Interpretation in Social Anxiety: Measurement, Modification, Mechanism and Mood.

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Declaration

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Abstract

Cotemporary cognitive models of emotion, in particular social anxiety, emphasise the role of biases in in information processing. Interpretive bias is central to this biased cognition, however research concerning it currently features a number of deficits. In particular, methods of measuring and modifying interpretations are currently of limited scope. The mechanism of action of interpretation modification and its interface with affective processing is also currently not directly evidenced. The current thesis begins by considering methods of improving cognitive bias modification for interpretation (CBM-I) by including explicit instructions and participant generated content. An innovative measure of interpretation is integrated and applied alongside conventional outcome measures for these tasks. The thesis moves on to consider the role of mood manipulation in interfering with or accentuating the outcomes gained in CBM-I work, and the roles of state and trait anxiety in interpretation in general. The primary findings were an absence of evidence for a training effect from both the conventional and newly-applied CBM-I techniques used across the experiments (making mood investigation inconclusive), but varied state and trait associations for the different measures of bias applied. Taken together, these results suggest a more conservative impression of the effects of CBM-I than that found in prior literature and imply caution with its application and assumptions regarding its mechanism of effect. Furthermore, they suggest that a conventional closed-resolution measure is responsive to state and trait variation in social anxiety, and that there is a trait associated bias in likelihood approximation but not generation or evaluation of negative material by socially anxious individuals. Implications of these results for theory and further empirical practice are discussed.

Chapter One: Literature Review Part One

Anxiety, Biases and Bias Modification

Over the course of this review chapter, the nature of anxiety, its cognitive portrayal, and the specifics of this modelling relevant to social anxiety are covered. Following these, the review narrows to explore the derived consensus on the instrumental role of cognitive biases in emotionality. Particularly, this is with a focus on the particular biases associated with high social anxiety and the evidence gained so far regarding the existence and nature of these. Attempts to modify such biases with direct targeting via simple cognitive interventions, and the evidence for these, are then discussed to provide a foundation for the following chapter which outlines how the current thesis seeks to address these.

Anxiety: The Need for Intervention Development

Anxiety is a distinct cognitive, affective and behavioural phenomenon with large variability in specific manifestations, causes and effects. It is affectively aversive, elicited by both trait predispositions and an array of short term state factors, and is associated fundamentally with ambiguity, goal conflict and *potential* threat. Whilst varying definitions exist, in the current thesis anxiety is considered distinct from fear, this being entirely specific to threat *avoidance* (see Gray, 1982). Its severity ranges from the everyday, mundane and adaptive to the clinical and severely impairing. Overall, whilst it may have beneficial functions, anxiety that occurs in unnecessary settings, too often and in excessive amounts presents one of the greatest obstacles to mental health in modern society. In the most extreme manifestation, there are a variety of estimations of prevalence in existence, and statistics vary in availability and diagnostic criteria. Clinically diagnosed anxiety disorders appear to have nationwide lifetime prevalences ranging from a minimum of four percent of the population to upper approximations of around thirty-one percent for certain countries, with a global average of seventeen (Somers, Goldner, Waraich & Hsu, 2006). They appear to broadly be increasing when age cohorts are compared (e.g. Kessler et al., 2007). According to survey data, they are currently the most prevalent of all mental health disorders across Europe (Wittchen & Jacobi, 2005), and the United States (Kessler, Chiu, Demler, Merikangas & Walters, 2005; Kessler, Berglund, Demler, Jin, Merikangas & Walters, 2005). Such anxiety disorders bring a host of negative implications. Beard (2011) notes the existing evidence suggesting that individuals meeting diagnostic criteria for anxiety disorders are subject to a poorer quality of life, educational and occupational impairment and significantly higher suicide rates than the general population. Woodward and

Fergusson (2001) add longitudinal associations with depression, substance dependence, and comorbid other anxiety disorders to this list. Furthermore, research regarding the economic impact of anxiety in the United Kingdom notes considerable costs (e.g. McCrone, Dhanasiri, Patel, Knapp & Lawton-Smith, 2013). As such anxiety has a considerable societal and socio-economic burden. These costs are also predicted by the authors to rise, particularly if progress is to be made in combating maladaptive anxiety successfully in a population with rising levels of it.

Existing approaches to intervention for anxiety

The situation outlined in the above section has already received a wide variety of responses, with interventions being developed from a variety of standpoints. In considering these it is necessary to leave aside interventions at the societal level (at which it is acknowledged there is indeed considerable anxiety causality, but a comprehensive review of which is beyond the scope and focus of the current thesis). Simple lifestyle changes (for example caffeine reduction to reduce arousal levels or diet changes to potentially improve levels of available tryptophan proteins for serotonin production) have also been omitted as a huge variety of these exists, and the varying qualities of formal evidence for the different types, along with their considerable interface with individual differences, would also be beyond the scope of this review. Accordingly, the remaining contemporary evidence-based clinical solutions at the individual level of analysis can be broadly classified into the (directly) psychopharmacological and the psychotherapeutic.

The former class encompasses a host of psychoactive substances. Maladaptive anxiety is conventionally accepted to have its chemical basis in dysfunction of benzodiazepine, serotonin and noradrenaline-based neural circuity (Ninan, 1999). Accordingly evidence, policy and prescription currently centre on the following classes of intervention (Choice and Medication, 2016). Benzodiazepines may be prescribed for short term management to aid the release of the calming GABA neurotransmitter. Selective serotonin and selective serotonin and noradrenaline reuptake inhibitors (SSRIs and SNRIs respectively) are frequently prescribed for longer term treatment to increase the levels of these mood-regulating transmitters, and other medications targeting serotonin and/or noradrenaline chemistry (such as Buspirone, Pregablin and older tricyclic medications) may also be applied. Other pharmacological interventions (for example beta blockers) are sometimes prescribed but for more specific management purposes (e.g. of physical correlates of a highly anxious state) and often not directly targeting the underlying neurochemistry of the anxiety itself. Other neurochemical pathways (including a focus on corticotropin-releasing factor, substance P, neuropeptide Y, oxytocin, orexin, and galanin) are under more recent investigation, but have yet to yield widely applied interventions (see Mathew, Price & Charney, 2008).

All psychopharmacological interventions have a considerable interface with individual differences, and can be accompanied by a diverse host of side effects when interacting with an individual's unique biochemistry and personality, even the relatively tolerable and newer SSRIs (Ferguson, 2001). The range of these is considerable, including but not limited to addiction, sleep deprivation, neurological and cardiovascular problems, and even exacerbation of symptoms or the triggering of additional symptoms like episodic panic attacks and comorbid depression in certain cases (serotonin chemistry in particular has a complex relationship with mental health symptomology). Additionally, such treatments do not directly address the full variety of cognitive and behavioural manifestations or antecedents of anxiety, simply targeting the affective neurochemistry.

Attempts at psychotherapeutic intervention meanwhile are considerably more diverse. Psychoanalytic, psychodynamic, humanistic, person-centred, and integrative approaches are a few examples of those which have been applied historically to the development of counselling and "talking cures" for anxiety. Currently, interventions rooted in cognitive psychology and behaviourist principles benefit from the most substantial evidence base (see Hofmann, Asnaani, Vonk, Sawyer & Fang, 2012) and widespread first-line implementation for anxiety in state public health provision. This is not without its practical caveats. For example, full programs of cognitive therapy can be costly, and barriers such as limited funding and limited numbers of assessment and therapeutic professionals create delays and waiting lists in even the health services of relatively affluent countries (British Association for Counselling & Psychotherapy, 2014). Relapse prevention may also be problematic, with limited data suggesting that whilst they appear to superior to comparable alternative treatments, the effects of cognitive and behavioural therapies appear to often fade over time to an extent where further treatment is necessary (Butler, Chapman, Forman & Beck, 2006; Ludgate, 2009).

The above said, what is broadly termed the cognitive perspective as a whole is still contemporary and providing (in conjunction with insights owing to more recent innovations at the level of neuroscience) considerable insight into psychological aspects of phenomena in language, memory and all of the other key cognitive faculties that an anxiety intervention might need to target, at a theoretical level. It is thus necessary to carefully consider contemporary cognitive science for opportunities to further develop interventions which might:

- Replace cognitively-focused therapies in instances of unavailability or where individuals present with less severe symptoms.

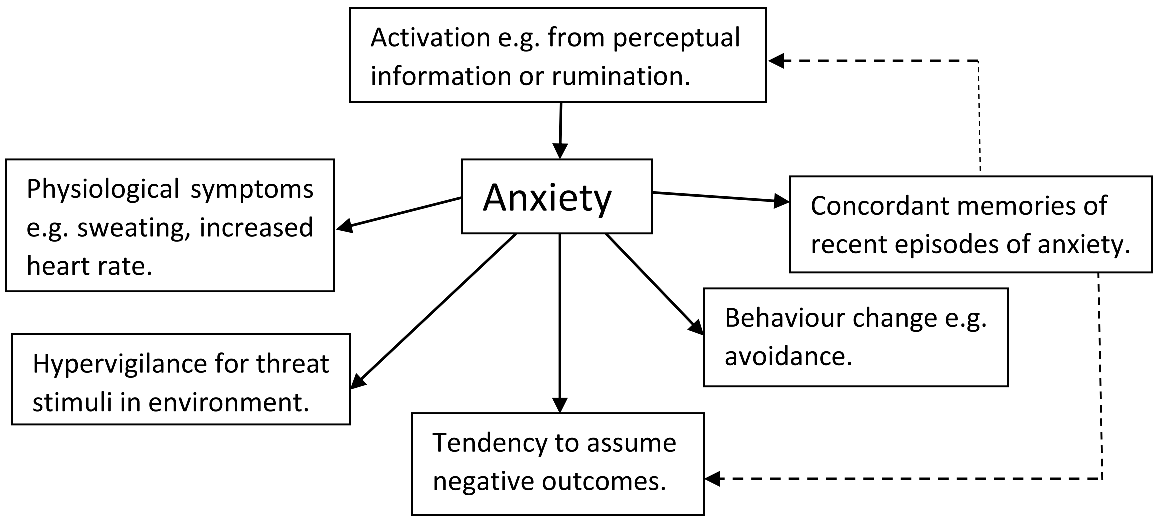
- Underpin and supplement cognitively-focused therapies, either by reiterating and maintaining training of key concepts, or by targeting broader supporting faculties. This could be both during a program of full therapy, before it as a preparation whilst on a waiting list or subsequent to It for the purposes of relapse prevention.

In recent literature many such attempts have displayed a notable focus on the concept of cognitive biases. Mathews and MacLeod (1994; 2005) provide good commentary on this, and it is elaborated upon in the following sections of the current review. The development of the focus via the relevant influential theoretical models of anxiety (with a particular focus on social anxiety) is detailed, before the concept and its specific manifestation across faculties such as attention, memory and interpretation (the primary focus of the current thesis) are outlined. This is followed by detail on the interventions themselves designed to target such biased cognition in anxious individuals, and the evidence base relating to them.

A Summary of Cognitive Accounts of Anxiety and their Implications

In terms of contemporary cognitive theory, Bower's (1981) associative network model is arguably the earliest account of relevance to the current thesis. Here the existence of emotional "nodes" in networked cognitive systems is suggested. The essential principle is that when an emotion is activated a spread of congruent activation across relevant, connected processes and faculties is observed. A basic schematic of this process is included in figure one below:

Figure 1: Examples of the causal relationships of anxiety as per Bower's (1981) model



As such, in people for whom certain emotional nodes are continuously activated (through extended exposure to a particular environment such as being in a stressful job or relationship, an inherent genetic susceptibility to activation or a combination thereof) the model predicts an accompanying constellation of distorted processing in areas such as memory and interpretation. This then goes on to influence subsequent behaviour, cognition and importantly further emotion. This can potentially maintain the activation of negative emotions even when no longer appropriate or adaptive, if conditions within the network are appropriate. For example, in the figure above, it is conceivable that if initial anxiety is significant and sustained (or the individual is subject to a genetic or developmental vulnerability), the activation of concordant memories of recent anxiety would create further rumination, which would activate the anxiety node further. Similarly they might also have a direct effect on the expectancy of negative outcomes. The dotted lines in the figure above highlight these two such examples of the potential for feedback loops in Bower's (1981) conceptualisation.

Further early theoretical developments were highly influenced by Bower's emphasis on such connectivity. Leventhal (1984) for example provides a generally concordant development of Bower's theory, expanding this to emphasise the organisation of cognitive responses in terms of how abstract or concrete they are. Teasdale and Barnard (1993) placed a similar emphasis on both a hierarchy of abstraction and on interconnectivity (albeit between semi-distinct interacting cognitive subsystems with their own processing "language" rather than a universal network of nodes) and the potential for maladaptive feedback loops that this allows. Because of the overall emphasis increasing emphasis in these theories on such interaction, researchers began to hypothesise regarding the effects of distorted processing in emotion-relevant faculties in creating and maintaining emotional states and disorders.

Further early-contemporary theorisation from Williams, Watts, MacLeod and Mathews (1988) expanded upon Bower's (1981) general associative network emphasis with a focus on the early, automatic elements of the allocation of processing resources to threat in anxious states via a simple initial model. The model focuses on three key assertions: that allocation of early processing resources is automatic, that it is based on threat detection, and that it is generally modulated by trait anxiety. The specifics of these processes and their interaction with later elements of processing was left to subsequent models. The latter was accounted for to some extend in modelling by Ohman (1996) who proposed three key components to the cognitive processing relevant to anxiety, outlining a feature detection system communicating with an expectancy system via a significance evaluation system. In this account stimuli would be registered initially by preset, automatic criteria instilled in the feature detection system. The significance of the overall constellation would then be assessed by the significance evaluation system and conveyed to the expectancy system. Here elements like the activation of relevant memories would take place for determining a prediction of what might be a likely outcome, prompting a potential further cascade of processing.

Later elements of processing were considered as even more salient in Wells and Matthews' cognitive model of anxiety (Wells and Matthews, 1996). Here, in what is arguably a slightly extreme inverse of the automatic foundation suggested by other theorists, the distinctive elements of early processing are considered subordinate and only occurring due to a conscious and controlled decision to be vigilant to threat, and a belief that it is important to monitor for threat relevant stimuli. Mogg and Bradley (1998) approach this from a different perspective, arguing for a. the salience of affective motivational processes (in which high trait anxiety is a key mediator) and b. the operation of dual process systems. The latter conceptualisation does have considerable overlap with prior models, particularly the feature detection and significance evaluation systems proposed in Ohman's (1996) model. One key implication of the emphasis on high trait anxiety as a motivating factor is that in order to distinguish between the processing of high and low anxiety individuals in response to threat, one must do so under conditions of *moderate* or *possible* threat, as conditions of definite threat will be automatically attended to by all individuals. This idea resurfaces in other theoretical and experimental work, and is a particularly key consideration in the design of the interpretive bias studies considered later in this review.

Mathews and Mackintosh's (1998) model also emphasises the role of ambiguity. Furthermore it suggests that stimuli are considered in parallel and compete for resources, with the processing of threat-relevant material strengthened via the activation of a threat evaluation system. The activation of this system importantly has different inter-individual thresholds (determined by factors such genetics and exposure to a variety of threat representations), thus allowing variation in the nature of this processing when weak/ambiguous danger cues are present. The process can therefore only be countered to some extent by effortful voluntary control, particularly in high anxiety, low threshold individuals. In this way Mathews and Mackintosh (1998) urge a focus on the automatic maintenance components of maladaptive anxiety-related processing.

Bar Haim et al. (2007) offer a theory that depicts a temporal chain of events in a similar manner to the prior depictions. They suggest that a pre-attentive threat evaluation system scans stimuli and feeds salient and threatening stimuli into a resource allocation system that allocates cognitive resources and initiates physiological, affective arousal. Subsequent to this a guided threat evaluation system determines whether attention should continue to be dedicated to the stimulus (and arousal /relevant cognitive resources should continue to be activated as a result) or whether a goal engagement system concerned with the individual's more controlled and long-term interests can be safely engaged instead.

Eysenck et al. (2007) provide a further focus on the slower and more executive processes, distinct from that present in Wells and Matthews' (1996) work. Their conceptualisation depicts anxiety as bringing about an absence of instrumental executive processes that allow attentional control: the inhibition and shifting of attention. It is proposed that anxiety weakens an individual's ability to inhibit and control automatic responses, including the ability to shift attention voluntarily. As such if this is combined with the networking or cascade effects suggested in the other theoretical accounts, it is understandable how such a low-level process might have a profound bottom-up chain of effect.

It is notable that alongside the above theorisations, in the therapeutic literature a generally concordant account of the cognitive-affective component of anxiety was developing concurrently. Of particular note within this area of the literature (e.g. Beck and Clark 1997; Clark and Beck, 2010) is the emphasis on five key features of abnormal or pathological processing in anxiety. Anxious individuals are depicted as having *impaired functioning*. It is further necessary for pathology that this impairment *persists* over a considerable time period, and that it involve considerable *dysfunctional cognition*. More specifically, anxious individuals are suggested to be prone to *hypersensitivity* to certain kinds of stimulus and to *false alarms* regarding threatening stimuli. The distinction between these latter three constructs is debatable (dysfunctional cognition is a rather broad term, used in the authors' elaboration to emphasise a more ruminative side to dysfunctional anxiety but under which the latter two constructs could nonetheless arguably be subsumed and portrayed as sub-elements). Nonetheless however it is the implications of the latter two features that are most relevant to projects like the current thesis: *stimulus hypersensitivity* emphasises the notion of distortion in early, detective processing and *false alarms* suggests a tendency to *interpret* situations as threatening when this is not the case.

Cognitive Models specific to Social Anxiety

Alongside the general cognitive models of anxiety, a limited number of theorists have offered further specific models, extensions or other elements of theorisation specifically relevant to the distinctive cognitive processing associated with social anxiety.

In Beck, Emery and Greenberg's (1985) conceptualisation a three-layered system operates, with negative automatic thoughts being the shallowest of this layer. A negative triad of mutually interacting, maladaptive cognitive processes regarding self, the world and the future operate here.

Within these a number of sub-processes are proposed, key ones being selective abstraction (a general tendency for certain potentially threatening details of the perceptual environment to become salient and other more benign elements to be downplayed and ignored) and generalisation (a tendency to extrapolate broad and catastrophic conclusions from limited perceptual evidence).

Below the negative automatic level in Beck's (Beck, Emery and Greenberg, 1985) modelling of social anxiety lies a set of rigid core beliefs (primarily implicit but potentially declarative) which serve to translate general schemas at the deepest level of processing into specific cognitive biases. These are often unhelpful "if-then" inferences, for example "if I speak in front of a group of people, I will make mistakes and they will judge me negatively as a result". The schemas themselves at the deepest level are the relatively elaborate and enduring constructs that allow the individual to make sense of their perceptual experiences by providing an organisational framework for concepts and their relationships. For a socially anxious individual maladaptive schemas focus on failure, and on social threats and judgments. When activated by processes corresponding to these themes, the schemas consequently activate the core beliefs and specific biases corresponding to them. In this aspect Beck's model corresponds somewhat to the cascade effect suggested by White, Suway, Pine, Bar-Haim and Fox (2011), although depicts the mechanism in a slightly different way. Clark and Wells (1995) and Rapee and Heimberg (1997) offer further models concordant with Beck, Emery and Greenberg's (1985) general focus, whilst offering variant models based on their interpretation of the experimental literature. The specifics vary and for the purposes of this review are of relatively little consequence, but the models overall emphasise a number of processes operating in feedback loops in which processing could deviate from the adaptive and then serve to maintain and accentuate a maladaptive cognitive (and thus affective/behavioural) state. As an example of one such loop in Rapee and Heimberg's (1997) model an individual might misperceive their social audience due to a faulty schema, and then misallocate attentional resources as a result. This could cause them to disproportionately attend to external indicators of negative evaluation and make an inaccurate and negative interpretation of how their audience perceives them. This would contrast unfavourably with their internal schema of how they believe their audience perceives them. They might then judge the probability of negative outcomes from the social event as higher, which in turn raises the negativity of their subjective behavioural, cognitive and physiological experience (with specific symptoms such as sweating, inhibited speech or a raised pulse). These perceived internal cues reinforce the mental representation of self in the eyes of others and complete the loop, creating further effects.

Hofmann (2007) synthesises more recent research findings to offer a broadly concordant but updated model. The conception is somewhat chronological in the sense of depicting the unfolding of events as a socially anxious individual entering a situation, but it does allow for a cyclical causality of maintenance or accentuation between the initially consecutive elements via feedback loops. Initially, perceptions of high social standards and the associated difficulty reaching them become highly salient to the socially anxious individual. Cognitive-affective apprehension and an associated increase in self-focused attention result. These then trigger overestimation of both the negative valence and likelihood of outcomes of the social situation, and further low approximations of control over an anxious response to such situations. At this point Hofmann (2007) stresses the negative views of self as a social object, an element which previously received emphasis in both Clark and Wells' (1995) and Rapee and Heimberg's (1997) prior work. Following this, social mishaps involving negative evaluation are naturally anticipated and so avoidance and/or safety behaviours are engaged in, followed by excessive rumination, which helps to maintain the prior elements in a cyclical manner.

Further, relatively minor theoretical updates have recently emerged. For example Heimberg, Brozovich and Rapee (2010), updated Rapee and Heimberg's (1997) original model to better account for the instrumental role of imagery as potentially central in the unique cognitive configuration of the socially anxious and their fear of negative evaluation. As such they suggest that it is the socially anxious individual's personal schema of how the scenario is likely to unfold which is instrumental in activating the cognitive and behavioural specifics they display. Jazaieri, Morrison, Goldin and Gross (2015) describe a five-factor behaviourally-focused summary of the emotion regulation processes that individuals high on social anxiety (particularly clinical patients) tend to show deficits in. Firstly, they suggest that these individuals exhibit distinct *situation selection*, which has a past and a future component. The past element encompasses their selective recall and depiction of past social situations, which is more negative than equivalent individuals from the general population. The present/future component then refers to the subsequent process of avoidance of subsequent, potentially anxiety-invoking scenarios that the inferences from such selective recall provoke. Should this strategy not entirely suffice, or should the situation be too moderate, the authors note that individuals with high social anxiety will attempt *situation modification* (for example moving to a corner of a room and sitting away from others) to minimise the chance of anxiety-invoking social encounters. They will implement, with varying degrees of adaptivity, *attentional deployment*: showing a preoccupation with the negative elements of the scenario and (if possible) implement a deliberate strategy both to minimise the effects of this and to exert further situation modification (for example getting out a mobile phone and absorbing themselves in something on it rather than engaging in eye contact with people in the room). After the social encounter itself the socially anxious individual will engage in *cognitive change*: convincing themselves that it was a negative situation and that it was best that they did not engage, and *response modulation*, a process of altering, avoiding and concealing the high levels of negatively-oriented thoughts and feelings they experienced.

