way of detecting threats to our survival, and they probably helped organise packs of pre-humans in ways that led to less killing, stealing, incest, and so forth.

One thing minds didn't evolve for was to help pre-humans feel good about themselves. Recent studies of natural thought processes consistently show that a large percentage of all mental content is negative in some way. We have minds that are built to produce negative content in the name of warning us or keeping us in line with the pack.

There is a paradox to address: Your mind is not your friend *and* you can't do without it.

<Click here when ready to continue>

Negative thoughts may be relatively automatic but people can become "fused" with the literal content of thoughts: compare "I am anxious" with "I am having the feeling that I am anxious"; the former inflexibly fuses self with thought.

The following task will encourage you to notice what your mind does (the process of thinking) rather than only noticing the products of your mind (the literal content of your thoughts).

<Click here when ready to continue>

Task 1 of 2

To begin, we would like you to think about milk. What is milk like? What does it look like or feel like? Type a few of the attributes of milk that come to your mind (in the text box below):

<Click here when ready to continue>

Now, when you are comfortable, start saying the word "milk" out loud and as fast as you can for 30 seconds. Just keep saying the word "milk" over and over for the whole time. Say it as fast as you can while still clearly pronouncing the word. The computer will time you and let you know when to stop. When you are ready, click below and start saying "milk, milk, milk, milk…"

<Click here when ready to continue>

Please stop now. How did this feel to you? What was your experience with saying "milk" over and over again? Now, in the text box below, type some notes on your response:

<Click here when ready to continue>

After saying "milk" over and over again as rapidly as you could, what happened to the meaning of the word? What happened to the cold, creamy, white substance that you pour over your cereal in the morning? Did the word still invoke the image the same way that it might have before you did the exercise?

Finally, did you notice anything new that might have happened? For instance, it is common to notice how odd the word sounds, how the beginning and end of the word blend together, or how your muscles moved when saying it. If so, note these effects by typing in the text box below:

<Click here when ready to continue>

For most people, the meaning of the word begins to fall away temporarily during the exercise. Noticing that words may be, at their core, just sounds and sensations, is very hard to do when you are caught up in literal meaning.

Now, when you are comfortable, start saying the word "spider" out loud and as fast as you can for 30 seconds. Just keep saying the word "spider" over and over for the whole time. Say it as fast as you can while still clearly pronouncing the word. The computer will time you and let you know when to stop. When you are ready, click below and start saying "spider, spider, spider, spider…"

<Click here when ready to continue>

Please stop now. Just think about what you noticed for a few moments. Click below when you are ready to proceed.

<Click here when ready to continue>

Now, try this particular technique one last time.

When you are comfortable, start saying the word "terrified" out loud and as fast as you can for 30 seconds. Just keep saying the word "terrified" over and over for the whole time. Say it as fast as you can while still clearly pronouncing the word. The computer will time you and let you know when to stop. When you are ready, click below and start saying "terrified, terrified, terrified, terrified…"

<Click here when ready to continue>

Task 2 of 2

Our minds can stop us from acting because we take the content of our thoughts literally. For example, a person who is anxious about a social situation may have the thought "I cannot face it" and may act accordingly (avoiding the social situation).

This task asks you to practice deliberately engaging in behaviour while thinking in an opposing way.

<Click here when ready to continue>

Now, repeatedly type the phrase 'I cannot type' until you have filled the text box below:

Exposure intervention task instruction slides

Exposure task 1 of 2

In this task you will be shown a number of spider images and each image will be shown several times.

Although the images may be difficult to look at, please try to focus on the pictures whilst they are presented. Try to remember the features of each image.

You will occasionally see single words presented on screen; please just read these words silently to yourself.

<Please click here to continue>

Exposure task 2 of 2

In this task you will be shown a number of spider videos and each video will be shown several times.

Although the videos may be difficult to look at, please watch each video carefully. Try to imagine that you are in the room shown onscreen. Focus on the spider, and don't try to suppress your emotional response.

After each video, you will be asked to rate your experience of the video.

<Please click here to continue>

Now please rate the video that you just saw (using the mouse)

How disgusting was the video?

Please click a number on the scale below:

0	1	2	3	4	5	6	7	8	9

Not at all disgusting — Extremely disgusting

Now please rate the video that you just saw (using the mouse)

How frightening was the video?

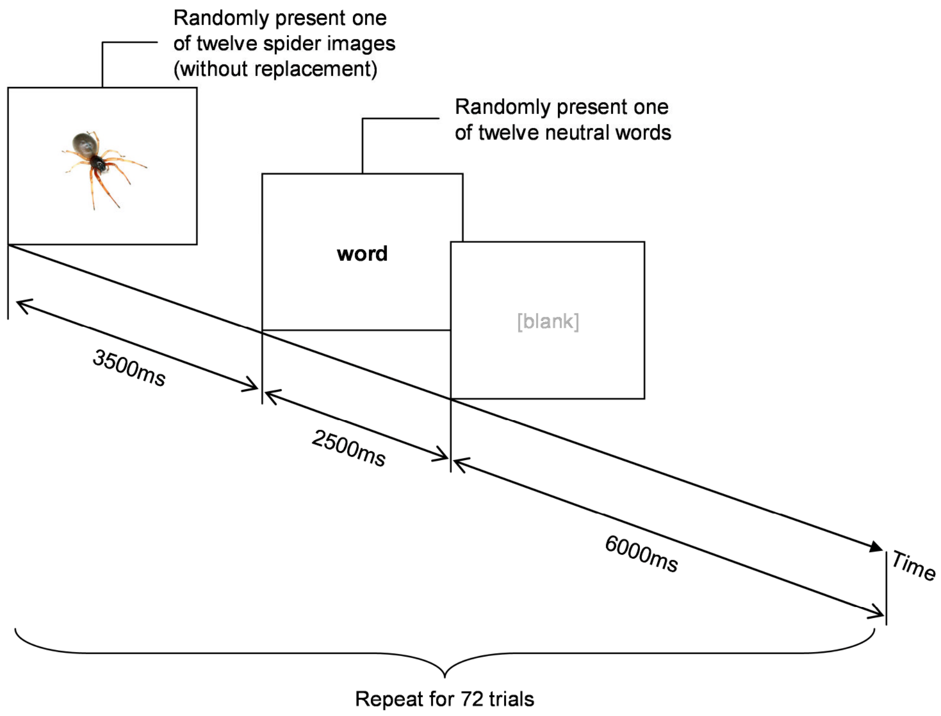
Please click a number on the scale below:

0	1	2	3	4	5	6	7	8	9

Not at all frightening — Extremely frightening

Stimuli presentation procedures

Exposure intervention task 1 of 2



Exposure intervention task 2 of 2