Biases: A Unifying Theme of Theories of Processing in Social Anxiety

Whilst they differ in emphasis and in specifics, what the outlined models of anxiety in general and social anxiety have in common is the prediction that individuals with high social anxiety will have accompanying distortions in their processing of information: a cognitive bias towards certain kinds of information processing, and in particular towards *negatively valenced* processing. In accordance with the historical cognitive emphasis on modularity, researchers investigating dysfunctional cognition began to break down this general notion of skewed information processing causing undesirable emotional states. Furthermore they began to specifically and explicitly target biases in particularly influential regions of processing. This was more out of a pragmatic need for focused investigation, and recognition of the instrumental role of key faculties like attention, rather than a reductionist or isolationist motivation. Using models like Bower's (1981) and Jazaieri, Morrison, Goldin and Gross's (2015) as a theoretical context, the feasibility of a bias in one area of stimulus processing creating concordant processing in others, and going on to create or maintain a negative and maladaptive emotional state becomes apparent. Finding such areas of potentially biased processing, clarifying the key processes, determining the causality involved, and attempting to intervene thus became major objectives for researchers.

On a definitional note, the term 'cognitive' bias developed within contemporary cognitive research to denote information processing that deviates significantly from "normal". Whilst the nature of normality is complex, debatable and ill-defined, in the pathology-focused research relevant to this review the working definitions used focus on the extent to which the individual's cognition and the associated behaviour and emotion are adaptive and prevent or minimise negative experiences. It is important to note that this adaptivity can be seen as distinct from objectivity, especially given that research indicates that adaptive processing can sometimes lack strict objectivity (for example the considerable body of work surrounding depressive realism initiated by Alloy and Abramson's (1979) paper). As such many relatively "normal" individuals may actually exhibit a moderate optimistic bias in certain areas of processing, which whilst it does not provide them with a particularly objective assessment of their current circumstances, may prove nonetheless adaptive for them overall. Such biases are thus not the concern of clinically-focused research (unless perhaps conspicuous in their absence). Functionally speaking then, a cognitive bias in the clinically significant sense is perhaps best defined as a processing feature which prevents individuals from functioning in an optimally adaptive way. More specifically the unifying element of these processing features appears to be a preoccupation with negatively-valenced stimuli. Practically and causally speaking, such bias might have a direct *causal* role for an excess of unpleasant experienced emotion, or a *contributory one* towards its maintenance.

Specific variants of cognitive bias: biased attention. An attentional bias, at the broad level, is simply a tendency to differentially allocate attentional resources to negative emotional stimuli that present a "threat" (real or perceived). More specifically, it has traditionally been thought of to involve a *preoccupation* with negative emotional stimuli. More recently however, as studies have explored the time frame of such a bias, certain research (considered later in this section) suggests that a subsequent element of *avoidance* of emotional stimuli may also occur. Attentional biases are one of the most frequently-evidenced phenomena to be associated with anxiety and have attracted a considerable amount of research due to their very early involvement in the processing of external stimuli: under the cascade or network modelling of processing in anxiety prominent in the cognitive models detailed earlier in this review, a bias in attention may conceivably be able to influence a considerable amount of further, ensuing cognition in a maladaptive way.

An early attempt to evidence and measure biased attention in emotional processing applied an adapted version of a dichotic listening task originally developed by Cherry (1953). In this the selectivity of attention was initially investigated by having participants listen to two streams of audio simultaneously but being instructed to attend to one throughout and indicate when certain target words were heard, regardless of which stream they were heard in (with the overall finding that little could be recalled from the unattended stream). In the modification by Burgess, Jones, Robertson, Radcliffe and Emerson (1981), these general target words were replaced by phobic-specific words with the design administered to clinically phobic participants and controls. Clinically phobic participants were found to have superior recognition of phobia-relevant targets such as "failure" in the message that they were not instructed to attend to, with the results forming early evidence of a selective processing bias associated with emotion. The implication was that these potential threats were granted greater salience due to the clinically phobic participant's overactive threat detection, and that such a biased threat detection system might play a role in maintaining their maladapative anxious state.

Many attentional bias experiments have utilised reaction times as a measure. In this way, a large portion of the early experimental evidence for attentional biases was first captured through work based on observations of the emotional Stroop effect. Similar to the conventional Stroop effect (Stroop, 1935) (in which discordance between written colour names and the colour they are written in causes slower identification of the colour), participants in emotional states or suffering from emotional disorders were found to take significantly longer to name the colour of emotionally-relevant words (often threatening words) displayed on a computer screen than words irrelevant to their emotion. Gotlib, McCann and Douglas (1984) were the first to extend the Stroop effect to emotional populations in their research into depression, noting that those with depression appeared to be continuously primed towards the negatively-valenced information in the test. Considerable research employing this neat paradigm as a method of measuring such priming or bias followed (see Bar-Haim, Lamy, Pergamin, Bakermans-Kranenburg & Van Ijzendoorn, 2007, for a review). Williams, Mathews and MacLeod (1996) explore some of the potential mechanisms behind the effect, suggesting that hyperactive pre-attentive, automatic processes for threat detection challenge attentional control for processing resources.

The Stroop paradigm has attracted some controversy however. Researchers have attempted to dissect the effect and given alternative or conflicting explanations. There is, for example, debate regarding whether there is a fast, automatic "online" bias present or a difficulty disengaging with threatening material located in more general, potentially offline processing (see Mathews & MacLeod, 1985; Phaf & Kan, 2007). Particularly anxious people may instead spend more time considering threatening material and its implications, and have difficulty moving on to other content. Several other explanations have been outlined. Some have suggested a threat-driven generic slowdown or disruption wherein anxiety diminishes processing capabilities generally (Eysenck, Mathews & MacLeod, 1987; Algom, Chajut & Lev, 2004; Mathews and MacLeod, 2005). As such, when a highly anxious or phobic individual encounters a stimulus perceivable as a threat, their cognitive resources in general may enter a state of relative lockdown. This phenomenon would be noteworthy in itself as a global influence on processing (indicating a pivotal and far-reaching role for anxiety within this) but not a specific bias in attentional processing. Some researchers have also suggested that processes of avoidance may be operating as well as, or instead of, the proposed disengagement difficulty to create the effect (De Ruiter & Brosschot, 1994): highly emotional participants may avoid processing the entire stimulus *per se* to create the observed delay. As such, Stroop procedures (whilst generally still considered an important indicator of the presence of attentional bias in anxiety in the early research base) are considered by some to be less than comprehensive measures of emotion-related bias in attention, and recent research has moved away from their implementation.

Accordingly a number of additional paradigms have been employed in the investigation of anxiety-related attentional bias. Some have used visual search tasks, in which participants' reaction times to search for stimuli of a certain valence amongst stimuli of a different valence is measured. Participants high on social anxiety have been found to display accelerated target-finding when searching for an angry face in a neutral crowd and significant slowing of this target-finding when a different face was sought and angry faces were used as the crowd (e.g. Gilboa-Schechtman, Foa and Amir, 1999).

A further proposed method of measuring biased attention, and one which has received more widespread application since its inception by Mathews, MacLeod and Tata (1986), is the dot-probe task. In this test, participants view two stimuli (generally words) which are displayed simultaneously on a computer screen. One of these words is emotionally salient (a threat stimulus such as the word "inadequate" in anxiety-focused studies) and one is benign or positive, with both words displayed simultaneously on a computer screen. One of these words is subsequently replaced by a small symbol. The task of the participant is to identify as quickly as possible which of the words, the word on the left or the word on the right, this symbol appears behind by pressing a corresponding key on their keyboard. Attentional fixation on the threat content is evidenced by participants having faster reaction times to the probe when it is behind a threat stimulus than when it is behind a non-threat stimulus. A number of studies have evidenced attentional bias using dot-probe methods, applying them across a range of anxiety manifestations including nonclinical high trait anxiety, generalised anxiety disorder and social anxiety (Bar-Haim, Lamy, Pergamin, Bakermans-Kranenburg & Van Ijzendoorn, 2007). The bias appears to be relatively fast and "online" (it is evidenced only when stimuli are presented briefly to participants and not at longer exposure times) when compared to the attentional bias associated with negative mood and depression, which appears to be more ruminative (Mathews & MacLeod, 2005), and associated with slower processing and the "dwelling on" of thoughts. This reinforces the notion of anxiety-related attentional bias as primarily a comparatively fast, automatic and online phenomenon.

Some studies have incorporated eye-tracking into experimental designs for investigating attentional biases, capitalising on the greater detail (in particular the temporal resolution offered by it being a more continuous measure) that this procedure has the potential to lend studies. Mogg, Millar and Bradley (2000) for example were importantly able to evidence a bias in clinical general anxiety disorder patients towards angry faces at first fixation when no bias existed in terms of a dot-probe task. This may have been due to the slightly long (1000ms) exposure time at which mixed results for dot-probe studies have emerged previously, or due to the validity of an explanation such as the contemporary "attend then avoid" hypothesis for anxious individuals considered elsewhere in this review (and which brings an element of doubt to certain prior dot-probe studies). Whilst this latter explanation has been supported directly with some eye tracking work, for example in Wieser, Pauli, Weyers, Alpers and Muhlberger's (2008) study into initial orientation and subsequent avoidance, more recent findings bring it (or at least its cross-method presence) into question. A 33-study meta-analytic review of eye tracking in affective disorders (Armstrong & Olatunji, 2012) suggested that anxious individuals tend to initially orient and fixate on threatening stimuli across the reviewed tasks, and had *difficulty disengaging* from threat on certain tasks. As such, whilst there is ongoing debate and investigation into its specific nature, overall the results from a multitude of studies have generally been interpreted as providing strong evidence for a distinctive pattern of performance by anxious individuals on tests of attention (Murphy & Isaacowitz, 2008). Biased cognition thus appears to have a presence in perceptual fundamentals like attention. In accordance with the interconnectivity emphasised in models such as those of Bower (1981) and White, Suway, Pine, Bar-Haim and Fox (2011) however, researchers have also begun to explore more mid-level processes involving the integration of perceptual data to form predictions of outcomes, and have sought evidence of biases in the interpretation of sensory data.

Specific variants of cognitive bias: biased interpretation. Interpretational processing has been a salient further candidate for cognitive biases. This is because it is potentially key to much of an individual's emotional processing, being the appraisal of sensory data, and is thus central in determining physiological response, extent of conscious awareness of the phenomenon and overt behavioural outcomes (Power & Dalgleish, 1999).

In terms of an operational definition, biased interpretation (in the sense of the studies contained in this review) refers to a tendency for an individual to make negative interpretations of ambiguous information. In the case of anxiety, this denotes a general and pervasive tendency to make negative, threatening conclusions from ambiguous stimuli. As Mathews and MacLeod (1998) detail in their modelling of social anxiety, this is likely to involve socially threatening and socially benign interpretations competing for resources with the threatening ones generally winning due to greater accessibility.

A number of procedures designed to provide analogues of the real-world situations of threat ambiguity that could be experienced by anxious individuals have been developed. In homophone-based procedures for investigating interpretive bias participants listen to a series of words. Amongst these are certain homophones: sounds which can be interpreted as two different words, one of which has a threat meaning (for example "pain") and one of which has a non-threat meaning (for example "pane"). These are mixed with non-homophonic threat and non-homophonic non-threat filler words and participants are asked to write down the words as they hear them. Findings show that participants with high anxiety show a greater tendency to write down the threat meaning of a homophone than low-anxious participants (Mathews, Richards and Eysenck, 1989; Eysenck, Mogg, May Richards & Mathews, 1991; Richards & French, 1992). Mathews, Richards and Eysenck (1989) provide the additional important finding that when reading non-homophonic threat words, clinically anxious participants exhibited significantly greater galvanic skin conductance, evidence of an anxious physiological reaction to this material and the link between interpretation, threat detection and this emotion.

A further method of measuring interpretive bias and its association with anxiety is the scrambled sentence task, a concept initially devised in depression research by Wenzlaff & Bates (1998). In the anxiety-focused version of this task (e.g. Standage, Ashwin & Fox, 2010) participants receive sets of six words, and are asked to select five of these words and arrange them into a coherent sentence. The nature of the set of words is such that only two sentences can be formed from them, one with a positive meaning and one with a negative meaning, tailored to the type of anxiety in question. As such a participant with social anxiety might receive the words “others talking hard easy is with” and be able to rearrange it into “talking with others is easy” or “talking with others is hard”. Participants high on anxiety have been found to create significantly more sentences with a negative implication than low-anxious participants.

Other researchers have adopted recognition tasks to measure interpretive biases. In such a task, participants typically receive a set of ambiguous stories or sentences (e.g. Butler & Mathews, 1983; Eysenck, Mogg, May Richards & Mathews, 1991; Amir, Foa & Coles, 1998). Subsequently, different interpretations of each story are presented to them, of varying valence, and they are asked to indicate how similar each is to the material they initially read. As such, an index of bias can be gained by the extent to which they rate positive and negative material as more or less similar to the initial ambiguous stories. Salemink and Van den Hout (2010) detail the validation of this measure and summarise the contemporary research that has used it.

Alternatively, Beard and Amir (2009) devised a word-sentence association procedure for the measurement of interpretive bias. Participants are exposed to multiple computerised trials. In each trial, a central fixation cross is displayed initially to direct attention to the centre of the screen. This is followed by either a positive or negative word, for example “funny” or “embarrassing” respectively. After this a sentence is displayed such as “people laugh at something you said”. The participant is then asked to make a categorical yes/no judgement, via a key-press, as to whether the word and sentence are related. Over multiple trials, the extent to which they indicate that the ambiguous sentences are related to the threatening and nonthreatening words is taken as an index of interpretive bias.

Calvo, Eysenck and Estevez (1994) developed an early and less-used interpretive bias measure in which they looked at the interaction of interpretation and attentional disengagement. They exposed participants to ambiguously-ending sentences concerning “ego” threats, physical threats and nonthreatening events. These were followed by either a word that confirmed or disconfirmed the threat, or a very similar non-word. Participants were asked to make judgements regarding these as legitimate words. High anxiety participants took longer to respond to ego threat confirming non-words, and ego threat disconfirming words, and responded faster to ego threat disconfirming non-words.

Huppert, Pasupuleti, Foa and Mathews (2007) attempted to create a more comprehensive measure of interpretive bias. In the task they created, participants read and responded to a series of sentences describing social situations which were missing a crucial end word for their resolution. Participants were asked to generate as many words as they could to end each sentence, and to place an asterisk next to the word that best completed each sentence. From this, the authors were able to extract data on total responses (of different valences), valence of first responses provided and valence of those responses endorsed as best completing their sentence. This study is currently the only one to attempt such measurement in an adult population however, with other open response work only taking place with child and adolescent populations (e.g. Dodd, Stuifzand, Morris & Hudson, 2015).

Other associated biases. Whilst attention and interpretation have been the focus of the majority of research into the processing biases associated with anxiety, a number of other elements of processing (albeit often with substantial linkage and overlap with these key perceptual processes) have been explored to varying extents for the presence of anxiety-relevant bias.

A substantial amount of work has investigated memory biases. A variety of tasks have been used here. These include tests of both implicit and explicit memory, including explicit cued recall tasks (which might typically ask participants to recall as many items as possible from a list of words containing both threat and non-threat content, and then assess the ratio of these) and recognition tasks (which might ask participants to judge whether they had encountered each of a series of words at a prior phase in the experimental procedure). Mitte (2008) provides a review of memory biases and their relationship with anxiety and anxiety disorders across an array of different experimental paradigms, concluding that the only area of memory containing a bias with a consistent association with anxiety is recall, and that even this is dependent upon aspects of experimental design such as encoding procedure or retention interval. Mathews and MacLeod (2005) had previously reached similar conclusions in an earlier review. They suggest that the current pattern of results is an artefact of the particular experimental designs used and cite evidence that high-anxious and low-anxious individuals encode differently, in support of their suggestion that it is necessary to allow flexibility in encoding in procedures investigating memory (in order to observe a significant between groups effect). They note that when this has been allowed, significant differences between controls and both high trait anxiety and general anxiety disorder participants have been observed. What is important about Mathews and MacLeod's (2005) account is that it suggests that memory bias may exist at the encoding stage of information processing rather than retrieval. A bias in encoding is conceptually appealing because encoding is a process tied greatly to interpretation and attention, in which there is already strong evidence for distinct biases. More research however, with careful design, is necessary here.

Another process linked to interpretation (and a candidate for a further aspect of processing with a causal link to emotional states) is association. Here some evidence of bias has been found in high-anxiety individuals. Implicit association tests (IAT) (Greenwald, McGhee & Schwartz, 1998) are the primary method of measurement here, and involve participants making a series of classifications of presented stimuli as falling under one of two polar adjectives, in order to elicit their implicit associations. A participant might for example see a word in the middle of the screen like *happiness* and be asked to classify it initially as *pleasant* or *unpleasant*. As the procedure progresses, these pairs of adjectives have a second pair added, so that at one side of their screen participants might see *black/pleasant* and on the other side *white/unpleasant*. These pairings are then reversed, with participant responses being continuously recorded. The logic behind the test is that participant responses will be easier (i.e. faster response times) when they have two words on the same side of the screen that are cognitively concordant for them, allowing for easier and thus faster classification. IATs have been found to have predictive utility for anxiety (Egloff & Schmukle, 2002). In addition, scores on the IAT have been found to change post-treatment in clinically anxious individuals (Gamer, Schmukle, Krausgrill & Egloff, 2008), and have been applied in the assessment of phobias (e.g. Teachman & Woody, 2003) and social anxiety (Tanner, Stopa & De Houwer, 2006), with the latter finding a general bias for associating positive words with self and negative words with others. This tendency was significantly weakened in high socially anxious participants. This is reminiscent of the evidence of a lack of positive interpretive bias in socially anxious individuals detailed elsewhere in this review, and may even reflect a common mechanism.

Anxious individuals' assessment of covariation has also been explored for a bias. A covariation bias is a tendency to over-interpret correlation between particular kinds of events and occurrences (and potentially thus attribute an accompanying causal association also). In the case of anxiety this is a tendency for anxious individuals to overestimate the co-occurrence of threatening information and negative outcomes. Covariation bias has been evidenced in both adult and child samples, indicating an influential developmental role. In Muris, Huijding, Mayer, Dan Breejen and Makkelie's (2007) study for example nonclinical highly fearful children overestimated the co-occurrence of negative outcomes with threat pictures such as spiders, and underestimated the co-occurrence of these threat pictures with positive outcomes, compared to low-anxious children. In adults, general and social anxiety are yet to receive direct investigation, but studies have found evidence for a covariation bias in a variety of applications to adults with specific phobias, including contamination (Connolly, Lohr, Olatunji, Hahn & Williams, 2009), dangerous animals (Kennedy, Rapee & Mazurski, 1997) and flight phobia (Pauli, Wiedemann & Montoya, 1998) and to participants with panic disorder (Amrhein, Pauli, Dengler & Wiedemann, 2005). In addition, at the theoretical level such a covariation bias is highly concordant with the theoretical models explored earlier in this review, in particular it is possible that they will encounter a single piece of threatening information (such as witnessing some smoke) and use this as a basis to extrapolate an entire disastrous conclusion (like the occurrence of a catastrophic fire), and thus maintain their anxiety or phobia, as De Jong and Merckelbach (1991) suggest.

Potentially related to covariation bias is the notion of a probability bias in anxious individuals. This denotes a tendency to over-estimate the likelihood of negative events occurring in the future. Studies here focus on having participants estimate the co-occurrence of fear-relevant and fear-irrelevant stimuli with aversive outcomes (e.g. Pauli, 1998). Again this is theoretically concordant with the previously outlined models. There is evidence for a developmental presence, with child studies (Muris & Van Heiden, 2006) and adult studies (reviewed by Nelson, Lickel, Sy, Dixon and Deacon, 2010) supporting the idea of biased approximation of probability in anxious participants. Furthermore, in addition to a probability bias in terms of the likelihood of occurrence of negative events, there may also be a "cost" bias through which anxious individuals also overestimate the potential negative effects of negative events (Shirotsuki, Sasagawa & Nomura, 2010). What is essentially the same concept has also been termed a "catastrophisation bias" by others and investigated as such by other researchers (Hinrichsen & Clark, 2003).

Lastly, there is evidence (from child-focused research only at this point) that anxious individuals may exhibit a "reduced evidence for danger" bias: a tendency to use less information to come to a threatening conclusion in a situation of ambiguity than an individual with low trait anxiety, as a further facet of their sensitivity to threat. Researchers have used story methods to evidence this bias, reading children narratives and asking them to indicate as quickly as possible whether the story was "scary" (see Muris, Merckelback & Damsa, 2000, for exemplary research using this paradigm). The child participants with high social anxiety needed significantly fewer sentences of the story to designate it as scary than control participants without high social anxiety. However to date there is no published research, to the reviewer's knowledge, that directly assesses this bias in anxious adults, although there is a well-documented presence of a similar but broader "jumping to conclusion" bias in schizophrenia research with adults (see Fine, Gardner, Craigie & Gold, 2007).

Biases over a lifetime in anxiety: a brief note on the developmental perspective. Whilst this review is concerned primarily with cognitive bias in adult populations, it is worth mentioning briefly that these do emerge from a developmental context, and that whilst generally considered a relatively "trait" element of human cognition, it is nonetheless possible for them to change over time in response to varying conditions. Field and Lester (2010) for example, in a review of the relevant developmental literature compare the static "integral" model of invariant, static biases over time with two developmental models (a responsive "moderation" model and an "acquisition" model). They conclude in favour of the latter two and of, in general, not simplifying our conception of cognitive bias to an invariant and inherent trait, but as a complex combination of often situationally-responsive processes.

Anxiety and biases: a summary. There is emerging evidence for a host of biases in the information processing of anxious individuals, and these have been evidenced via a considerable array of innovative quantitative methods. Furthermore, it is conceivable that these may in fact all draw on similar cognitive mechanisms. They may even function in the highly linked (spreading activation) manner that Bower's (1981) associative network depiction suggests, with a deviation in just one process leading to a cascade of future negative priming in the information processing of new situations of ambiguity. In this way, individuals with anxiety may experience profound difficulties removing themselves from a considerable host of contributory cognitive processes that maintain their maladaptive state.

Biases in Clinical and Subclinical Social Anxiety: Current Evidence

One key and robust notion from the evidence and theory supporting cognitive bias research is that the displayed biases are highly specific to the type of emotion or emotional disorder that the participant experiences. As such the cognitive biases of participants with post-traumatic stress disorder would appear very different, in terms of the type of information that they focus on, to those of participants with unipolar depression (Mathews & MacLeod, 2005). Accordingly the materials used in studies must be tailored to a specific type of dysfunctional emotional processing, and it is often necessary for researchers to focus their efforts on a single disorder or vulnerability and then extend and adapt this work to other types of emotional dysfunction once this is established (In-Albon, Klein, Rinck, Becker & Schneider, 2008).

Social anxiety is a particularly viable candidate for further cognitive bias-focused investigation. Aside from being a prevalent, potentially underdiagnosed (Den Boer, 1997), and highly debilitating form of anxiety (Schneier, Heckelman, Garfinkel & Campeas, 1994), elements which alone merit investigation, it currently has a reasonable existing body of theory that suggests that it is associated with several key cognitive biases. Accordingly there is a considerable and expanding array of empirical work that supports this perspective. For example at the general level, functional magnetic resonance imaging work (Shah, Klumpp, Angstadt, Nathan & Phan, 2009; Hahn et al., 2011) has indicated a unique profile of amygdala and insula reactivity in socially anxious individuals when they process social threat images. Furthermore, a further benefit of investigating social anxiety is that it appears that continuum-based modelling of both social anxiety and the biases associated with it is appropriate (Stopa & Clark, 2001) as measurable levels exist in the nonclinical population. This has the practical advantage of allowing such samples to be used as nonclinical analogues alongside clinical investigations and implications from each strand of investigation to have at least partial relevance to the other.

Specific evidence for biased social anxiety: biased attention. A social anxiety-associated attentional bias has received considerable research attention. Initial studies, which primarily focused on the use of word-based procedures (emotional Stroop and dot-probe paradigms primarily), provided mixed results. Evidence for (e.g. Asmundson & Stein, 1994) and against (e.g. Horenstein & Segui, 1994) a bias emerged from early work. This lack of clarity continued into the early adoption of procedures with faces. Some authors found no evidence for a bias (Bradley et al., 1997) whilst others found evidence for a bias only under conditions of induced social threat (Mansell, Clark, Ehlers & Chen, 1999). Others still found differing evidence for the nature of the bias implied by their findings: attention towards threatening stimuli (Gilboa Schectman et al., 1999) and attention away from stimuli (e.g. Yuen, 1994) have both been evidenced. Pishyar, Harris and Menzies (2004) suggest that much of these prior inconsistencies are artefacts of varying experimental designs: key variations such as the control stimuli used, use of clinical vs subclinical participants and the exposure time applied do occur between the aforementioned studies. They did however not address these flaws entirely but went on to compare word and face-based dot-probe procedures, and additionally attempted to create a more ecologically valid dot-probe measure by having pairs of faces looking at one another, and the inclusion of the participant's own face in certain trials. For the former, they found that high socially anxious participants displayed an attentional bias towards threatening stimuli on a face-based task, and this was effect was replicated in a second experiment. They found no effect when words were used. Whether the participant's face was used in a trial or not did not exert a significant main effect. The findings favouring words over pictorial stimuli differ from the wider anxiety literature, which indicates no significant difference between the two overall (Bar-Haim, Lamy, Pergamin, Bakermans-Kranenburg & Van Ijzendoorn, 2007). As such this may be a particular specificity of the bias associated with social anxiety.

To return briefly to the contrasting patterns of results gained in some of the earlier social anxiety attentional bias investigations, a popular alternative or supplementary explanation to Pishyar, Harris and Menzies' (2004) is that socially anxious individuals may display an "attend then avoid" pattern of attention (Mogg & Bradley, 2002; Garner, Mogg & Bradley, 2006; Machado-de-Souza et al., 2010). The adoption of eye tracking in experimental paradigms has assisted the assessment of this hypothesis and there is initial evidence indicating that this may be the case in subclinical populations. Individuals high on a social anxiety measure have been found to look significantly more at emotional faces than neutral faces in an initial second of exposure but significantly less in the subsequent half-second (Wieser, Pauli, Weyers, Alpers & Mühlberger, 2009). Two elements are necessary to consider these findings in context. Firstly it is notable that these were emotional faces *per se* and not just negative ones. However evidence from further studies helps to put this apparently slightly discordant finding in context: socially anxious individuals have been found to rate all valences of scenarios that they read as significantly more negative than controls and to rate all kinds of faces as significantly less approachable (Campbell et al., 2009). These findings may explain the prior parity. They do not, in themselves, account for the preference of emotional over neutral faces. However when it is borne in mind that every emotional face was presented alongside a consistently neutral one it is likely that (when offered repeated pairs of emotional vs. neutral faces) the emotional ones in general quickly become more salient, particularly when factors such as the fact that the emotion changes from pair to pair are considered. The second element of context is the fact that, in the wider anxiety literature considered earlier in this chapter, despite some findings in support of an attend-then-avoid hypothesis, the overall key findings for the field currently support early fixation and difficulty disengaging.

Continuing with the use of eye tracking to explore further the attentional differences present in social anxiety, the recent literature on eye gaze should receive brief mention (for the clarifying, yet also complicating, role it has played in recent conceptions of the differences between high socially anxious individuals and controls). Gaze direction of viewed faces used may moderate a tendency for avoidance for example (Roelofs, Putman, Schouten, Lange, Volman & Rinck, 2010), although this avoidance was not attentional avoidance of the kind implemented in the prior reviewed tasks but instead involved participants pulling or pushing a joystick to enlarge or shrink the face stimuli used. This kind of avoidance, whilst it has received considerable research interest recently, is arguably a little beyond the scope of consideration of this review. Whilst it may affect what an individual with social anxiety attends to it is arguably a behavioural output rather than a distortion of the processing of sensory input. It may however be a key moderator when translating attentional bias findings for social anxiety into real world applications.

To summarise, whilst clarification remains necessary in some areas, recent review (Bar-Haim, Dominique, Pergamin, Bakermans-Kranenburg and Van Ijzendoorn, 2007) provides strong evidence for a distinctive pattern of performance in tests of attention associated with socially anxious participants. The authors report a significant association (with an overall effect size of d = .45) of attentional bias and social anxiety across studies.

Specific evidence for biases in social anxiety: biased interpretation. The interpretive bias associated with social anxiety has been investigated in several studies. Amir, Foa and Coles (1998) presented individuals with a clinical diagnosis of social anxiety with an instrument in which they were asked to rank three preset explanations for each of a set of social scenarios that they read. One of these preset responses was negative, one neutral and one positive. Clinically socially anxious individuals tended to choose negative interpretations as their first choice significantly more than clinically obsessive compulsive individuals and nonclinical controls, and only when making self-relevant judgments as opposed to ones about people in general.

Constans, Penn, Ihen and Hope (1999) had nonclinical participants read a vignette of a social scenario containing several ambiguous moments (at which positive and negative interpretations of what was being described were possible). They then completed both a questionnaire regarding their interpretation of what had taken place and rated their agreement with a series of preset statements about the events. Participants high in social anxiety showed significantly more socially negative interpretations on these measures than low anxiety participants. Stopa and Clark (2000) used both an open-response measure and a preset answer-based task with a clinically socially anxious sample. This was in order to assess both the tendency to generate negativity in the face of ambiguity and the tendency to catastrophise from mildly negative information. For the former, participants read and responded to ambiguous social and non-social material. Clinically socially anxious individuals were more likely to produce negative interpretations of the ambiguous social material only, suggesting specificity of the interpretive bias to social information. The tendency to catastrophise from moderately negative social information in clinically socially anxious participants was also supported: they rated extremely negative explanations (for example “I'm a boring person”) of a set of unambiguous mildly negative social scenarios (for example “You've been talking to someone for a while and it becomes clear that they're not really interested in what you're saying”) as more likely to spring to mind than participants in the nonclinical control group.

The interpretation bias associated with social anxiety has been found to be specific to social situations (Foa, Franklin, Perry & Herbert, 1996; McManus, Clark & Hackmann, 2000). Within this however, participants in the clinical sample were more likely to rate scenarios of all kinds of valence, from positive (receiving a compliment) and neutral (someone looking in their direction) to mildly (a newly introduced person not speaking to them) and strongly negative (being informed that a colleague disliked them) as having negative implications for them compared to nonclinical controls. Vassilopolous (2006) replicated both findings with a subclinical sample.

Huppert, Foa, Furr, Filip and Mathews (2003) offered confirmatory evidence that the negative interpretation biases associated with social anxiety were associated with the social anxiety itself only and not to general negative affect. However Voncken (2007) found that in clinically socially anxious participants, comorbid depressive symptoms appeared to accentuate the existing negative interpretive bias in the socially anxious, when high and low depressive symptom socially anxious individuals were compared with depressive and non-depressive controls.

Further support for and detail regarding the interpretive bias that accompanies social anxiety comes from a small host of further recent studies. Taylor and Alden (2005) evidenced the bias in a potentially more ecologically valid context using a discussion task with a confederate experimenter. Social anxiety patients overall demonstrated a negative bias compared to nonclinical controls in subsequent judgments about their own performance in the discussion. This occurred both when they had been interacting with a prosocial confederate and when the confederate behaved ambiguously.

Huppert et al. (2007) attempted to break down the overall bias displayed in social anxiety in two ways via an open-ended sentence completion task: these were presence of a negative bias vs absence of a positive bias and generation of responses vs selection of a most likely response. Both presence of a negative bias and absence of a positive bias were evidenced in both response generation and response selection. Beard and Amir's (2008) Word-Sentence Association Procedure has been used to evidence interpretive bias in the socially anxious with concordant results: the presence of a negative bias and the absence of a benign bias. Miers, Blote, Bögels and Westenberg (2008) however found evidence only for the presence of a negative bias in a socially anxious adolescent sample, which can be interpreted either as mixed evidence or as a developmental phenomenon: it may be that the absence of a benign bias is not a consistent anxiety-associated phenomenon or that this tends to be acquired later in development by anxious individuals. Huppert, Foa, Furr, Filip and Mathews (2003) had previously hypothesised that the negative bias is the element most associated with social anxiety specifically and the lack of positive bias appears to relate more to general negative affect, an idea that has been supported with further direct work which has accounted for not only social anxiety, but also general trait anxiety and depressive symptomology in the interpretation of ambiguous social material (Amir, Beard & Bower, 2005; Alden, Taylor, Mellings & Laposa, 2008).

In addition to the described differences in their processing of ambiguous social situations and word-sentence associations, several studies have evidenced bias in the interpretation of faces by socially anxious participants. They have been found to rate the approachability of viewed faces (with a variety of expressions) as significantly lower than low-anxious controls (Campbell, Sareen, Stein, Kravetsky, Paulus, Hassard & Reiss, 2009), and to be able to judge certain inconsistent combinations of smiling faces with a threatening element (angry, fearful or disgusted) as not happy faster than a control group. Consistent results with the former were found by Stevens, Gerlach and Rist (2008), who also reported that alcohol consumption appeared to partially ameliorate this bias in terms of the ratings of negative faces by alcohol-consuming participants in both socially anxious and control groups, compared to non-alcohol consuming controls. Jusyte and Schonenberg (2013) however gained null results when comparing clinically socially anxious individuals and non-anxious controls on interpretation of faces in a facial morphing procedure. In further investigation of approach behaviour by Lange, Keijsers, Becker and Rinck (2008) participants were presented with crowds of mixed neutral and angry or happy and angry faces on a computer screen and tasked them with either pulling this display towards themselves and enlarging it or pushing it further away and thus shrinking it with a joystick. Socially anxious participants were found to avoid the angry/neutral crowd and accelerate it away from themselves faster when the proportion of angry faces in the angry/neutral crowd increased and to avoid the happy/angry crowd regardless of proportion. This suggests an interpretation of angry faces as significantly more salient and threatening in these participants.

A linked interpretive bias (or at least a process contributing to overall interpretive bias) has emerged recently in eye tracking studies: individuals with high social anxiety may perceive others to be looking at them more often than they actually are, and this may increase further when the looker does not exhibit a positive facial expression (Jun, Mareschal, Clifford & Dadds, 2013: Schulze, Lobmaier, Arnold & Renneburg, 2013). This becomes interesting when it is placed in the context of the previously mentioned tendency for increased avoidance behaviour when socially anxious individuals perceive a face is looking at them. A socially anxious individual might perceive individuals as looking at them more often, increase their avoidance behaviours as a result, and then create a further bias in what they attend to and interpret as a result. Future research should substantiate and clarify these potential new processes for bias maintenance further, and perhaps extend the current models of bias to account for them.

The Modification of Biases

The need for modification. In order to conceptually clarify their role, the necessity of assessing the potential causal role of cognitive biases became salient. Accordingly, a first step was to attempt to manipulate the biases under controlled experimental conditions. As such, researchers began to explore the potential of the bias measurement techniques that existed and hypothesised that if appropriate principles were applied the procedures might be able to be adapted to exert change on the biases that they had previously measured. Hertel and MacLeod (2012) outline these principles. They acknowledge how associative learning was applied in order to automatise responses in participants. Participants implicitly learn rules for responding to emotional content: as such whilst they may be aware of the tasks they are completing in the procedures and some researchers are beginning to experiment with giving them explicit instructions for how to make interpretations to enhance the effects gained, the basic procedures themselves do not demand reflective, executive capacity from the participant. Hertel and MacLeod (2012) note how a focus on associative learning was combined with a general focus on transfer-appropriate processing and how different tasks may draw on similar cognitive processes and faculties, such that change in how one task is processed may exert change on others via a shift in the underlying process (note that the idea of a “near-far” continuum is prevalent here, and returned to later in the consideration of near-far transfer effects). Most early attempts at modifying biases thus cautiously used modified versions of measurement tasks to achieve “near” transfer before the range of measurement was expanded to consider what else the task might modify. Accordingly with these basic characteristics a number of modifications and innovations around key measurement techniques were made in order to experiment with the causality surrounding cognitive biases in anxiety

Types of training task for cognitive bias modification for attention (CBM-A). For the application of visual search tasks to bias modification, the adaptation to modification from measurement involves merely altering the content of the task. Participants are shown a display featuring many faces and are instructed to search for a particular type of face concordant with the bias to be induced. Repeated practice of this task with positive faces and with negative faces results in the induction of the respective bias (e.g. Dandenau, Baldwin, Baccus, Sakellaropoulo & Pruessner, 2007), as evidenced by self-report stress and physiological measurement of cortisol levels, relative to a control group instructed to search for certain benignly-valenced stimuli.

Dot-probe tasks have been the most prevalent form of attentional bias modification applied in empirical research, with at least sixteen published studies demonstrating their application since the first documented application of this technique by MacLeod, Rutherford, Campbell, Ebsworth and Holker (2002). In these tasks, the participant completes a dot-probe procedure in the same way that they would if it were a measurement task, but the probe consistently replaces the non-threat word rather than the threat word. In this way, via the principles described above, the participant is trained in the shifting of attention towards the positive stimulus in order to quickly ascertain and indicate the probe's location. Overall, the anxiety-alleviating effects of this apparently simple training (see below for further detail on this evidence base) have been repeatedly demonstrated (Browning, Holmes & Harmer, 2010).

Types of training task for cognitive bias modification for interpretation (CBM-I). The key procedures for modifying biases in interpretation appear to rely slightly less on fast, on-line processing, as (like the interpretive bias measurement procedures that exist) they involve the participant resolving situations of ambiguity, which usually involves at least the comprehension of a sentence. Scenario-based interpretation modification, first conceived by Mathews and Mackintosh (2000) is used frequently in this literature, and involves participants completing a computer program in which they read scenarios, each of which has an incomplete end word (a fragment with some missing letters). Participants are asked to press a button on their keyboard corresponding to the first letter of this missing word. They are constrained in doing so in a way that means the only coherent response they can give is to indicate that the partially complete word is a positive one that will bring a positive resolution to the scenario. In this way, via repeated practice, the participant is trained in the resolution of ambiguous information in a positive manner. As an example of such a scenario a participant might read the following:

You invite a variety of family and friends to your house for your birthday party. They are quite different and you know that some of them don't always get on. As you are clearing up afterwards you think that getting all these people together made the evening

i—eresti--

They would be asked to provide the first missing letter of this fragment, and for a grammatically correct word would have no choice but to answer "n". A confirmation question would follow such as: *"are you pleased that you invited everyone to the party?"* with a choice of yes or no responses, before the next item is initiated.

Grey and Mathews (2000) devised a further ambiguity resolution procedure that, whilst less applied overall in the literature, appears to also be able to exert change on interepretive bias. In this they replace the entire ambiguous scenario characteristic of Mathews and Mackintosh's (2000) procedure with a single homograph of ambiguity: a word which can be interpreted in a negative or a benign manner. Each is then followed by a word fragment as per Mathews and Mackintosh (2000) that can only be completed in one way. For the homograph component for example, the word *sink* might imply a benign kitchen item to an individual lacking an interpretive bias but have connotations of drowning to a highly anxious individual, but the idea is that when a participant receives and resolves the subsequent paired homograph *wa-h* to create the word *wash* (as opposed to resolving dro-n to drown in a negative condition) they also bring about the benign association in the process.

The word-sentence association procedure developed by Amir and Beard (2008) has also been applied to modify as well as measure interpretive bias. Participants see pairs of one sentence and one word as in the measurement program. They are asked to simply indicate whether the word and sentence are related by pressing keys corresponding to yes or no on their computer keyboard. Upon receiving each response, the program differs from the measurement variation by providing the participant with response-contingent feedback. As such if the participant, when being positively trained, indicates that a word and sentence with a negative implication are associated or that a word and sentence with a positive implication are unrelated, the program issues them with the response "you are incorrect". Similarly they receive a "you are correct" response every time they either make an association between a positive word-sentence pair, or indicated a lack of association between a negative pair. In this way, participants need not become directly aware of the goal of training as per the preceding measures of interpretive bias modification.

Cognitive Bias Modification: A brief summary of evidence for the use of existing techniques.

Once basic techniques for the modification of anxiety-relevant cognitive biases had been established, a number of objectives became paramount in the research base. Firstly many researchers concerned themselves with further substantiating the claims that these techniques had causal efficacy on biases under a variety of circumstances. Secondly, as Hertel and MacLeod (2012) note, it was necessary to establish what causal role the biases might play by looking at "far" transfer: both to check that training effects could be produced on measurement tasks that were more distinct from the training task, and to ensure that a modification in a bias produced a corresponding effect on the emotion experienced. Thirdly, and highly linked to the prior two objectives, was a need to see whether CBM techniques might, if they proved reliable methods of influencing bias and emotion, actually translate into valid clinical interventions (in practice, a procedurally diverse array of experimental work often attempted to consider two or more of these objectives at once). Additionally at a conceptual level underpinning these objectives was a need to clarify what processes were involved, but this has been relatively under-investigated until recently (and is dealt with specifically in more detail in the subsequent chapter of this review).

The ability of the techniques developed to exert change in attentional bias was further substantiated by a host of studies. Key examples include work by Amir, Weber, Beard, Bomyea and Taylor (2008) who successfully trained attention away from threat in socially anxious individuals using dot-probe methods. They were able to so by consistently causing the sought probe to replace a benign face: over time this was found to exert implicit attentional retraining. Dandeneau and Baldwin (2004) used a visual search task modification task and found the process of searching for happy faces in a matrix of threatening ones to bring about a significant increase on positivity of bias as measured by an emotional Stroop task incorporating threat words focused around the theme of interpersonal rejection. The evidence base as a whole for this has received considerable recent review attention, with Beard, Sawyer and Hofmann (2012) finding a moderate and robust effect size overall for modifying attention to threatening stimuli across a variety of training paradigms and measurement tasks.

Many such studies accounted for emotional change also and found evidence for this in a variety of ways. Some have incorporated highly applied emotional stressors in attempts to increase the ecological validity of their assessment. Amir, Weber, Beard, Bomyea, & Taylor (2008) for example reduced self-report anxiety in socially anxious participants in relation to a subsequent public speaking task by training attention away from threat. See, MacLeod & Bridle (2009) meanwhile gave Singaporean high school students similar training over multiple sessions, with results indicating an apparent reduction in both trait anxiety and state anxiety in response to the real-world stressor of moving abroad for tertiary education. The modification of attentional bias and the modification of accompanying emotion do not always co-occur however as some null results have been gained on affective indices when positive results for change in bias have been recorded (e.g. Van Bockstaele, Verschuere, Koster, Tibboel, De Houwer & Crombez, 2011). This may be a result of the measurement methods used, or may indicate that greater investigation of the relevant mechanisms is necessary as feasibly different emotional states and disorders appear to be best represented with distinct theoretical models and as such may feature a more or less critical role for cognitive biases. Overall however, recent review indicates a medium-sized effect of CBM-A on anxiety. Whilst Hakamata et al. (2010) found an overall Cohen's d of .61, Beard, Sawyer and Hofman (2012) note that under their criteria this varies from 0.03 to 0.6 and that it appears to be moderated by a number of factors of design heterogeneity including stimulus type and number of sessions (and importantly also design elements like stimulus orientation – an important consideration). It should be noted that whilst of some relevance the latter review was a broad one, including not just anxiety-relevant studies using threatening stimuli but also several studies retraining attention to appetitive stimuli (e.g. in populations with addictions) so the implications in terms of this review should be considered with this caveat in mind.

For interpretive bias, following Mathews and Mackintosh's (2000) promising proof-of-concept results using a scenario completion program, a host of studies evidenced the modifiability of bias and emotional reactivity in this way (e.g. Salemink et al., 2007; 2009). Further studies using homographs (e.g. Mathews, Ridgeway, Cook & Yiend, 2007; Hoppitt, Mathews, Yiend & Mackintosh, 2010b), and using a word-sentence association procedure (e.g. Beard and Amir, 2008; MacDonald, Koerner & Antony, 2013) have emerged. This triangulation helps to verify CBM-I as not a phenomenon of a particular experimental setup (or at least if it is, then it is at least a cross-design phenomenon perhaps rooted in the compatibility between lexically-based training and testing procedures). Beard (2011) summarises the supporting evidence for the modifiability of anxiety-related interpretive biases via the above techniques in a (non-systematic) literature review, with favourable conclusions rooted in the comparable effect sizes to existing treatments gained, and the relative ease of implementation. Subsequent to her review a further host of studies have emerged with supportive results (e.g. Beard, Weisberg and Primack, 2011; Vassilopolous, Blackwell, Moberly & Karahaliou, 2012). Cristea, Kok and Cuijpers (2015) however provide cautionary evidence in their more recent and systematic meta-analysis. For anxiety in general, an effect size of g = .37 was gained, which with outlier removal decreased to g = .27. Such figures for a Hedges g statistic are conventionally interpreted as representing a small effect size. Social anxiety provided a higher effect size (g = .4) with unadjusted results, which with outlier removal again declined to g = .23. Inclusion of studies using clinical samples only reduced these further. Two elements are key to the further contextualisation of these results. Foremost is the high heterogeneity of the constituent studies (highlighted by the authors themselves) and the difficulty in drawing consistent implications from a field with such considerable diversity of method (particularly in methods of modification and measurement in the designs). Secondly, the analysis focuses on emotion outcomes and not on the ability of the training to exert effects on the bias itself, which remains quantitatively unaccounted-for. These points notwithstanding however, the results do urge caution with CBM-I findings and in particular the rush to clinical application. They certainly cast doubt on the ability of CBM-I interventions in their current state to perform as a reliable clinical intervention. Furthermore the findings regarding heterogeneity of method, and the indicated presence of significant publication bias that accompanied the results are significant cause for concern with the field as a whole, regardless of the outcomes focused on.

Cognitive Bias Modification for Social Anxiety: The State of the Evidence Base in Clinical and Subclinical samples

A few existing CBM-A studies have targeted social anxiety specifically. In terms of single-session designs, Dandeneau, Baldwin, Baccus, Sakellaropoulo and Pruessner (2007) report a series of studies in which they reduced stress reactivity (measured via cortisol and self-report measurement) in two laboratory and two real world stressor contexts (high performance-pressure scenarios with samples of students undergoing examinations and professional phone salespeople). Amir, Weber, Beard, Bomyea and Taylor (2008) reduced attentional bias and anxiety in response to a public speaking task in nonclinical high social anxiety participants.

CBM-A has been applied in several studies with patients meeting the criteria for a clinical diagnosis of general social anxiety. In multi-session, dot-probe based trials with these clinical patients, significant reductions both in displayed bias and the number of participants meeting the criteria for diagnosis by the end of the experiment were observed (Schmidt, Richey, Buckner & Timpano, 2008; Amir, Beard, Burns & Bomyea, 2009; Amir et al., 2009). Li Tan, Qian and Liu (2008) report a mixed outcome from a 7 day program of CBM-A however, reporting significant change in only one of three social anxiety self-report measures issued. Amir, Taylor and Donohue (2011) attempted to investigate facilitating factors for the success of the treatment (in which considerable individual differences have been observed). Their key findings were that, somewhat unsurprisingly, the greater the initial level of an individual's attentional bias the more they can benefit from the training and, more surprisingly, that ethnicity affected outcomes, with non-Caucasian participants outperforming Caucasians in response to the training, a result which has yet to be followed up with further investigation or substantial justification.

Grey and Mathews (2000) were first to evidence a training effect of CBM-I for social anxiety, over a series of four experiments with single-session homograph procedures and non-selected participants. They successfully induced a significant increase in negative bias in these individuals, using sustained negative resolution (via passive and active completion of valenced word fragments) of ambiguous homographs. Murphy, Hirsch, Mathews, Smith and Clark (2007) used a single session of scenario-based CBM-I, delivered via audio to participants, in which participants listened to a set of ambiguous scenarios. It was found that participants who then listened to the positive resolution of each scenario interpreted new ambiguous situations presented on a computer screen more positively (using a series of preset potential endings) than a control group given unresolved scenarios. It should be noted however that this finding stands in contrast somewhat to the findings of Hoppitt, Mathews, Yiend and Mackintosh, (2010), that active resolution by participants is necessary for a significant training effect of scenario-based CBM-I to occur, as does one of Grey and Mathews' (2000) early homograph experiments in which active resolution was also not found to be necessary. It may be that this was an example of a priming effect, facilitated by similarity of training and testing materials, rather than a full rule-learning bias modification (the following chapter discusses such mechanisms in more detail).

Further evidence with reasonably distal bias measurement procedures (still computer-based lexical tasks but distinct in the specifics of procedure from the training task), which also demonstrates the temporal efficacy of CBM-I procedures, has been provided by Yiend, Mackintosh and Mathews (2005) and Mackintosh, Mathews, Yiend, Ridgeway and Cook (2006). These studies evidenced the longevity and durability of single-session CBM-I effects, providing evidence that these biases endured over a 24-hour period, and through changes in experimental context. Important to note is that these were induced negative biases in non-anxious participants from the general population.

Evidence with clinical populations has also emerged. Murphy, Hirsch, Mathews, Smith and Clark (2007) used single-session scenario-based training to impart a benign interpretation bias in a clinically socially anxious sample, as evidenced by a near transfer recognition task. Beard and Amir (2008) applied their word-sentence association procedure in eight sessions over four weeks, finding a decrease in negative bias (composed of decreased threat associations and increased benign associations) and an alleviation of anxiety symptoms. Turner, Hoppitt, Hodgekin, Mackintosh and Fowler (2011) conducted a single case series study into the effects of single session CBM-I on social anxiety following a psychotic episode, with beneficial effects in three of the six participants (assessed via a recognition task). Elsewhere, four sessions of scenario completion CBM-I with a socially anxious Iranian student sample was found to decrease interpretive bias and social anxiety (Khalili-Torghabeh, Mobini, Salehi Fardadi, Mackintosh & Reynolds, 2012). Five sessions of training reduced negative interpretations and social evaluative fear (but not more general measures of negative affect) in a sample of students anxious about starting university (Hoppitt, Illingworth, MacLeod, Hampshire, Dunn & Mackintosh, 2014).

In terms of elaborations on CBM-I designs, in addition to addressing them separately, some interventions have been trialled that either target both attentional and interpretive biases, in attempts to construct optimally efficacious treatment protocols, or assess both of them as outcome measures following intervention in one. For the former, Beard, Weisberg and Amir (2011) applied dot-probe attentional bias and WSAP interpretive bias modification together in an eight-session randomised controlled trial, finding significant reduction in self-report and behavioural anxiety when completing a speech task following their last session. It should be noted with this study that data relating to bias measures was not provided in this study, so it is not possible to determine whether each or both of the procedures created this effect. Furthermore single-bias control groups were not incorporated so it is difficult to ascertain whether administering the combined bias modification offered any significant advantage over single-bias training. Brosan, Hoppitt, Sillence, Shefer and Mackintosh's (2011) work with a small clinically anxious population does support this however. There is meanwhile evidence that (at least in terms of altering both biases) it may not be necessary, in the form of studies which suggest that intervening in one bias may produce considerable transfer effects: Amir, Bomyea and Beard (2010) had previously modified interpretation bias and found some significant effects on attention via dot-probe measures with socially anxious individuals, as had Lange et al. (2010) who instead used an automatic avoidance task. Meanwhile White, Suway, Pine, Bar-Haim and Fox (2011) recently produced results which indicate that the reverse transfer from attention to interpretation might be possible when training individuals with high levels of general anxiety. Bowler et al. (2012) conducted four sessions of CBM-I on individuals with high (nonclinical) social anxiety, comparing these with a group who received no training and a group who received an equivalent computerised cognitive behavioural therapy procedure over the same time course. Reductions in social anxiety, trait anxiety, depression and an improvement in attentional control were observed in both active intervention groups, alongside the finding that participants in the CBM-I condition displayed significant reduction in interpretive bias (as measured by a scrambled sentence task) under conditions of cognitive load. Amir and Taylor (2012) evidenced a similar pattern of transfer in a clinically socially anxious sample using word-sentence association-based training.

Furthermore in the wider research base there is evidence of interplay between interpretation and memory (Salemink, Hertel & Mackintosh, 2010; Tran, Hertel & Joorman, 2011) and interpretation and ideation (Hirsch, Clark & Mathews, 2006). In their investigation of the latter Hirsch, Clark and Mathews (2006) emphasise the benefits of not considering biases in isolation and propose a combined cognitive bias hypothesis. White, Suway, Pine, Bar-Haim and Fox (2011) take a similar viewpoint, suggesting that in vulnerable individuals anxiety has a very early effect on information processing which produces a cascade effect. This is a perspective very much concordant with Bower's (1981) associative network model in particular. Due to absence of both direct investigation and accompanying mechanism theorisation it is not yet known whether a reverse effect from interpretation to anxiety is possible or whether this is a unidirectional "bottom up" effect.

In terms of subsequent variations of CBM-I designs (often in attempts to either extend or clarify the contextual efficacy of the procedures further) a number of recent innovations have taken place for CBM-I with socially anxious populations. Scenario-based CBM-I has retained its effect when translated to an audio-based format (Murphy, Hirsch, Mathews, Smith and Clark, 2007), although Standage, Ashwin and Fox (2009), whilst successfully modifying biases in this way in both a positive and negative manner in a moderately socially anxious group, did note a decline in mood not present in a visual CBM-I condition. One further study (Clerkin & Teachman, 2010) mixed verbal and face-based CBM-I studies via a procedure in which participants were trained to pair self-relevant stimuli and positive faces. Self-relevant associations (via an implicit association test) were used as the bias outcome measure, and negative associations were found to be significantly reduced following the training, indicating the interplay between ambiguity resolution and association in social anxiety. The anxiety outcomes for this study were more mixed however, with participants more likely to complete an impromptu speech but not self-reporting significantly decreased anxiety, compared to controls, whilst doing so (as such it may have been the case that participants simply learned a production rule and that this failed to translate adequately to alleviation of emotion). Furthering the investigation of self-relevance, Standage, Harris and Fox (2014) found important initial evidence for individual differences in participants' amenability to CBM-I procedures, with results that suggest participants who assimilated themselves into descriptions of scenarios responded significantly more positively to a CBM-I training program than those who contrasted themselves with each description they were presented with.

CBM-I has also been implemented with accompanying simple explicit instructions to interpret positively in highly (nonclinical) socially anxious individuals (Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt, 2013) and compared with standard CBM-I and a neutral CBM-I dummy program as a control. All three groups also received a single session of computerised cognitive behavioural therapy. The two CBM-I variants were equivalent, both exerting positive change on interpretive bias, attentional bias and reduction of social anxiety symptoms at 1 week follow up.

Summary and Conclusion

The outlined evidence thus suggests the clinical and empirical viability of cognitive biases. It does however note elements of a lack of clarity in the theoretical background, mechanism of effect and efficacy of cognitive bias measurement and modification techniques, particularly those concerning interpretations. The following chapter draws these elements together to provide a series of reflections, extracted implications and areas for further research, the experimental uptake of several of which forms the foundation of this thesis.

Chapter Two: Literature Review Part Two

Reflections on Current Literature, Implications and Design of the Current Project

The prior chapter established the association of cognitive biases with anxiety, their apparent causal role, methods of measurement, and the feasibility and nature of bias-focused interventions in an overview of the state of this unique field. In this chapter, this evidence base will be reflected upon with a slightly more critical emphasis, and its current shortcomings noted. These will then be used to lay the foundations for the current thesis, its research questions and the salient issues behind their conception and investigation.

Pragmatics of Study design: The Focus on Interpretive Biases and Social Anxiety

As detailed in the prior chapter, a number of potentially influential cognitive biases have been evidenced and associated with maladaptive emotionality. Foremost amongst those which have been measured are biases in attention, interpretation and memory. Of these, biases in attention and interpretation have received the most attention from modification as well as measurement studies (whilst memory modification work has taken place in general, little has been systematically applied in the context of cognitive interventions). Furthermore, within these two, attentional bias research is arguably currently benefitting to a greater extent from methodological advances (in particular measurement, both on its own and as an outcome measure for modification work, benefits from the widespread uptake of eye-tracking paradigms). Additionally there is initial evidence that the "cascade" effect of biases affecting other biases may not be unidirectional from bottom up, and that intervening in interpretation may be able to exert influence on attention (Amir, Bomyea & Beard, 2010; Lange et al., 2010). At a theoretical level meanwhile, if we see anxiety as an emotion of potential threat and goal conflict, interpretive appraisal of situations is conceivably very key. As such interpretive biases become the focus of the current project due to having a moderate existing body of measurement and modification literature, but being in need of further conceptual clarification in a number of key areas which are detailed below.

Regarding further focus of the current project, as mentioned briefly in the prior chapter, there is an apparent content specificity to cognitive biases. The current project does aim to have considerable (albeit tentative) transferable wider implications for interpretive bias measurement and modification work in general. However it was nonetheless necessary to select a specific type of content, and a relevant population. A focus on social anxiety (SA) and the biases which accompany it was thus decided upon, partially for this pragmatic reason. In addition further basic practical concerns include the fact that social anxiety has been suggested to be reasonably amenable to bias modification (see pages thirty two to thirty six of the prior chapter in particular). Furthermore, it exists on a continuum, affording the possibility to test large numbers of high yet nonclinical SA individuals available within the general population (which particularly affords reasonable statistical power whilst remaining a suitable close analogue for clinical populations).

Further discussion and reflections on the pragmatics of design of the series of studies contained within this thesis are included in the final chapter. The following section consolidates the key specific observations made regarding the state of the relevant literature, and the responses to these in the current project in terms of research design.

Observations on the State of the Research Base

Observation one: The rush to clinical application with cognitive bias interventions and the need for further conceptual clarity. In reviewing the modification literature for this project, it became clear that in many cases there is considerable enthusiasm for the implementation of CBM-I techniques as clinical interventions. This is of course a valid and worthwhile objective. Particularly in the early phase of the research base there has been good reason to suspect that CBM-I may represent a highly resource-efficient candidate method of implementing clinical change. It may be able to do so as a precursor, replacement (in less severe cases), supplement or subsequent relapse-prevention treatment for full cognitive behavioural therapy with individuals suffering from anxiety.

As an objective however, clinical implementation of CBM-I is arguably an eventual rather than proximate one. A justifiable counter-position of careful implementation in standardised designs, with comprehensive outcome measurement, comprehensive modification program design and conceptual clarification emerged, and became the general standpoint of the current thesis. It was highly formative in the initial design and subsequent refinement of the constituent empirical work, and the various facets of this receive further detail later in this chapter.

Whilst some evidence for the feasibility of current CBM-I techniques as interventions for social anxiety in a clinical context exists, this is a. somewhat minimal and b. when favourable generally suggests equivalence, rather than superiority of CBM-I techniques. Regarding the former, as Cristea, Kok and Cuijpers' (2015) results indicate, CBM-I study designs as a whole are sub-optimal in terms of homogeneity, especially for overall accurate assessment of their feasibility as a mechanism for exerting clinical change. Furthermore cautious replication of key findings is notably absent. For the latter meanwhile, studies such as Bowler, Mackintosh, Dunn, Mathews, Dalgleish and Hoppitt's (2012) have found broad equivalence with similarly issued cognitive behavioural therapy (CBT). Comparison of effect sizes between relevant meta-analytic reviews of CBT (Otte, 2011; Hofmann & Smits, 2008) and CBM (Hallion & Ruscio, 2011; Cristea, Kok & Kuipers, 2015) also suggests this to be the case overall, albeit with considerable variation in estimations of CBM-I effect size depending upon the review protocol adhered to. As such if implemented in its current state CBM-I would be issued more as a resource-effective replacement for, or supplement to, such interventions rather than a true improvement in efficacy. From this perspective the impetus to implement it in its current state may be overly ambitious in one way, but may actually lack ambition in another facet: that concerning what greater change CBM-I might, with further theoretical clarity, methodological adjustment and cautious empirical testing, be able to achieve.

CBM-I is currently administered via extremely simple procedures. As detailed in the prior chapter, participants make repeated, consistently-valenced resolutions to ambiguous short paragraphs or sentences displayed on a computer screen. Overall this simplicity is considered beneficial in that it potentially confers the kind of resource-efficiency mentioned above, and facilitates simple and accessible implementation in a number of therapeutic contexts. It is however worthwhile considering whether some simple further considerations might not be advisable to maximise our knowledge of its operation, our measurement of its outcomes, and eventually its clinical operating efficacy.

The prior chapter mentioned the current paucity of detailed evidence regarding CBM-I's mechanism of operation and discussed briefly the two potential explanations suggested by Mathews and Mackintosh (2002) for the effects gained from existing CBM-I work. It is highly important, especially given the status of CBM-I as a potential clinical intervention, that its mechanism of operation is clarified as much as possible before widespread implementation. Whilst a full investigation of the mechanism of effect of CBM-I is beyond the scope of the current project, it can provide some important clarification to tease apart the two accounts suggested.

The first way in which it has been proposed that CBM-I might operate is via a process of sustained affective priming. The second is via the implementation of a learned production rule (Mathews and Mackintosh 2002). Affective priming might be expected to be more transient, have less longevity but most importantly be subject to further emotional manipulation, being amplified or sustained by subsequent, concordant emotional stimuli and attenuated by incongruent ones. As such it would be of limited use as a clinical intervention. A learned production rule meanwhile would provide an effect that should be independent or at least resistant to subsequent emotional stimuli.

Two ways in which the applicability of these accounts could be (and triangulated) are by comparing CBM-I with an equivalent purely mood-priming condition, and by looking at the resistance of CBM-I to mood priming. These two methods are applied in the third and fourth experiments in this thesis respectively. These are contained in chapter five.

Observation Two: The Limitations of Current Methods of Measurement of Cognitive Biases. Further work on clarifying and improving CBM-I and the quality of evidence for it need not, and arguably should not, be restricted to enhancing the bias modification programs themselves. Measurement studies concerning cognitive biases themselves should also be investigated with depth, scrutiny, experimental replication and as comprehensive theoretical modelling as is possible before broad conclusions are made on the nature and operation of the cognitive biases they aim to provide an indicator of. Whilst a number of bias measurement-focused studies exist, evidencing differences in comparative levels of biases between low and high general population (e.g. Constans, Penn, Ihen & Hope, 1999), or general population and clinical population samples (e.g. Stopa & Clark, 2000), there is little large-sample work. Across the field, bias measurement is currently notably heterogenous, un-standardised and is in a sub-optimal state of validation (Rohrbacher & Reinecke, 2014), making broader conclusions difficult.

In other aspects meanwhile, the measurement studies are generally united. They often take place with simple, closed resolution bias measurement tasks. Furthermore they also tend to involve participants making a single response to each stimulus they encounter. They usually provide a single index of outcomes that is assumed to cover all of the relevant interpretive processing and provide an indicator of overall bias. These are elements which may be sub-optimal for comprehensive bias measurement, both as a standalone investigation of the nature of processing in anxious individuals and importantly as an outcome measure to CBM-I designs.

Fixed-resolution, single-answer and single-index procedures were pragmatic choices for the very early measurement of bias and “proof of concept” CBM-I designs as they are simple, and easy to repeat/implement in experimental designs. There is however cause to consider the expansion of measurement further, especially given the lack of conceptual clarity on a). what constitutes and b). what should be seen as constituting an interpretive bias, as defined in the prior chapter. Such expanded measurement might thus include sub-processes from within what the current overall bias index measures, or might account for processes that are external to what is currently measured. Current procedures focus on a tendency to assume a single negative meaning or outcome when confronted with ambiguity but this is arguably a simplistic portrayal in a number of ways.

Firstly, something that is specifically of high relevance to anxiety-related cognitive biases, is the consideration of multiple possible outcomes. Anxiety, as defined in the prior chapter, is primarily a goal-conflict emotion associated with possible, rather than confirmed threat and thus measurement that takes into account all of the outcomes that a participant perceives as possible, rather than a single forced choice, may be a more representative way of representing the cognitive biases associated with it. Looking at a participants single, first, automatic response may not adequately capture the uncertainty (and cognitive-behavioural indecision) which accompanies anxiety. As such including procedures which allow participants to make multiple responses to single stimuli may allow for a more accurate or encompassing measure of their overall cognitive state when faced with ambiguity, and the presence of potential threat within this.

Further to the above, it is arguably not just participants' responses which should have multiple outcomes, but the output (in terms of bias indices) from interpretive bias measures themselves. This is because overall interpretation, and in particular the effects it is able to have on further cognition, affect and behaviour, has a number of moderating and mitigating aspects. In the prior chapter the evidence for other biases linked to or with some overlap with interpretation was outlined. Arguably some of these processes could be subsumed within interpretation as aspects of it, as they are instrumental in an individual's overall appraisal of the ambiguous scenario in question. They could also be better accounted for as outcomes of interventions like CBM-I. Two of the most salient aspects here are probability bias, and catastrophisation bias.

Probability bias refers to a tendency to estimate negative outcomes as more likely. As such it is linked to, but distinct from, the type of interpretive bias indicated by current measures (which is simply a tendency to generate negative resolutions to ambiguity, and does not explicitly account for the participants' perceived likelihood of them occurring). It is highly theoretically concordant that experienced anxiety might be moderated by perceived likelihood of any threat occurring. Whilst it might still evoke some anxiety, a socially threatening outcome simply generated by an individual (for example falling over at a party in front of many people) would evoke less anxiety if there was a perceived lower chance of this outcome occurring (e.g. the individual expecting an obstacle free environment and themselves to be good at walking) than if there was a high perceived likelihood (e.g. the individual anticipating several difficult staircases that they doubt their ability to navigate). In this way biased or non-biased probability judgment might accentuate or attenuate biased generation of endings to ambiguous scenarios. Current evidence suggests that anxious individuals have such a tendency to over-approximate the likelihood of negative events occurring (Nelson, Lickel, Sy, Dixon & Deacon, 2010). Measures of probability bias have only been applied as outcome measures for CBT at the point of writing of this review. It is conceivable that the bias measured might be amenable to CBM also: at the general level this is concordant with the cascade effects and similar processes indicated by the contemporary cognitive models of anxiety reviewed in the prior chapter (e.g. Bower, 1981; Bar-Haim, Lamy, Pergamin, Bakermans-Kranenburg & Van Ijzendoorn 2007). Transfer of bias from interpretation to attention has been evidenced by Amir, Bomyea and Beard (2010), and probability bias is arguably a lot “nearer” in terms of the relevant cognitive processes to the generation-based interpretive bias that outcome measures currently target, than the latter is to attentional bias. Furthermore if we consider that in CBM-I an individual consistently practices resolving ambiguous situations in a positive manner, it is possible that this practice might lead them to lower their estimations of negative outcomes occurring in future encounters.

The second key component that is potentially able to exert further influence on the overall anxiety outcomes of ambiguous scenarios is catastrophisation bias. This is essentially a bias in the severity of the negatively valenced outcomes generated. To continue the example from the previous paragraph, even if an individual tended to generate negative endings to scenarios, generated several such endings in a particular scenario (such as falling over, knocking a tray of drinks, or saying something inappropriate) and rated these all as highly likely to occur, their overall effect might be further moderated by their perceived severity. For socially anxious individuals this would primarily be composed of the extent to which negative evaluation from others for might be expected in the event of that particular outcome. As such a socially anxious individual might also feature a bias in expecting to be severely judged if they fall over, smash some glasses or say something wrong (Hinrichsen & Clark, 2003). This bias is conceivably more distal from the generation and probability biases explored previously, so possibly less amenable to change from current CBM-I methods. However as a potentially key factor in moderating the anxiety experienced by individuals with generation or probability biases, it is worth considering and accounting for the levels of this bias in the populations studied, and whether intervening in participants’ generation of negative material has any effect on it.

Taking general tendency to negatively resolve ambiguity that prior tasks have attempted to capture as a further element, a three-part capturing of interpretive appraisal is the result*[1]. This may better represent a socially anxious individual’s response to an ambiguous social situation. They may feature a greater tendency to make negative resolutions when confronted with ambiguity, may perceive such resolutions as more likely to occur, and/or may perceive such outcomes as more catastrophic. By expanding our conceptualisation of interpretive bias in this way, the nature of this important construct can be further clarified.

The above viewpoint developed out of discussions with colleagues in the child anxiety and developmental interpretive bias fields. Some of them (Dodd, Stuifzand, Morris & Hudson, 2015) were

[1] * Dodd, Stuifzand, Morris and Hudson (2015) consider these biases in a slightly different way to that in which they are measured in this thesis, and also consider bias in participants’ first responses as a separate index. This was not undertaken in the current series of studies for pragmatic reasons detailed in the experimental and cross-experimental measurement chapters.

developing a preliminary task which could serve as a basis for expanded interpretive bias measurement (see the cross experimental analysis chapter for more details on this measure), and neatly incorporated measurement of participants generation, likelihood approximation and evaluation in a single measure. A modified version was incorporated across the constituent studies of this thesis in order to account for the interpretive appraisals of participants in the most comprehensive manner. Furthermore, it was decided that cross-experimental data from it would be compiled to give information on the nature of the constituent interpretive tendencies in the general population and specifically in individuals with high social anxiety.

A further benefit of incorporating such measures is their distinct nature from the training programs administered. CBM-I designs are often evidenced by outcome measures of bias with a high degree of similarity to their training materials. Some outcome measures replicate the scenario format itself (e.g. Tran, Hertel and Joorman, 2011). A potential concern here is that such task overlap facilitates temporary, transient and fragile priming effects, which serve to accentuate the outcome measurement of CBM-I. Whilst some cross-sensory exceptions exist (e.g. Hayes, Hirsch, Krebs & Mathews, 2010), and one aspect of "far" transfer (to experienced emotion and emotional reactivity) has been evidenced (Mathews & MacLeod, 2005), it is worth considering the incorporation of bias measurement tasks more distal from the training materials, to assess the cognitive limitations of the effect and further the evidence regarding the efficacy of the modification. This was a concern throughout the design of the constituent studies of this thesis, which manifested in the incorporation of multiple and distinct indices of bias in each experiment, both from the open response task and the scrambled sentence tests included as a "conventional" bias measure.

Observation three: Potential for improvements to CBM-I designs. Given the above consideration of the limitations on current measures of interpretive bias, and the potentially restricted definitions of interpretive bias under which they were conceived, it is conceivable that CBM-I programs in their current format may not be modifying all of the outlined elements of interpretive bias that they could. From the standpoint of maximising the efficacy of these interventions before widespread clinical implementation, comprehensive measurement of what CBM-I *currently does* (in ways like that detailed in the prior section) and potentially *could do* becomes a priority. A few researchers have already begun to take an appropriate approach.

A particular candidate area for improvement in CBM-I methods is the way in which the programs engage and immerse participants in the ambiguous scenarios they convey. In particular, recent CBM-I studies have sought to improve the mental imagery of trained participants to this end, with promising initial results gained with depressive patients (Torkan et al., 2014; Blackwell et al., 2015). Furthermore studies have considered explicitly instructing participants to make positive interpretations, finding that this does not affect outcomes (Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt, 2013) and a single recent study has combined these elements considered having participants generate endings themselves, finding equivalence with the effect exerted by a standard CBM-I condition for depression (Blackwell et al., 2015). This area of research was furthered in the current study via the application of explicitly-instructed and self-generated CBM-I variants to social anxiety specifically, and in the context of expanded bias measurement (which allows greater comparison of the effects on bias that each variant of CBM-I was able to have, and thus a more detailed portrayal of their relative benefits).

In the design of potentially improved CBM-I programs to be tested in this thesis, there was considered to be scope for an explicitly-instructed, self-generated version of CBM-I to be efficacious for social anxiety-associated interpretive bias. This optimism did not however extend the equivalence of explicitly instructed closed-resolution CBM-I to standard implicit closed-resolution CBM-I: it was suspected that this could be an artefact of study design and that this effect might break down under more comprehensive bias measurement such as testing under conditions of cognitive load. In particular, for a truly resilient bias modification to occur and extend beyond that imparted by standard implicit CBM-I in terms of observed effects, explicitly instructed closed-resolution CBM-I was suspected to be inadequate. In particular, it was assumed that any candidate for a superior training program would need to engage a greater array of cognitive processing, not what could arguably be less. In explicitly-instructed training with closed resolutions participants are explicitly given a production rule at the start of training and not given the opportunity to acquire implicitly over time it by repeated resolution of scenarios. As such they do not need to engage much with the procedure at all and the process involves less of an active search for alternative meaning (valence is already provided) as emphasised in cognitive therapeutic techniques in general (Beck, 2011).

For the self-generation based training meanwhile, we were hopeful of considerable enhanced effects. It was suspected that it might engage more of the relevant processes and have greater similarity to the real life situations that CBM-I training attempts to provide a training analogue for: put simply such situations involve a participant producing a suspected ending themselves, rather than completing a highly constrained preset fragment of an ending. If the nature of this task is considered further (real-life realism aside), potential for enhanced effects becomes clearer.

Participants completing standard, implicit closed-resolution CBM-I would encounter a series of word fragments, the constrained resolution of which provides sustained, consistent experience in bringing positive ending to ambiguity and thus imparts bias modification. With participants fully generating their own endings meanwhile, the modification would occur through them actively reconsidering the scenario for the potentially positive. This entire process demands a greater degree of consideration and comprehension of the content. Furthermore, considering for a moment the sustained engagement of participants in the procedure over time, generating an entire positive ending requires a consistent higher minimum of engagement, as participants have to comprehend the scenario, and engage with its potentially positive aspects, then speculate regarding how it could have a positive resolution, in order to provide an entire word of relevance. As such the process also arguably holds a closer analogue to the more thorough search for a positive point of view that a participant might undergo during full cognitive therapy (see Beck, 2011). Participants in closed-resolution CBM-I on the other hand can get away with paying little heed to the scenario itself and thus the value of the training as an analogue of a real life situation (evoking a realistic and detailed image of a real-life situation) may be minimal in those individuals with poorer attentional capabilities over time. This may limit effects of CBM-I for certain participants, and thus the effect sizes gained for measures of bias change and, more ultimately, experienced negative emotion.

Alternatively however it might be the disruption to the expected at the point of completion that is key to CBM-I's effect and the (re)learning of a production rule: this process may create the salience necessary for cognitive change to occur. As such the potential effect of CBM-I in this respect would be limited to that of its current implicit, closed-resolution manifestation. Either way this is a key element of CBM-I's mechanism of effect that is not clear, but one for which explicit justification and substantiation would potentially be of considerable use.

In accordance with the emphasis on gradual and thorough developments in methods this thesis takes a step-by-step approach to making improvements to CBM-I designs. As such it was decided that the first study in this thesis would simply further test the explicit instructions equivalence, applying further measures (a conventional bias measure *with and without* conditions of cognitive load to consider breakdown of effect under conditions in which working memory is occupied, and the expanded open response measure detailed above). As mentioned it was considered necessary to check whether explicit instructions *per se* might provoke less engagement with the experimental procedure, and so whether the effect gained might break down when more thorough bias measurement was applied, particularly under conditions of cognitive load. Furthermore it was necessary to look at the effects of explicit instructions alone before adding self-generation into this mix for control purposes.

It was decided that the second study would then extend this by trialling a novel "open response" CBM-I variant in which participants did not complete fragments but generated the entire ending themselves under explicit instructions to interpret positively, with appropriate control for explicitness and generation without instruction (which also enabled replication of the experimental conditions of the first experiment). The open response element was then returned to in the final study which looked at its ability to resist mood-incongruent stimuli and applied a two-part positive and negative training approach.

In considering explicit instruction and open response modifications to the CBM-I designs, the fact that expanded outcomes are measured alongside conventional ones is of considerable utility. It was considered feasible that improvements to the modification program may yield effects not just for one aspect of interpretive appraisal but several (continued full generation of positive endings to scenarios may for example lead participants not just to generate fewer negative scenarios post-training but also to estimate negative outcomes as less likely to occur). Furthermore it was necessary to consider the different improvements upon the basic CBM-I procedure, in terms of differences in their ability to modify these bias indices.

Summary and Design of the Empirical Elements of the Current Project

Whilst a considerable portion of the design of this thesis was laid down in an initial planning stage of the thesis it was nonetheless a responsive and ongoing process. In particular the planned design of the latter three experiments (chapter five) changed considerably in response to situational perspectives, findings and constraints. The eventual result was a format for the empirical components of the thesis that is detailed below.

Experiment one: Chapter three This chapter reports a single experiment with highly socially anxious participants, testing explicitly-instructed CBM-I against a standard implicit variant and non-valenced control training, using expanded interpretive bias measurement (load and non-load scrambled sentence tasks and open response bias measure). As such this experiment considered the scope of modification of standard CBM-I in terms of this greater outcome measurement. Furthermore it provided similar information on the explicitly-instructed variant of CBM-I, and allowed comparisons to be drawn between their performance.

Experiment two: Chapter four. This chapter features a single experiment with highly socially anxious participants, replicating implicit and explicitly-instructed training conditions from the first experiment and adding two conditions where participants generated their own ending to CBM scenarios, in one of which they had explicit instructions to provide positive interpretations. This again featured expanded interpretive bias measurement of load and non-load SSTs and the open response task. This experiment was able to provide replication of the two active conditions from the first experiment. Furthermore it provided extension of the improvements to CBM-I designs theme, with the addition of an explicitly-instructed condition in which participants generated the content, plus a control condition for the effect of (uninstructed) open generation *per se*.

Chapter five. This chapter contains three experiments with members of the general population, linked in standardised format but distinct in the experimental conditions compared and the aspects investigated as a result. All feature expanded measurement of interpretive bias. They also all incorporate a simple mood induction, and two sessions of CBM-I training (or a control program) separated only by a period of measurement. Experiments three and four consider the mechanism of effect of CBM-I in greater detail and experiment five returns to the open response training, comparing this with standard training in its ability to resist a simple mood induction.

Experiment three. This experiment looks at the comparative performance of CBM-I alone against a mood induction over the same period. As such it is able to provide initial evidence regarding whether or not CBM-I is able to go beyond a pure mood-priming effect and influence interpretation itself.

Experiment four. This experiment considers the ability, or lack thereof, of a simple mood induction to interfere with CBM-I training. A control condition of CBM-I only was compared with the same program issued with simple positive and negative mood inductions in further respective groups. Negative resolution training was issued to participants first, followed by positive resolution training. In this way all combinations of congruent and incongruent positive and negative training pairs could be considered.

Experiment five. This experiment is an extension of the comparison between standard and open response CBM-I implemented in experiment two, and looks at the comparative ability of standard and equivalent open-response CBM-I to resist the mood induction implemented. Again negative training is implemented first, followed by positive training.

Cross-experimental chapter. This chapter provides further, combined analysis of data from the time one measurement of bias and affect across the experimental studies. As such this component was able to give clarification on the interpretive bias measures implemented, the nature of the targeted biases in the general population, and their co-occurrence with state and trait anxiety.

Conclusion. This chapter summarises the overall implications of the evidence gained in the experimental and cross-experimental chapters, its strengths and limitations. It contextualises this with existing theory and empirical investigation and provides recommendations for future interpretive bias-focused research.

Ethical Concerns

It was necessary to navigate a number of ethical issues in the design of this series of studies. Many were easily accounted for. Standard anonymity procedures could be incorporated in the data collection from the experimental sessions for example by using a numerical system and removing other identifying features of data as appropriate. Participants in the initial experiment did send initial fear of negative evaluation data across via a personal email but identifying information was removed at the earliest stage possible. This was altered (for this reason and due to concerns regarding experimental control) in the second experiment, where participants completed all measures in the experimental session. It remained necessary to score participants' questionnaires on the spot to check their eligibility to participate, but no records were taken of this data and all data was analysed in a fully anonymised format.

Participants were able to give reasonably informed consent to the experimental procedure: the tasks they would be expected to complete were broadly detailed to them via a standardised information sheet (appendices one and two) and only the aims of these tasks were withheld. This said, there is the obvious ethical issue of manipulating a participants' interpretation without their direct consent within the experimental procedure. The duration of effect of single-session CBM-I does not appear to be a long-term one hence the provision of multiple training sessions (e.g. Bowler, Mackintosh, Dunn, Mathews, Dalgleish & Hoppitt, 2012) in studies that seek to go beyond proof of concept work. Nonetheless it was necessary to further navigate this issue by:

1. Closely following protocol from previous work (particularly Bowler, Mackintosh, Dunn, Mathews, Dalgleish & Hoppitt, 2012).
2. Subjecting the experimental designs to detailed scrutiny from the UEA School of Psychology Ethics Committee.
3. Ensuring that any negative training administered was followed by positively-valenced training in the latter series of experiments.
4. Provision of appropriate participant-focused debrief.

Comprehensive verbal and written debrief (appendices one and two) were administered to participants with a standardised sheet of information and accompanying opportunity to discuss the experiment with the experimenter. Written debrief included resources for the eventuality that the participant was concerned about their anxiety, and comprehensive contact details for the experimental team.

Conclusion of the Current Chapter

The current chapter details the evidence and rationale in favour of greater empirical exploration and conceptual clarity regarding both the nature of interpretive bias and its potential modification. It also outlines a supportive rationale for the expansion of CBM-I procedures beyond their current limited operation, in particular via greater consideration of explicit and participant-generated content in experimental designs. As such, the following chapter directly furthers the evidence regarding the expansion of measurement of interpretive bias, and the consideration of explicit content in CBM-I with a single test of simple CBM-I variants with highly socially anxious individuals.

Chapter Three: Experiment One

Implicit and Explicitly-Instructed Interpretive Bias Modification and Open Response Bias Measurement

Introduction

One of the key contributions of the cognitive approach to investigating emotion has been the discovery of specific processing biases in individuals with high levels of anxiety and negative affect (Mathews & MacLeod, 2005; MacLeod, 2011). In particular, content-specific biases in the information processing of key faculties such as attention, interpretation and memory have been measured via a variety of techniques and associated with a wide variety of emotional states and disorders, including depression and anxiety (Hertel & MacLeod, 2012; Mathews & MacLeod 1994, 2005). Furthermore, clinical studies indicate that such biases are instrumental in the maintenance of emotional disorders such as social anxiety, and that these biases can be manipulated via simple training programmes based on principles such as conditioning and rule-learning (Hertel & MacLeod, 2012; the literature review chapter for this thesis provides a more in-depth explanation of the principles upon which CBM-I is based, in particular pages seven to ten and twenty seven to twenty eight). This approach to intervention has been termed cognitive bias modification (CBM), and a number of relevant training programs have been developed under it, primarily focusing on modifying the attention and interpretation of participants (Mathews & MacLeod, 2005). Successful implementation of such training has been repeatedly demonstrated in nonclinical analogues and increasingly in clinical samples (Browning, Holmes & Harmer, 2010; Hertel & MacLeod, 2012).

A growing body of work within the CBM field has considered the application of cognitive bias modification for interpretation (CBM-I) to the alleviation of anxiety. Over the course of a CBM-I training program, participants are encouraged to make positive interpretations when faced with emotionally ambiguous information. In a commonly used CBM-I task, participants would be asked to read and visualise descriptions of one hundred events, each of which would have a missing end word, replaced by a fragment which would bring a resolution to the scenario described. Participants are asked to complete this fragment, and its nature means that they are constrained in doing so to only giving the scenario a positive resolution. Repeated practice of such tasks has been found to promote positivity in the interpretation of ambiguity and reduce the anxiety associated with this. Effects have been demonstrated for a variety of types of anxiety at both clinically-diagnosable and sub-clinical levels found in the general population. These include general anxiety (e.g. Amir, Beard, Burns & Bomyea, 2009), performance anxiety (e.g. Beard, Weisberg & Amir, 2011), and importantly social anxiety (e.g. Bowler, Mackintosh, Dunn, Mathews, Dalgleish & Hoppitt, 2012). Cognitive bias modification is therefore deemed a potentially important tool and worthy of a considerable body of further research aimed at translating it successfully to therapeutic settings (Mathews, 2011; Hertel & MacLeod, 2012).

A further area in which CBM-I, and the study of interpretive biases in general may be able to be improved is the measurement of the outcomes of training, particularly the biases themselves. Several more recent studies have suggested that it may be more appropriate to both expand and refine initial, broad definitions of overall interpretive bias into separate biases in abilities like the generation, evaluation and likelihood approximation of material (Huppert, Pasupuleti, Foa & Mathews, 2007; Creswell O'Connor, 2011; Dodd, Stuijfzand, Morris & Hudson, 2015). As such, it may be the case for example that an anxious participant may feature a bias in their generation of possible interpretations, their selection of a likely interpretation, or their evaluation of the chosen interpretation, or any combination thereof. Furthermore, different elements may be relatively transient or "state", susceptible to influence and thus modifiable and certain may be more "trait" and relatively static, less-modifiable tendencies for interpretation. These might require more sustained clinical effort in order to change significantly.

Whilst occasional efforts have been made to expand bias measurement as detailed above, widespread uptake and standardisation of these has yet to take place. Furthermore, bias modification studies in particular typically rely on notably limited methods of outcome measurement. These often involve fixed-choice, single answer tasks which provide a single broad index of interpretive bias and offer little specific detail on the processing of the individuals undertaking them. It is important to gain clarification on the nature of biased interpretation for both directly applied, practical and more distal, foundational, theoretical reasons.

Regarding the former, direct clinical benefits are possible simply from expanding the measurement of cognitive biases. Broader measures have potential use as diagnostic tools for early detection of anxiety-exacerbating processing. Whilst a full formalisation of comprehensively expanded bias measures is beyond its scope, the current thesis presents a key step towards such expansion with the measurement tasks it implements, detailed in full in the method section of the current chapter.

Furthermore, by looking in greater detail at the processing that an anxious individual exhibits when confronted with an ambiguous situation, the improved measurement techniques gained may be able to be implemented as outcome measures to future CBM-I studies. One key benefit of broader measurement of interpretation in bias modification designs is that researchers focusing on the practical applications of interpretive biases and their modification can ensure that the techniques put into place to attempt to change interpretation do so in an optimally productive way. As considered in the introductory chapters of this thesis, and returned to in the final chapter, CBM-I is often lauded for its simplicity by researchers, but this may be instead be a key limitation. An alternative perspective is that current manifestations of CBM-I are unnecessarily limited and may be able, with minor adjustments, to target a much more optimal amount of the maladaptive processing that can occur when anxious individuals encounter ambiguity. A key first step in such investigation is to gain clarity on what CBM-I currently does with expanded outcome measurement, before considering its potential to be improved. This is a key concern of the current chapter.

At the theoretical level meanwhile, such clarification would considerably expand cognitive models of anxiety, which at their most comprehensive currently only predict a general tendency to interpret negatively. There is a lack of empirical work considering what, in a cognitive and neurological sense, the interpretive bias actually is that current CBM-I procedures actually modify, particularly the processes which compose this overall effect, and related processes that influence it and are influenced by it. This lack of specific theoretical detail on the constituent processing of such a general tendency is unusual for such a field, and possibly symptomatic, as considered in the introductory chapter of this thesis, of a rush to clinical application. The postponement of clinical application in favour of exploring the finer-grained cognitive components of interpretation that are of relevance would yield a more comprehensive theoretical grounding and this is the perspective taken in the current and following experimental, cross experimental and discussion chapters.

Related to the above general lack of specific cognitive modelling is the issue of transfer effects from CBM-I training. Researchers have recently become interested in how influencing one cognitive process with training can have implications for other related ones (e.g. White, Suway, Pine, Bar-Haim & Fox, 2011), often using attentional bias modification and considering its impact on interpretations and other cognitions and behaviours. Whilst there is considerable research looking at affective transfer effects from CBM-I training (for example basic anxiety reduction in response to potentially stressful events), there is relatively little looking at its transfer effects to other cognitive abilities. This raises questions for interpretive bias modification work, such as whether training participants to resolve ambiguous scenarios in a positive manner might also decrease their perceived likelihood of negative outcomes occurring, or even their evaluation of these outcomes.

In addition to developing a wider array of bias measures, applying these as outcome measures and clarifying the theoretical nature of biased interpretation, theoretical mechanisms of interpretation modification and its potential for transfer effects, it is also necessary to improve the bias modification techniques applied. Whilst these have, as mentioned, yet to be informed by fully expanded measurement of interpretive biases, researchers have nonetheless attempted an important array of recent improvements to CBM-I programs and protocols. Supportive evidence has been found for elements such as multi-session training and the active engagement of participants in the resolution of ambiguity in the procedure, as opposed to them playing passive, receptive roles to the displayed resolution of ambiguity (e.g. Beard & Amir, 2008; Hoppitt, Mathews, Yiend & Mackintosh, 2010).

An unknown element that has received recent research interest is how implicit or explicit bias modification procedures should ideally be, in terms of how informed the participant is during their administration. A single recent direct study with interpretive biases suggested that participants explicitly being told to produce the desired valence of material (as opposed to being left to gain a training effect implicitly during the CBM program) made no difference to training outcomes in a CBM-I design (Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt, 2013) using a single, closed resolution bias measure. This is a key issue that should elicit further research interest for confirmation. If CBM-I measures are to be implemented as a widespread clinical intervention, it is likely that many participants will become reasonably aware of their purpose and operation. As such if this knowledge affects outcomes the procedures might become compromised in terms of validity and effect. Whilst the single existing study suggests this not to be the case, further substantiation is required, particularly given the limited, close to training nature of the outcome measurement applied. Theoretically meanwhile, there is also good reason for further investigation. CBM-I methods which are explicitly instructed and tell participants the production rule at the start of training may conceivably limit participant engagement. They may promote a more passive training experience, similar to that administered by Hoppitt, Mathews, Yiend and Mackintosh (2010), who found that participants needed to actively resolve scenarios and not be simply exposed to valenced endings in order to gain a training effect. As the prior chapter emphasised, the precise details of the mechanism of action of effective CBM-I are unknown, and knowing a production rule at the start of training might limit this and minimise the "search for positivity" and subsequent affirmation of this potential positivity that participants undergo. Knowing that the scenarios have a positive ending from the start does not necessitate the consideration, retrieval of an appropriate word, and affirmation of positivity that implicit training involves. As such it is arguably a much less active process.

In response to this, an aim of the current study was to extend investigation of the explicitness issue with a partial replication and extension that paid close attention to bias measurement. It was considered necessary to investigate whether the equality found in Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt's (2013) experiment was due to the measurement implemented. Further comparison of the comparative value of the two variants of CBM-I training was thus necessary: explicit instructions could be a simple but useful addition to certain types of CBM-I intervention or conversely might not work under different circumstances to Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt's (2013). In terms of what was expected, it was suspected that explicitly giving participants a production rule might do little to affect their performance when left to openly generate responses to ambiguity, but that it might have a reduced effect and potentially even limit or negate the effects of CBM-I training when participants were tested under conditions of cognitive load (due to these conditions occupying working memory, prohibiting the explicit implementation of the production rule and explicitly-instructed participants potentially relying less on learning an implicit, automatic rule during their training).

The clinical efficacy of CBM-I methods is arguably the paramount concern for researchers, and thus perfecting the ingredients is of high importance. As such clarifications like those detailed above are of importance for their potential to inform regarding what CBM-I methods can currently do, and also what we might be able to expand them to target in subsequent work. The current experiment was thus devised as a way of providing initial evidence on these linked questions regarding the nature of interpretive biases in the socially anxious, the scope of current methods of modifying interpretive bias to exert change on a variety of aspects of interpretation, and the comparative efficacy of implicit and explicitly-instructed variants of CBM-I when tested with a greater array of outcome measures. As such it compares a group receiving standard CBM-I with a group receiving inconsistent "dummy" training as a control and a further group receiving CBM-I with explicit instructions to interpret positively. Comparisons are undertaken via measurement of overall bias both with and without conditions of cognitive load, and via a novel open response bias task which generates indices relating to several different aspects of interpretation.

It was hypothesised that the control group receiving an inactive CBM program would show no significant change in interpretive bias. For experimental groups meanwhile it was hypothesised that, as evidenced in previous studies, both groups receiving CBM-I would exhibit significant reduction in negativity of interpretive bias (as indicated by a non-cognitive-load scrambled sentence bias measure and their generation, evaluation and likelihood approximation of negative material on an open response task) and significantly greater change in bias than a control group on these bias indices.

As outlined above, we suspected that when participants were asked to perform under mild cognitive load those participants who explicitly learned a production rule for CBM-I would perform significantly less well. Accordingly it was hypothesised that the group receiving the standard version of the training would show significantly greater modification of bias than the control group and the group receiving the explicitly-instructed version of the training on a scrambled sentence task issued under conditions of moderate cognitive load.

Method

Design

A three by two experimental design was used (see Figure two). The independent variable was experimental condition, with three groups of participants each receiving either explicitly-instructed CBM-I, CBM-I alone or a "sham training" control condition. Participants were measured pre-training for their trait social anxiety (as a selection criterion). Participants were also measured (repeated measures pre- and post-training) for their state anxiety, their biases in the generation, selection and evaluation of interpretations of ambiguous social scenarios via a novel open response task, and their overall interpretive bias as evidenced by scrambled sentence tests with and without cognitive load. As load was included as an additional within-participant variable for initial analysis of scrambled sentence task data the design became three by two by two for scrambled sentence task outcomes. Figure two below summarises the experimental design.

Figure 2: the experimental procedure

<table>
<tr><td>Experimental group</td><td>Control</td><td>Experimental group one</td><td>Experimental group two</td></tr>
<tr><td>Pre-session trait anxiety measurement</td><td colspan="3">Fear of Negative Evaluation Scale
Open Response bias task</td></tr>
<tr><td>Introduction</td><td colspan="3">Consent form and standardised instructions</td></tr>
<tr><td>State anxiety measurement</td><td colspan="3">Spielberger State Anxiety Inventory</td></tr>
<tr><td>Bias measurement
(Randomised order)</td><td colspan="3">Scrambled sentence task with cognitive load
Scrambled sentence task without cognitive load</td></tr>
<tr><td>Training</td><td>Control condition “Sham training”</td><td>CBM-I</td><td>CBM-I with explicit instructions</td></tr>
<tr><td>Affect measurement</td><td colspan="3">Spielberger State Anxiety Inventory</td></tr>
<tr><td>Bias measurement
(Randomised order)</td><td colspan="3">Scrambled sentence task with cognitive load
Scrambled sentence task without cognitive load
Open Response bias task</td></tr>
<tr><td>Debrief</td><td colspan="3">Standardised debriefing sheet and further verbal debrief as appropriate</td></tr>
</table>

Participants

Sixty student participants (forty-eight females and twelve males) were recruited from the University of East Anglia campus via posters and electronic advertisement including posts on relevant internet pages and use of an online participant recruitment system. Participants were over eighteen, with a mean age of twenty one years and ten months, and had received no treatment for mental health problems (self-disclosed) in the past year. Participants received money or psychology research participation system credits for their participation. All participants scored over fifteen on the Fear of Negative Evaluation Scale (FNE: Watson & Friend, 1969), indicating a moderate to high level of social anxiety. As such approximately one in three participant applicants were eligible to take part in the design.

Materials

Social anxiety. Participants for the experiment were screened using the Fear of Negative Evaluation Scale (FNE: Watson & Friend, 1969) in order to ensure that they had suitably high (whilst nonclinical) levels of trait social anxiety. The FNE is a 30-item scale consisting of items such as “I rarely worry about seeming foolish to others” and categorical yes or no responses. The FNE has good internal consistency, with Cronbach’s alpha coefficients of between .88 and .94, and has exhibited test-retest reliability and discriminative validity for social anxiety (Oei et al., 1991).

State anxiety. As a potentially influential extraneous variable, state anxiety was tracked during the experimental design using the state portion of the Spielberger State-Trait Anxiety Inventory (STAI: Spielberger, Gorsuch & Lushene, 1970). This is a 20-item scale where participants rate items such as “I am tense” on a scale from 1 (not at all) to 4 (very much so). A score of between 20 and 80 is produced for each participant. The scale has moderate test-retest reliability (to be expected of a state measure), good construct validity (Spielberger, 1983) and high internal consistency, with Cronbach’s alpha coefficients that reached a minimum of .86 and a median of .93 across a diverse variety of independent samples (Spielberger, 1983).

Overall interpretive bias. In order to measure the positivity of participants’ interpretive biases, scrambled sentence tasks (SST) were administered to participants pre- and post training as a pencil and paper task. Scrambled sentence tasks were first devised by Wenzlaff and Bates (1998) and have since been applied in a number of studies with the aim of measuring interpretive bias in an acceptably implicit way in depression and anxiety. In the social anxiety-focused variant of the task, as used by Standage, Ashwin and Fox (2010), participants receive twenty sets of six words, and are instructed to rearrange five of these words to make a coherent sentence. Each set of words is arranged so that it can either be arranged into one coherent sentence with a positive implication, or one sentence with a negative implication. All of these implications were relevant to social anxiety, the focus of the study. As such, a participant might receive the following during the task:

me fault won't people other will

The above would be able to be rearranged to either “other people will fault me” or “other people won’t fault me”. Four versions of the task were used, with two pre-training tests and two post-training tests completed by all participants, each with different sets of words. In two versions of the task, participants were asked to complete their sets of words under conditions of cognitive load. In these versions each participant was issued with a six-digit number at the beginning of the SST and asked to retain this and recall it at the end of the task. The administration of the different tests was counterbalanced and thus equally distributed across groups, so that some participants did task one with load first, some participants did task two with load first, some participants did task three without load first, and so on, with all participants completing all versions of the task by the end of the experiment (and all doing one load and one non-load version before training, and one load and one non-load version after training). Scrambled sentence tasks were scored by calculating the proportion of grammatically correct sentences that were completed that had a negative valence as per Bowler et al. (2012). Content of the tasks is included in appendix three.

Bias in interpretation generation, selection and evaluation. For the measurement of bias in the generation, selection and evaluation of participants' responses to ambiguous scenarios an open-ended bias task was applied. This was a modification of a prototype task created by Dodd, Stuijfzand, Morris and Hudson (2015), with some new content generated so that the task could exclusively target social anxiety, and every scenario rated for valence and likelihood of occurrence. This was as opposed to Dodd, Stuijfzand, Morris and Hudson's (2015) method of having participants select a single most likely scenario and rate the valence of this only. This was conducted under the rationale that participants may actually consider a range of scenarios while interpreting a situation and thus that a single most likely scenario (which may only be considered marginally more likely), may be only part of the overall nature of their interpretive bias. A participant might consider one scenario ending positive and most likely to occur, but may have a host of negative "what ifs" that also spring to mind for them and are highly instrumental in their overall anxiety. For example a participant might initially suggest that others laugh after something they said due to it being funny, but then be plagued other by considerations such as "what if I had food on my face?" or "what if they were humouring me?".

For the purposes of this experiment, the content of the scenarios was adapted to allow participants to provide more socially relevant answers. It was decided that each item should remain open-ended enough to allow a range of social and non-social responses of both positive and negative valence. As such the additional scenarios were developed with this in mind and sent to three colleagues with expertise in cognitive biases and/or anxiety who gave feedback and attempted to provide answers for each of them. Scenarios with insufficient responses, both overall and of each social/valence combination were considered for exclusion. Accordingly if more than one person provided fewer than five responses to a scenario this was modified or excluded. Similarly if a scenario consistently provoked scenarios of a certain valence, or participants consistently responded with purely social or non-social material, it was modified or excluded.

Twelve scenarios (appendix four) were produced in this manner. These were randomly split into two blocks for each participant to complete pre- and post-training. Counterbalancing was implemented so that one participant might receive scenarios one to six first, followed by seven to twelve at the last point of measurement, and a second participant might receive scenarios seven to twelve first, followed by scenarios one to six.

Participants were presented with ten spaces in which to generate multiple resolutions to each open-ended scenario. Every ending each participant generated was rated by the participant for both likelihood of occurrence and positivity on a scale from one to seven. For likelihood, one represented "extremely unlikely" and seven represented "extremely likely" with four being neither unlikely nor likely. For positivity, one represented the participant feeling "extremely negative" if this outcome were to occur and seven represented them feeling "extremely positive".

The extracted variables were whether participants generated negative material, whether they perceived any negative material that they generated as more negative, and whether they perceived such material as more likely to occur. Because the focus of the experiment was social anxiety, and any associated bias could be expected to be specific, the negative material included in analysis of these variables was narrowed to scenarios with a personal implication of social judgment (see below).

Three variables were derived from the responses participants gave to this measure at each timepoint. An independent coder with expertise in social anxiety was employed to provide the further necessary data by judging whether each scenario was relevant to social judgment (defined as having implications for how the participant would be perceived by others) and to give it an independent evaluation rating (the coder was blind to the evaluation ratings given by participants). Ten percent of this data was second-coded by a further independent rater, and a strong intraclass inter-rater correlation, r = .91 was gained on the raw ratings themselves. Ratings were then collapsed into social judgment negative and non-social judgment negative categories. Social judgment negativity is hereby referred to as SJN in the remainder of the chapter.

A ratio of participants' generation of (independent rater-classified) SJN content to non-SJN content was created per-timepoint. All the participant responses to each scenario designated as SJN were summated and divided by the overall number of responses to it that scenario. These per-scenario totals were then added together and divided by the number of them to which the participant had provided any responses (usually six at both timepoints but occasionally otherwise if participants could not think of any answers for a particular scenario). Per-timepoint totals were thus an index of bias: a higher figure indicated a higher tendency for SJN in the generation of material.

Participants' average evaluation of SJN material was also calculated. Participants' evaluation ratings for each SJN answer they gave per scenario were added up and divided by the number of SJN responses that they had provided for that scenario. These per-scenario bias indices were then added together for each timepoint and divided by the number of scenarios for which a response had been provided to provide a per-timepoint index of bias as with the generation variable, however this time the lower this number the more negative a participants' bias on this measure was.

Finally, an index of participants' average approximation of the likelihood of the negative outcomes they provided as scenario responses was calculated. Likelihood ratings for each SJN response per scenario were added together and divided by the number of answers given, then these per-scenario responses were summated and divided by the number of scenarios answered in a similar manner to the evaluation ratings. For this variable, the higher this number, the greater the perceived likelihood of SJN events occurring.

Interpretive bias modification. The procedure for modifying interpretive bias used was a computerised, scenario-based procedure, similar to that applied by Bowler et al. (2012). Participants received standardised instructions:

In this task you will be asked to read short descriptions of four lines each. To display each line press the `advance' key (arrow down). As you read, please do your best to imagine yourself in the situation described. The last line always has the final word missing from it. When you press `advance', the missing word will appear but in an incomplete form (like this: w-r-: both the `o' and the `d' are missing from `word').

Your job is to use the description you have just read to help you complete the word correctly, so that it finishes the last sentence. When you know what the incomplete word is, press the advance key. Then enter the FIRST missing letter. Find it on the keyboard and press the appropriate letter key. When you have found it, the correct word will be displayed. Finally, you will be asked a question about the description to check that you understood it.

For this you will be using the left (for NO) and right (for YES) arrow keys. You will be reminded of this each time on the screen. Don't worry if this seems complicated; there is some practice to remind you how to do it.

Press the advance key to try some examples.

For participants in the explicitly-instructed condition the word "*correctly*" in the second paragraph was replaced by "*in a positive manner*".

Participants began by completing two non-social anxiety relevant example scenarios. These were followed by one hundred scenarios describing social anxiety-relevant events, in each of which the final word (crucial to overall outcome) is incomplete. Participants were instructed to press what they believe to be the first missing letter of this word on their keyboard. The nature of the words and letters missing was such that participants could only provide a correct answer that completes the word in a positive manner and brings an overall positive resolution to the scenario. Participants completed a comprehension question following each item. This task was delivered via a computer running E-Prime software. An example of one of the hundred items received by participants would be:

You invite a variety of family and friends to your house for your birthday party. They are quite different and you know that some of them don't always get on. As you are clearing up afterwards you think that getting all these people together made the evening

i—eresti--

The participant would be asked to provide the first missing letter of this fragment, and for a grammatically correct word would have no choice but to answer "n". The participant would then be asked a confirmation question *"are you pleased that you invited everyone to the party?"* with a choice of yes or no responses. Scenarios were presented in mixed blocks of ten with a randomised within-block order. In the explicitly-instructed condition participants were issued with the following instructions in addition to those that the CBM-I only group received:

"In the main task, each situation will begin emotionally ambiguously, but turn out well in the end (like the final practice item). If you bear in mind that all the situations end positively it will help you with the task."

Control group participants meanwhile received a sham-training version of the CBM-I only procedure, identical except that it did not allow participants to make consistent positive resolutions to scenarios. Fifty of the viewed scenarios could only be resolved in an irrelevant manner that preserved the ambiguity and potential negativity of the scenario, and a further fifty could only be resolved positively. Scenarios were again presented in mixed blocks of ten with a randomised within-block order. Such sham training has been used successfully as a control in a number of prior CBM investigations, under the logic that it is the *sustained* nature of the positive resolutions that participants make which exerts an effect (in terms of theory, the nature of cognitive bias modification mechanisms of action is considered in more depth in chapter five of this thesis, but both of the two currently suggested accounts, valence priming and rule learning, should require consistency to exert an effect).

Procedure

Potential participants who enquired about the experiment were sent a copy of the Fear of Negative Evaluation scale and an initial open response bias measure. Those scoring over fifteen on the FNE were invited to an experimental session at a time convenient to them. Participants attending the experimental session were led to a noise-attenuated research pod and presented with a sheet of further information about the experimental procedure and a consent form for the session. Participants who agreed to take part first completed two scrambled sentence tasks as a measure of initial interpretive bias. Participants completed one test under cognitive load (retention of a 6-digit number during the test as described above) and one without this additional task. When they had completed the scrambled sentence tasks, participants completed a copy of the STAI.

Participants were randomly allocated to one of three conditions. Participants in the implicit experimental condition completed a computerised interpretive bias modification program only. Participants in the explicit experimental condition completed the program but additionally received a standardised set of instructions to interpret positively prior to its commencement. Participants in the control condition for this experiment completed the “sham training” variant. Subsequent to completing the computer program, participants completed a second pair of scrambled sentence tasks featuring new scenarios (as with the pre-bias modification test one was completed with cognitive load and one without), and the order of these was again counterbalanced. After this they filled out a second copy of the state component of the Spielberger State-Trait Anxiety Inventory. The final measure completed by participants was a second open response measure with a new set of six ambiguous social scenarios. Participants were thanked, debriefed verbally and with a standardised sheet, and received recompense in money or research participation system credit for their time.

Results

Participant Characteristics

Fifty females and ten males contributed data to the experiment. The groups were comparable in terms of their age (with a mean age of 19.1 years) and gender balance, and their scoring on measures of attentional control, state anxiety, initial interpretive bias under conditions of load and no load, and initial interpretive bias in response generation, response selection and response evaluation. The relevant means and standard deviations are given in table one.

Table 1: Pre-training means of attentional control, state anxiety, scrambled sentence test score (load and no load), and response generation, selection and evaluation bias scores.

Variable	Control group	CBM-I only group	Explicitly-instructed CBM-I group	F
Attentional control	49.42 (6.9)	48.9 (7.56)	45.24 (7.15)	1.88
State anxiety	42.83 (10.05)	39.19 (10.84)	41.48 (8.32)	.67
Scrambled sentence test score (no Load)	47.35 (19.19)	42.7 (20.8)	39.48 (16.7)	.84
Scrambled sentence test score (load)	32.86 (16.24)	35.19 (18.33)	36.97 (12.8)	.32
Response generation bias score	.2 (.11)	.22 (.1)	.19 (.09)	.31
Likelihood approximation bias score	3.64 (1.1)	3.44 (.84)	4.02 (.98)	1.6
Response evaluation bias score	2.17 (1.16)	2.33 (.89)	2.09 (1.32)	.23

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parenthesis.

Higher scores on the SST indicate higher proportions of negative resolutions made.

Levene's tests were performed to check homogeneity of variance for each group on each of the studied variables. Results indicated that there was no significant deviation from homogeneity of variance for any of the variables used. Normality procedures indicated generally satisfactory normality, with slight deviations for the evaluation variable. It was decided to rely the robustness of the ANOVA model and no transformations were undertaken. Trimmed means showed little variation from untrimmed and so no outlier removal beyond participants failing to complete the measures correctly was implemented. Some exclusion was necessary on this basis where participants failed to complete measures correctly: participants' data was excluded for a measure if they had not completed a measure according to the instructions given: this was particularly the case for the open response measure where not entering a complete response for at least two of the scenarios at each time point was considered to be failing to complete the measure. Furthermore a small minority of participants chose to leave the experiment at time point two and as such did not contribute data from this point onwards. meaning that from the twenty two participants gathered for each experimental group final numbers were eighteen participants in the control group twenty one participants in the implicit CBM-I group and twenty one in the explicitly-instructed CBM-I group.

Control Variable: State Anxiety

Anxiety levels during the experiment were considered a potential extraneous variable, especially given the role of state affect in outcomes of bias measurement tasks (discussed and investigated in greater detail in the latter experimental and cross-experimental chapters of this thesis). For the purposes of this experiment it was thus necessary to account for the role of anxiety and check that it did not vary significantly in response to group, time of measurement, or a combination thereof. Accordingly, prior to analysis of bias variables a three by two mixed ANOVA was conducted on the state anxiety data, with the between groups factor of intervention received (with the three levels of control training, standard implicit CBM-I and explicitly-instructed CBM-I) and the within groups factors of time of testing (with the two levels of pre- and post-CBM-I program). No significant main effect of time of test, $F(1, 59) = .01$, $p = .93$, partial eta squared < .001 or experimental group, $F(2, 59) = .93$, $p = .4$, partial eta squared = .03 was observed. Furthermore no significant interaction effect between time of test and experimental group was observed, $F(3, 58) = .05$, $p = .95$, partial eta squared =.002.

Overall Interpretive Bias: Scrambled Sentence Resolution with and without Cognitive Load

Analytical approach. Because a three-way interaction between experimental condition, time of test and presence/absence of cognitive load was predicted, an initial three-way omnibus ANOVA was applied. Significant effects from this were followed up with dedicated two-way ANOVAs, and significant effects within these considered with t-test comparisons. A t-test table is included at the end of this section (Table two) and contains all of the relevant means and standard deviations.

Initial omnibus ANOVA. A three by two by two mixed ANOVA was conducted on the scrambled sentence data, with the between groups factor of intervention received (with the three levels of control training, standard implicit CBM-I and explicitly-instructed CBM-I), the within groups factors of time of testing (with the two levels of pre- and post-CBM-I program) and the further within groups factor of presence of cognitive load (with the two levels of cognitive load and no cognitive load).

A significant main effect of time of test was observed, $F(1, 59) = 6.76$, $p = .012$, partial eta squared = .11. The constituents of this effect are considered in full later in this results section, but the results appear to have been primarily created by reductions in bias in the control and CBM-I only groups without conditions of cognitive load. No significant main effect of experimental group was observed, $F(1, 59) = 1.33$, $p = .27$, partial eta squared = .05.

A significant main effect of cognitive load was observed, $F(1, 59) = 17.02$, $p < .001$, partial eta squared = .19. Comparisons revealed a significant difference between negativity of sentence resolution without conditions of load (M = 42.97, S.D. = 18.91) and with conditions of load (M = 35.11, S.D. = 15.75) at pre-training, *t*(59) = 3.67, *p* = .001. This difference between non-load (M = 35.92, S.D. = 21.29) and load (M = 33.02, S.D. = 19.27) scores was not significant at post-training measurement, *t*(59) = 1.59, *p* = .12. However this distinction was not sufficient to create a significant interaction effect between time of test and cognitive load, *F*(2, 57) = 3.74, *p* = .06, partial eta squared = .06.

A significant interaction effect between time of test and experimental group was observed, *F*(3, 59) = 6.78, *p* = .002, partial eta squared = .192. The constituents of this interaction are assessed with two-group ANOVAs and corrected t-test comparisons (Table two) later in this results section. To briefly summarise their nature, the primary effect producing this significance appears to have been the significant interaction between time and experimental group between the standard implicit CBM-I and the explicitly-instructed CBM-I conditions, with standard CBM-I participants exhibiting significant (within-group) reduction in bias and explicit participants not doing so.

No significant interaction effect between cognitive load and experimental group was observed, *F*(3, 59) = .51, *p* = .6, partial eta squared = .02.

A significant interaction effect between time of test, cognitive load and experimental group was observed, *F*(4, 59) = 4.39, *p* = .02, partial eta squared = .13.

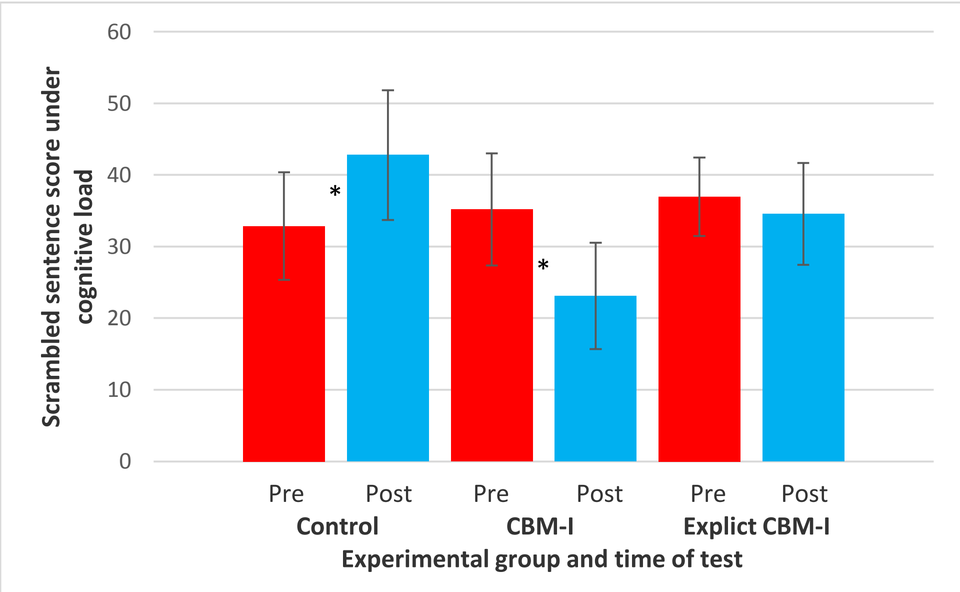
To briefly summarise, effects of time, load, time and group and time, load and group interactions were observed for scrambled sentence task performance. The first aspect of further investigation that these prompted was consideration of load and non-load group performance for the latter interaction, which was of particular interest in determining outcomes from training.

Comparative group performance over time, split by presence of load. In response primarily to the significant main effect of cognitive load found it was considered necessary to further assess the comparative performance over time of the groups, under load and non-load conditions separately. A further pair of three by two mixed ANOVAs were performed, using the between groups variable of experimental condition received.

When participants completed measures without conditions of cognitive load no significant main effect of experimental group was observed, *F*(1, 59) = .84, *p* = .44, partial eta squared = .03. A significant main effect of time was observed, *F*(1, 59) = 9.4, *p* = .003, partial eta squared = .14. Within-group comparisons are detailed in full in table two, but indicated that this decrease over time was only significant in the standard implicit CBM-I group. These differences were (marginally) not enough to create a significant interaction effect between experimental group and time of test, *F*(2, 58) = 2.97, *p* = .06, partial eta squared = .09.

When participants completed measures under conditions of cognitive load no significant main effect of experimental group was observed, $F(1, 58) = 1.85$, $p = .17$, partial eta squared = .06. No significant main effect of time was observed, $F(1, 58) = .55$, $p = .46$, partial eta squared = .01. A significant interaction effect between experimental group and time of test was observed, $F(3, 58) = 9.28$, $p < .001$, partial eta squared = .25. Again within-group comparisons are detailed in full in table two, and to summarise these indicated that this interaction was composed of a significant increase in bias negativity in the control group, a significant decrease in the standard implicit CBM-I group, and no significant change in the explicitly-instructed group. Figure three displays the means and confidence intervals of this interaction.

Figure 3: means and ninety-five percent confidence intervals for scrambled sentence performance under cognitive load pre-and post-training program for each experimental group.



In summary, a significant effect of time was observed and a marginal (just outside of significance) effect of time and group interaction were observed when participants completed measures under cognitive load. The effect of time was not significant and the interaction became significant when load was removed, which comparisons indicated were driven by the significant effects in the above figure.

To consider two-group comparisons in further detail, it was subsequently necessary to run further ANOVAS, again split according to presence or absence of cognitive load. These are detailed in the following section.

Time of test and experimental group: two group comparisons. The interaction between time of test and experimental group was followed up with a set of mixed ANOVAs looking at comparative performance between pairs of groups in more detail, and breaking these down into load and non-load performance. The relevant means and standard deviations that created this effect are included in table two, located in the comparisons section which follows.

Performance without cognitive load. For the comparison between the control group receiving inactive CBM-I training and the experimental group receiving CBM-I only no significant main effect of condition was observed, $F(2, 39) = 1.52$, $p = .23$, partial eta squared = .04. A significant main effect of time was observed, $F(1, 39) = 14.55$, $p = .001$, partial eta squared = .28. No significant interaction effect between time of test and experimental group was observed, $F(3, 39) = 1.03$, $p = .32$, partial eta squared = .03.

For the comparison between the control group receiving inactive CBM-I training and the experimental group receiving CBM-I with explicit instructions to interpret positively no significant main effect of condition was observed, $F(2, 39) = .54$, $p = .47$, partial eta squared = .014. No significant main effect of time was observed, $F(1, 39) = 1.77$, $p = .19$, partial eta squared = .05. No significant interaction effect between time of test and experimental group was observed, $F(3, 36) = 1.76$, $p = .19$, partial eta squared = .05.

For the comparison between the experimental group receiving CBM-I only and the experimental group receiving CBM-I with explicit instructions to interpret positively no significant main effect of condition was observed, $F(2, 39) = .39$, $p = .54$, partial eta squared = .01. A significant main effect of time was observed, $F(1, 39) =5.81$, $p = .02$, partial eta squared = .13. A significant interaction effect between time of test and experimental group was observed, $F(3, 39) = 5.8$, $p = .02$, partial eta squared = .13. The comparisons detailed in table two indicated that this was composed of significant decrease in bias in the CBM-I only group and no significant change in the explicitly-instructed group.

In summary of the two-group outcomes from non-loaded scrambled sentence task, only a significant decrease in bias over time was observed in control and implicit CBM-I groups, and only an effect of experimental condition was observed for the control and explicitly-instructed CBM-I groups. For the implicit and explicit comparison suggested both overall between group differences regardless of time, a significant decrease in bias over time and an interaction between condition and time, created by a lack of explicit group change and presence of bias decrease in the CBM-I group.

Performance under cognitive load. For the comparison between the control group receiving inactive CBM-I training and the experimental group receiving CBM-I only no significant main effect of condition was observed, $F(2, 39) = 2.93$, $p = .1$, partial eta squared = .07. No significant main effect of time was observed, $F(1, 39) =.16$, $p = .69$, partial eta squared = .004. A significant interaction effect between time of test and experimental group was observed, $F(3, 39) = 16.32$, $p < .001$, partial eta squared = .31. Comparisons (table two) indicated that a significant increase in control group bias and a significant decrease in CBM-I only group bias created this effect.

For the comparison between the control group receiving inactive CBM-I training and the experimental group receiving CBM-I with explicit instructions to interpret positively no significant main effect of condition was observed, $F(2, 39) = .21$, $p = .65$, partial eta squared = .01. No significant main effect of time was observed, $F(1, 39) = 2$, $p = .17$, partial eta squared = .05. A significant interaction effect between time of test and experimental group was observed, $F(3, 39) = 5.36$, $p = .03$, partial eta squared = .13. Comparisons (table two) indicated that the significant increase in control group bias and lack of significant change in the explicitly-instructed group made up this effect.

For the comparison between the experimental group receiving CBM-I only and the experimental group receiving CBM-I with explicit instructions to interpret positively no significant main effect of condition was observed, $F(2, 39) = 2.1$, $p = .16$, partial eta squared = .05. A significant main effect of time was observed, $F(1, 39) = 10.95$, $p = .002$, partial eta squared = .22. A significant interaction effect between time of test and experimental group was observed, $F(3, 39) = 4.9$, $p = .03$, partial eta squared = .11. Comparisons (table two) indicated that a significant decrease in CBM-I only group bias and a lack of significant change in the explicitly-instructed group made up this effect.

To summarise two-group comparisons for loaded scrambled sentence task, the control and implicit CBM-I experimental groups displayed a significant interaction effect only, created by increase in control group bias and decrease in experimental group bias. A lack of significant change in the explicitly-instructed group maintained such an interaction in its comparisons with both the control group and the implicit group. With the latter, effects of time and condition were also observed however.

Comparisons. The significant interactions gained were further assessed with corrected comparisons to consider whether each of the individual group changes that contributed to each of them was significant in itself at a within-group level. These comparisons are included in table two with effect sizes in Cohen's *d*. Of note is that the CBM-I results both with and without cognitive load met significance when Bonferroni corrections were applied but that the control group outcome did not.

Table 2: Pre- and post-training comparisons of scrambled test performance with and without cognitive load, split by experimental condition received.

Experimental condition	Presence of load	Pre-test mean	Post-test mean	*t*	df	*d*

Control group, inactive training	No load	47.35 (19.19)	39.54 (19.76)	1.89	17	.41
	Load	32.86 (16.24)	42.78 (19.62)	2.17*	17	.5
CBM-I only	No load	42.71 (20.81)	29.26 (22.57)	3.58**	20	.7
	Load	35.19 (18.33)	23.1 (17.36)	3.79***	20	.61
CBM-I with explicit instruction	No load	39.48 (16.69)	39.47 (20.6)	.01	20	<.001
	Load	36.97 (12.8)	34.57 (16.61)	.8	20	.12

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

Higher scores on the SST indicate higher proportions of negative resolutions made.

Interpretive Bias as measured by Open Response task.

Bias in response generation. A three by two mixed ANOVA was conducted on the response generation data, with the between groups factor of intervention received (with the three levels of control training, standard implicit CBM-I and explicitly-instructed CBM-I) and the within groups factors of time of testing (with the two levels of pre- and post-CBM-I program). No significant main effect of experimental group was observed, $F(2, 59) = 2.65$, $p = .08$, partial eta squared = .09. A significant main effect of time of test was observed, $F(1, 59) = 32.01$, $p < .001$, partial eta squared = .39. Comparisons (table three) indicated that the control and CBM-I only groups significantly increased in bias (even when a Bonferroni correction was applied) whereas the explicitly-instructed group did not. This created a significant interaction effect between time of test and experimental group, $F(3, 59) = 5.03$, $p = .01$, partial eta squared = .17. Comparisons are detailed in table three below and means are displayed in figure four with respective ninety-five percent confidence intervals.

Table 3: Pre- and post-training comparisons of social judgment-related negative material generated in response to open response task scenarios.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*

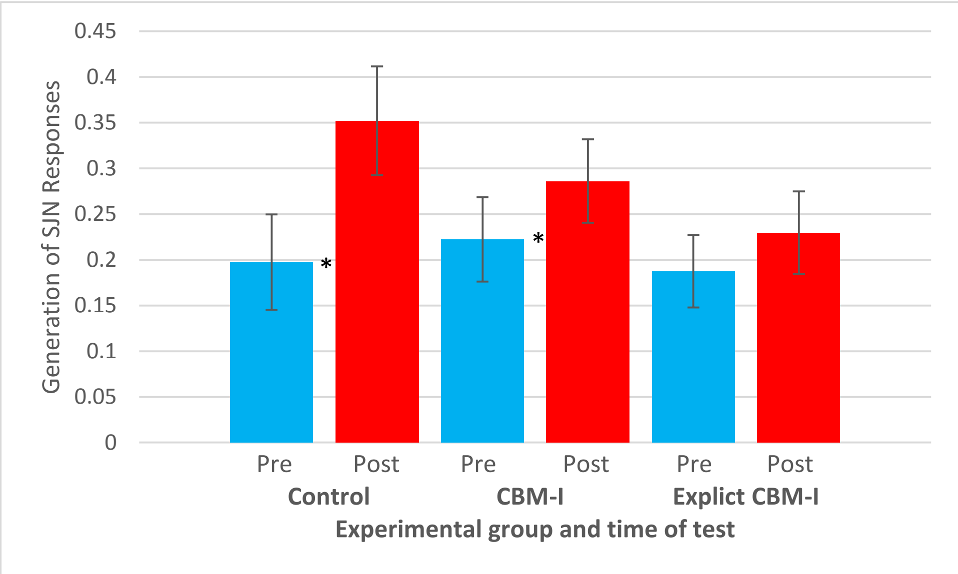
Control group, inactive training	.2 (.11)	.35 (.13)	5.1***	17	1.37
CBM-I only	.22 (.1)	.29(.01)	3.09**	20	.56
CBM-I with explicit instruction	.19 (.09)	.23 (.01)	1.53	20	.37

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

Higher scores indicate greater provision of SJN responses and thus greater negativity.

Figure 4: means and ninety-five percent confidence intervals for generation of social judgment-relevant negative endings pre-and post-training program for each experimental group.



Bias in response likelihood approximation. A three by two mixed ANOVA was conducted on the likelihood approximation data, with the between groups factor of intervention received (with the three levels of control training, standard implicit CBM-I and explicitly-instructed CBM-I) and the within groups factors of time of testing (with the two levels of pre- and post-CBM-I program). No significant main effect of time of test was observed, $F(1, 59) = 3.31$, $p = .08$, partial eta squared = .06. No significant main effect of experimental group was observed, $F(2, 59) = 2.14$, $p = .13$, partial eta squared =.08. No significant interaction effect between time of test and experimental group was observed, $F(3, 59) = .56$, $p = .58$, partial eta squared =.02.

Bias in response evaluation. A three by two mixed ANOVA was conducted on the response evaluation data, with the between groups factor of intervention received (with the three levels of control training, standard implicit CBM-I and explicitly-instructed CBM-I) and the within groups factors of time of testing (with the two levels of pre- and post-CBM-I program). No significant main effect of time of test was observed, $F(1, 59) = .87$, $p = .35$, partial eta squared = .02. No significant main effect of experimental group was observed, $F(2, 59) = .003$, $p = .997$, partial eta squared <.001. No significant interaction effect between time of test and experimental group was observed, $F(3, 59) = .33$, $p = .72$, partial eta squared =.01.

Discussion

Scrambled Sentence Task Performance without Cognitive Load

For scrambled sentence task performance it was hypothesised that both CBM-I groups would exhibit a significant reduction in negative interpretive bias after CBM-I training on the version of the task issued without cognitive load. This was supported for the standard implicit CBM-I group only. It was also predicted that change in bias in both experimental groups would be significantly greater than any non-significant change in a control condition for a scrambled sentence task issued without conditions of cognitive load. This was not supported.

For non-load results it is primarily of note that whilst t-tests suggested superiority of the standard, implicit CBM-I group, overall when an ANOVA devoted to non-load scores only was run, a significant overall main effect of time only was found and no significant interaction as a result of these outcomes. The absence of a significant interaction in this ANOVA suggests that, when taken in the context of the greater standard deviation of all the groups, the effect in the implicit group was not significantly different from the other groups' performance and may be able to be explained by the overall tendency of participants to reduce (or at least demonstrate some fluctuation) in negativity of bias score over time.

Scrambled Sentence Performance with Cognitive Load

It was further hypothesised that the group who received the standard, implicit CBM-I training would show significantly greater positive modification of bias than the other groups when bias was measured by a scrambled sentence task issued with conditions of moderate cognitive load, and that they would be the only group to show significant reduction of bias on this measure. This was technically supported by the results gained, but should be considered in the context of an unexpected increase in bias in the control group.

Testing under conditions of load did somewhat reverse the comparative effect between explicitly instructed and control groups, with the former outperforming the control group, but this was as a result of the explicitly-instructed group changing very little and the control group actually became markedly more negative in their bias. The overall interaction was significant, alongside the more specific two group ANOVAS run to dissect this further, but the control group effects add a caveat to this outcome as they enhance the comparative effect of the active training. The individual t-test comparisons meanwhile suggested that when only within-group variance was taken into account, both the control group and the CBM-I only group displayed significant change, in their opposing directions. A minor element in favour of the efficacy of the CBM-I implicit training however is the observation that this group did reach a notably higher level of significance.

Scrambled Sentence Task Performance: General Discussion

Given the above outcomes it is worth considering the role of cognitive load in scrambled sentence-based bias measurement in more detail. The first element of note here is that in general participants, regardless of group or time, scored significantly higher on the scrambled sentence task, indicating greater negativity, when tested without conditions of cognitive load than when cognitive load was present (this effect was significant at pre-training measurement but attenuated to non-significance by post-training). This outcome is an unexpected one, and whilst a tentative result (and one which would benefit from replication), should nonetheless receive consideration. Applying load in scrambled sentence tasks is taken to elicit responses at a less declarative, more implicit level from participants, by making it more difficult for participants to exert cognitive control over the more automatic elements of processing involved (Wenzlaff & Bates, 1998). The implication is thus that a result gained under load is potentially a more accurate and representative indication of the participants' actual biased interpretive processing. Why then participants might show more negative bias when able to exert greater control over attention and working memory is difficult to explain. The outcome may however just be a false positive without a substantial mechanism behind it, particularly as the effect dissipated at the second time of measurement. Alternatively, participants may bring more explicit components of overall cognitive bias to bear, consciously considering the scenarios and their perceived feasibility (which may be biased both at an automatic level and a more controlled one) rather than processing these at a purely automatic level, or even elaborating on their initial automatic tendencies.

Reasons for the absence of improvement in the explicitly-instructed group should be considered, as this element of the study provides contrast with Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt's (2013) findings. The current study indicates that it had no effect, with inertia maintained in scrambled sentence task-measured bias in the current design, and that the standard, implicit CBM-I training has considerably more promise for actually improving this bias. It is important to keep in context the differences in method here, particularly in measurement. Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt's (2013) study used a single bias measure and the current study used two distinct bias measures, which had some key differences. If we posit continua of breadth of measurement, speed of completion and use of closed resolution (with the scrambled sentence task being one extreme and the open response task being close to its inverse in these respects), the measure applied in the prior study is arguably between the two. Participants were asked to rate preset endings, of positive, negative and "foil" valences, to a set of 15 previously viewed scenarios for their similarity to the original description of the scenario. In the current study the difference between the implemented measures and those in Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt's (2013) study was intentional: as mentioned in the introduction to this chapter, a key aim was to consider as much of the scope of interpretive bias as possible (given practical experimental constraints) in the measurement implemented. This said, there may be something about the ending-rating measure that gives it an advantage in terms of being modified by the CBM-I training implemented and allows an explicitly-instructed rule to take effect, and it may be procedurally "nearer" to the experiences of participants during the CBM-I training than either the scrambled sentence task or the open response task.

Methodological differences with Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt's (2013) experiment, and the need for further replication and triangulation using different measures aside, it is worth considering some mechanism-based explanations for the effects gained. It may be the case that the explicit condition disrupted a process of rule-learning that is best left implicit, and explicit instructions to implement it may have served as a distraction to participants that prevented them from engaging fully with the CBM-I task. It may have made participants ruminate or check answers slightly more rather than proceeding automatically with the task. In a converse but related way it may alternatively have facilitated participants' progress through the task and meant that they needed to consider the solution to each word fragment less, as candidate positive words were slightly primed and more easily accessible. It is possible that the search for a solution is a critical component of CBM-I's mechanism of action. This said, self-report indications from participants during debrief in this and other CBM-I experiments suggest that participants often report realising that the only correct ways to end scenarios are positive in CBM-I conditions, so a large between-groups difference of this type may not exist (note that this is unlikely to confound by breaking the single blind nature of the experiment because participants only show knowledge of the full *experimental* aims in exceptional cases, and within the current design this was something checked during the experimental debrief, with exclusions made accordingly).

Open Response Task Performance: Generation, Likelihood and Evaluation

The predictions that both CBM-I groups would exhibit a significant reduction in negative interpretive bias (within-group and relative to a control group) after CBM-I training in their generation, likelihood approximation and evaluation on the open response measure were generally not supported for either group. The only exception was with generation of responses, in which participants in the standard, implicit CBM-I condition exhibited a significant increase in bias. However control group participants demonstrated a significant increase in bias, meaning that both experimental groups technically produced a greater number of SJN responses than them despite non-significant change in the explicitly-instructed group. Potential explanations for this are discussed below.

It is necessary to first consider the unexpected findings for participants' generation of responses. Here participants in all experimental groups tended to generate more social judgment-related negative outcomes at post-training than pre-training. This rebound effect is difficult to explain. It may however be that it is an artefact of general social response generation. As detailed in the results section this index was calculated as the proportion of social judgment-related negative content to all other content generated, including benign non-social and benign social judgment related content. Exposure to the series of social judgment-related scenarios in the CBM-I training programs may prime participants towards the consideration of social scenarios, and this could potentially extend to generation. As such their ratio of social judgment-related positive scenarios may have increased also, but the increase in the index could be the result in a corresponding decrease in non-socially-relevant material considered as possible for an ending to each scenario, as participants became used to generating social material in general.

The unexpected findings regarding generation aside, conceptually it is worth considering how the declarative approximation of likelihood and evaluation elicited by the open response task may certainly be a more reflective construct, and may not reflect the fast, on-line and responsive processing characteristic of prior bias measurement tasks. Participants did not, unlike in the open response task, have a time limitation on the open response task, and tended to take ten to fifteen minutes to complete it (note that this is a post-experimental reflection and exact timings were not taken). They may have quickly generated a set of responses and then spent a relatively large portion of this time considering how likely and negative each one was with relatively high depth and rationality. This process may not have entirely accurately reflected or been concordant with the kind of initial approximations that participants might make, but there was something of an immediacydepth trade off here and, as outlined above, this task was concerned with encompassing as much of the participants' interpretive process as pragmatically possible.

To consider the nature of the open response task in more detail, it should be noted that open response tasks as a whole have only been applied previously in a few measurement studies (Huppert, Pasupuleti, Foa & Mathews, 2007; Creswell & O'Connor, 2011; Dodd, Stuijfzand, Morris & Hudson, 2015), and no modification studies, and one was adapted in terms of content and measurement to fit the sample and research interests of this study. It was very much a preliminary attempt to expand measurement of interpretive bias beyond the simplicity of measures like scrambled sentence tasks and recognition tests. However there is good reason to think that such measures may actually encompass more of the key elements of interpretation than prior closed-resolution measures, particularly scrambled sentence resolution (this is discussed in more detail, and in the light of further evidence, in chapter six). The task might measure more trait elements of the sub-biases that accompany social anxiety and would contribute to an interpretation in a real-life setting, and these may be considerably less amenable to bias modification. They may for example require multi-session training or an enhanced training program that engages more cognitive processes in order to be successfully modified. If they did turn out to be modifiable by any variant of CBM-I however, this could be a finding of considerable implication (the following experimental chapter details a further attempt to exert change on participants' generation, likelihood and evaluation when resolving ambiguity).

The above said, the open response task may measure elements of interpretation that are less on-line and state in nature than existing measures of bias. Indeed Dodd, Stuijfzand, Morris and Hudson (2015) go further in measuring generation, considering the first responses that participants made as a separate aspect of their generation of responses. This would have been useful in the current sample but was not feasible due to their study looking at general anxiety and the current one focusing on social anxiety and thus on SJN responses only. Whilst participants generated a high amount of SJN responses in general, some participants did not provide an SJN response as a first answer for each scenario in the current study: as such practically this would have reduced statistical power to a sub-useful level, and a larger sample at least would have been needed for investigation which was not practical. In this anxiety-focused experiment (anxiety being an emotion associated with ambiguity, potential threat and indecision), it was considered to be of primary salience and relevance to look at the full range of possible scenarios a participant considers in response to ambiguity. This was in order to represent their overall processing (even if this encapsulation concerned more of their subsequent ruminative consideration than their fast, on-line automatic processing as potentially indicated by first responses).

General Discussion

Overall then, whilst it did not favour the effectiveness of explicitly-instructed CBM-I, the current experiment provided a very limited and caveated amount of supporting evidence for the effectiveness of standard, implicit CBM-I training in modifying interpretive biases as measured by the previously used tasks, and some limited evidence of its ability to affect wider bias in response generation via an open-ended task. These results need substantiation via replication and triangulation of methods and measures, and whilst the explicitly instructed version of CBM-I implemented was unaffected, other variants should be sought that may exert greater change both on those variables that were affected by the training in the current design and those that were not. The results of this experiment are not cause, at the broader level, to abandon the notion of refining CBM-I procedures beyond their current state *per se*. It may be that a small modification like explicit instructions may serve as a distraction or annoyance, but that larger modifications that engage further faculties may be able to modify interpretive bias more comprehensively (including those aspects included in and indicated by the open response task). This formed a considerable element of the rationale for the following chapter. This partially replicates the active conditions of the current design for confirmation and adds additional conditions in which participants were instructed to generate their own content.

Chapter Four: Experiment Two

Open Response Training and Bias Measurement

Introduction

The current thesis provides an initial exploration of key modifications to cognitive bias modification for interpretation (CBM-I) programs to facilitate greater therapeutic efficacy, and expansions to the ways in which these outcomes are measured. Taking a stepwise approach, the experiment detailed in the prior chapter focused on the comparative efficacy of explicitly and implicitly instructed CBM-I training under more comprehensive and demanding conditions of measurement than those implemented previously. What was found was that a standard implicit CBM-I program appeared to modify interpretive bias to a slightly greater extent than an explicitly-instructed condition. There was limited and slightly mixed evidence for this, with significant findings occurring only under certain analyses, conditions of cognitive load and only when measurement with a basic, closed resolution bias measure was implemented. It was acknowledged that this was a tentative finding, and that improvements to CBM-I designs could also potentially occur in other ways. In particular it was hoped that further and more considerable adaptations of CBM-I designs could provide significant change beyond that achieved in the prior CBM-I implementations.

The current experiment was run in response to two key further aims that developed from the previous work. The first of these was simply to provide a degree of replication of elements of the first study. Replication is, as mentioned in the second literature review chapter of this thesis, a highly under-emphasised element of empirical psychological work, and the CBM-I field is no exception to this, with no direct, entire-procedural replications in existence currently. Further to this, there is the fact that accurate, optimal meta-analysis requires that studies be closely aligned with similar conditions and ideally identical or near-identical outcome measures. CBM in general has been found to have a sub-optimal level of study homogeneity (Cristea, Kok & Cuijpers, 2015), and the effect of this is that meta-analytical conclusions are notably more tentative. This is something which gains importance when the clinically-oriented nature of the field is considered. CBM-I tasks are considered a potential intervention and therefore should produce reliable and relatively invariant effects.

In following closely the design of prior experiments, the current study also maintained a number of the general aims and themes found throughout this thesis. Specifically, the expanded measurement of interpretive bias continued to be implemented in order to measure outcomes in as comprehensive detail as possible, and consider the effect of cognitive load on bias outcomes. The more unique element of the current study was a concern for further expanding the method of modification of bias in CBM-I programs. Such programs currently focus on asking participants to resolve ambiguity by providing a conclusion that is preset and invariant. Dodd, Stuifzand, Morris and Hudson (2015) in their justification for open response *measurement* of interpretive bias, note that such a task is not an entirely appropriate analogue for the cognitive process facing an individual interpreting ambiguity. In particular, this is due to the fact that such individuals are not suddenly presented with a pre-set closed resolution which they merely need to finish, but are generally involved in a more complex process of generating and appraising such resolutions. They make the case for a multi-element capturing of individuals' interpretation to correspond more closely to this. The justification for open response measurement in the current study is that a similar rationale applies to the modification of interpretation. To date, a single published study has taken up this approach to the modification of bias (this study emerged just after data collection had finished for the current study and at the time of inception no experimental paradigm existed). Rohrbacher, Blackwell, Holmes and Reinecke (2014) found broad equivalence between open and closed resolution CBM-I variants, administered via audio to individuals with mild to moderate depression, with the closed-resolution version also affecting mood. They justified the general approach of participant-generated content primarily in terms of authenticity, suggesting a degree of participant scepticism to be present for certain, personally incongruous scenarios under prior, closed-resolution training (in which participants encounter both scenarios and resolutions that are preset by the designer of the experiment and thus not necessarily of maximal relevance or feasibility for each individual). This reasoning aligns considerably with the (transferred) justification offered by Dodd, Stuifzand, Morris and Hudson (2015) of improving the analogue that procedures provide to real life interpretive scenarios. A convincing initial case for open response bias modification forms as a result. Furthermore, this case gets stronger if we consider that doing so may engage further interpretive processes (or existing ones to a greater extent) than those activated simply by completing closed-resolution tasks (like resolving word fragments and receiving feedback on these) in closed resolution designs. For example a participant is compelled to consider feasible possibilities for a positive resolution in the content of the scenario they read, rather than simply considering candidates for a missing letter in the word they read. In such a scenario it is very conceivable that this is could exert a more efficacious bias modification and a more transferrable process to real life interpretive ambiguity. The task both places greater demand on the participants' process of generating appropriate positivity, and provides an arguably greater analogue (in terms of ecological validity) for the kind of process they might undergo in a real life setting.

No open response research currently exists with anxious individuals, who may differ substantially in their processing of ambiguity and their response to such training. Furthermore, no research exists that attempts to capture the broader outcomes of training in a manner concordant with Dodd, Stuifzand, Morris and Hudson's (2015) approach to interpretation. It is important that any research concerning open response training attempts to capture its effects in a broad and comprehensive manner, and thus open response training and measurement were implemented (alongside more traditional closed-response methods of modification and measurement) in the current experiment.

In line with all of the above, the current study followed closely the design of the prior one, and exactly replicated the two active experimental conditions, whilst adding a further experimental condition and the necessary distinct control condition in order to consider other elements of potential improvement to CBM-I training. The standard implicit CBM-I and explicitly-instructed conditions were maintained both in order to provide replication of the prior experiment and to allow better experimental control within the current design: they provided direct control over presence of CBM-I training per se, and over presence of explicit instructions for CBM-I training respectively, which were an element of the novel experimental condition. In this, participants were asked to generate entire ending words to each scenario they encountered (as opposed to completing the constrained fragments as in prior CBM-I programs) and to consistently do so with words which would bring a positive implication to each scenario. Presence of open response training meanwhile was accounted for with a further, non-explicitly instructed open response program (which asked participants to simply generate entire words to end the scenarios, with free choice over valence). This formed an updated control condition for the current experiment. Figure five below shows the combinations of elements that allowed this more comprehensive control for clarity.

Figure 5: combination of program variations creating control in the current design and their manifestation as experimental conditions.

Implicit/explicit	*Constrained resolution*	*Participant generated resolution*
Implicit	Standard word fragment CBM-I	Open-ended control condition
Explicitly positive	Explicitly instructed word fragment CBM-I	Open ended CBM-I training with instructions to provide positive resolutions

A further series of hypotheses for testing were devised in order to tease apart the distinct accounts of necessary and potentially supplementary operating elements of CBM-I. Of primary concern, aside from the substantiation of prior findings, was testing the effective primary null hypothesis of the current series of experiments. This is essentially that CBM-I is in its optimal format currently in its closed resolution and implicitly-implemented manifestation, and further to this that it is the very absence of implicitness that lends it its existing potential. It may be that implicitness brings about more effective conditioning and affective priming or rule-learning due to the "surprise" of each constrained ending, for example and that making the valence of training explicit thus disrupts this. The extreme-end alternative to such a position is that CBM-I is in its most basic and limited implementation currently and that it is possible to expand its procedure of administration and outcomes (along with the associated potential clinical efficacy) considerably.

For the specifics of hypotheses, firstly an assumption was that none of the groups would show significant differences in state anxiety, both to one another and across time points, during the experiment. This was the case in the last experiment and would again indicate that any changes were independent of fluctuations in anxiety.

Regarding explicit closed-resolution training, due to the results of the prior experiment, it was assumed that there would be no significant change in bias on any of the applied measures of interpretation. Similarly it was predicted that participants in the non-explicitly instructed open response control condition would not show significant change in bias on any of these measures either.

Based on the results of the prior experiment, it was predicted that participants in the closed resolution, standard implicit CBM-I condition would show significant change from pre-to post training on the scrambled sentence task within-group across the time points. It was predicted that this would be significantly greater than any (non-significant) change in the explicitly-instructed CBM-I group and the open response control group.

Regarding open response training, it was predicted that the participants who received open-response with explicit positive interpretation instructions would show significantly lower levels of negative interpretive bias (as indicated by scrambled sentence task performance under load and non-load conditions, and open-response bias index scores for generation, likelihood approximation and evaluation) post-training than the two closed resolution CBM-I groups and the open response control group (and that this change would be significant at a within-group level).

Method

Design

A four by two experimental design was used. The independent variable was experimental condition, with four groups of 20 participants each receiving either explicitly-instructed CBM-I, CBM-I alone, explicitly-instructed open response CBM-I or a “sham training” control condition in which participants simply generated their own responses with no constraints on valence. Participants were measured pre-training in terms of their trait social anxiety (the latter being a selection criteria only). They were measured with repeated measures pre- and post-training for their state anxiety levels, overall interpretive bias as evidenced by scrambled sentence tests with and without cognitive load, and their biases in the generation, selection and evaluation of interpretations of ambiguous social scenarios via a novel task. As with the prior experiment, because load was included in a single omnibus analysis for scrambled sentence test scores the design effectively became four by two by two for this variable.

Participants

Eighty student participants (sixty-five females and fifteen males, with a mean age of twenty years and eight months) were recruited from the University of East Anglia campus via posters and electronic advertisement including posts on relevant internet pages and use of an online participant recruitment system. Criteria for participation were that participants were over 18, had self-disclosed that they had received no treatment for mental health problems in the past year, and scored over 15 on the Fear of Negative Evaluation Scale (FNE: Watson & Friend, 1969), indicating a moderate to high level of social anxiety. Participants received money or psychology research participation system credits for their participation.

Materials

Social Anxiety. The Fear of Negative Evaluation Scale (FNE: Watson & Friend, 1969) was used to check participant eligibility for the experiment. The FNE provides a 30-item measure of nonclinical social anxiety, to which participants provide yes or no responses to statements such as “I rarely worry about seeming foolish to others”. Each item is a single potential point and as per the prior experiment a cut-off of fifteen was applied in the current experiment. The FNE has Cronbach’s alpha coefficients of between .88 and .94, and satisfactory test-retest reliability and discriminative validity for social anxiety (Oei et al., 1991).

State anxiety. Participants completed the state portion of the Spielberger State-Trait Anxiety Inventory (STAI: Spielberger, Gorsuch & Lushene, 1970) at two points during the experiment. The inventory was applied in the same manner as that in the prior chapter.

Overall interpretive bias. Scrambled sentence tasks were administered to participants pre- and post-training. The tasks were administered with load and counterbalancing identical to that in the prior experiment. Bias indices were also calculated in the same manner as the prior experiment as the proportion of grammatically correct sentences that were completed that had a negative valence. As such each participant again had one pre-training bias index under load and one without conditions of load, and a corresponding pair of indices for their post-training performance. Content of the tasks was identical to that applied in the prior experiment and thus is included in appendix three.

Bias in interpretation generation, selection and evaluation. An open ended interpretive bias task was employed to capture variation in three further aspects of participants' interpretive appraisals. Detail on the adaptation and content of this task is included in the prior chapter as it was applied in exactly the same manner with identical scenarios (appendix four) and counterbalancing procedures utilised. Identical outcome indices of generation, likelihood approximation and evaluation were also extracted using the method described in the prior chapter.

Interpretive bias modification. Bias modification was attempted using a computerised, scenario-based procedure, an adaptation of that used by Bowler et al. (2012). In this, initial standardised instructions are read by participants:

In this task you will be asked to read short descriptions of four lines each. To display each line press the `advance' key (arrow down). As you read, please do your best to imagine yourself in the situation described. The last line always has the final word missing from it. When you press `advance', the missing word will appear but in an incomplete form (like this: w-r-: both the `o' and the `d' are missing from `word').

Your job is to use the description you have just read to help you complete the word correctly, so that it finishes the last sentence. When you know what the incomplete word is, press the advance key. Then enter the FIRST missing letter. Find it on the keyboard and press the appropriate letter key. When you have found it, the correct word will be displayed. Finally, you will be asked a question about the description to check that you understood it.

For this you will be using the left (for NO) and right (for YES) arrow keys. You will be reminded of this each time on the screen. Don't worry if this seems complicated; there is some practice to remind you how to do it.

Press the advance key to try some examples.

For participants in the explicitly-instructed condition the word "*correctly*" in the second paragraph was replaced by "*in a positive manner*".

For participants completing the open response control condition, the second paragraph was replaced by:

Your job is to use the description you have just read to help you complete the word, so that it finishes the last sentence in whatever way you choose. When you know what the incomplete word is, press the advance key. Then enter the letters of the missing word. Finally, you will be asked a question about the description to check that you understood it.

For participants completing the explicitly-instructed open response experimental condition, the second paragraph was adjusted to:

Your job is to use the description you have just read to help you complete the word in a positive manner, so that it finishes the last sentence in a positive way. When you know what the incomplete word is, press the advance key.Then enter the letters of the missing word. Finally, you will be asked a question about the description to check that you understood it.

Example scenarios and the same one hundred training scenarios and confirmation questions were applied. As such the exemplar content in the prior chapter remains representative. Three different active training variants of the program, plus a further one modified to provide a control condition, were issued to participants. In a condition that provided standard, implicit CBM-I training, participants completed the implicit training detailed in the prior chapter only. In an explicitly-instructed condition, also as per the prior experiment, participants read the following before completing their CBM-I training:

"In the main task, each situation will begin emotionally ambiguously, but turn out well in the end (like the final practice item). If you bear in mind that all the situations end positively it will help you with the task."

As the current experiment considers the potential for enhanced effects from participant generated content in training, two newly-devised open-ended program variants were employed as the remaining two conditions. Both of the programs had identical scenario content to that received by participants in the closed-resolution conditions. The differences were in the method of scenario resolution and the instructions issued. Participants did not receive a word fragment at the end of each scenario, instead there was simply a space, and the on-screen instructions asked them to enter a word that would complete the scenario using their keyboard. Participants in the active, explicitly-instructed open-ended training group were instructed at the start of training and after every ten scenarios to vividly visualise themselves in each of these scenarios before providing an appropriate response.

Procedure

Participants who responded to advertisements regarding the experiment were sent an initial information email, attached to which was a copy of the FNE scale and an initial open response bias measure. Those scoring over fifteen on the FNE were invited to arrange an experimental session at a convenient time, and as with the prior experiment this resulted in around one third of potential participants taking part in the experimental design.

The sessions themselves again took place in a noise-attenuated research pod. Participants read a sheet of further information about the experimental procedure (appendix one) and signed a consent form. Upon agreeing to take part, participants completed two scrambled sentence tasks (with order counterbalanced as described in the materials section above) as a measure of initial interpretive bias. One test was completed under conditions of cognitive load and one without this requirement, with the order of the presence of load also counterbalanced. This was followed by participants completing an initial copy of the STAI.

Participants were then randomly allocated to their experimental condition. Participants who participated in the implicit CBM-I condition completed the standard interpretive bias modification program only. Participants in the explicit experimental condition did the same but with a preceding standardised set of instructions to interpret positively. Participants in the open-ended control condition for this experiment completed a variant in which they simply provided an entire word for the ending to each scenario. Participants in the active training open-ended condition completed similar training with additional instructions to consistently provide words which would bring a positive implication to each scenario they encountered.

Post-completion of the CBM-I or control program, participants completed a second copy of the state component of the Spielberger State-Trait Anxiety Inventory. Participants then completed two further scrambled sentence tasks composed of new scenarios (with the order of both presence of load and content again counterbalanced). Finally, participants filled out a second open response measure with a new set of six ambiguous social scenarios (content was again counterbalanced). Participants received verbal and standardised written debrief (appendix one), thanks and were given monetary payment or research participation system credit for their time. Figure six below summarises participants' progress through the experimental procedure.

Figure 6: the experimental procedure

Experimental group	Open Response Control Group	Closed Resolution Experimental group (Implicit)	Closed Resolution Experimental group (explicit)	Open Response Experimental Group
Pre-session trait anxiety measurement	Fear of Negative Evaluation Scale			
Introduction	Consent form and standardised instructions			
State anxiety measurement	Spielberger State Anxiety Inventory			
Bias measurement (Randomised order)	Scrambled sentence task with cognitive load Scrambled sentence task without cognitive load Open Response bias task			
Training	Unconstrained open response control program	Closed resolution CBM-I	Closed resolution CBM-I with explicit instructions	Open response CBM-I with explicit instructions
Affect measurement	Spielberger State Anxiety Inventory			
Bias measurement (Randomised order)	Scrambled sentence task with cognitive load Scrambled sentence task without cognitive load Open Response bias task			
Debrief	Standardised debriefing sheet and further verbal debrief as appropriate			

Results

Participant Characteristics

Sixty three females and sixteen males contributed data to the experiment. The groups were comparable in terms of their age (with a mean age of 19.5 years) and gender balance and their scoring on measures of starting state anxiety, initial interpretive bias under conditions of load and no load, and initial interpretive bias in first response given, response generation, response selection and response evaluation (see the dedicated sections below for the calculation of these scores). The relevant means are given in table four.

Table 4: Pre-training means of initial state anxiety, scrambled sentence test score (load and no load), first response bias score, and response generation, selection and evaluation bias scores.

Variable	Implicit CBM-I group	Explicitly-instructed CBM-I group	Open response control group	Open response CBM-I group	F
State anxiety	35.7 (6.09)	36.2 (8.4)	38.6 (10.4)	35 (9.1)	.7
Scrambled sentence test score (Load)	30.97 (17.4)	33.09 (19.27)	27.71 (11.73)	26.96 (14.09)	.69
Scrambled sentence test score (no load)	30.19 (15.05)	27.32 (20.54)	23.39 (15.17)	24.16 (16.82)	.65
Likelihood approximation bias score	3.25 (.78)	3.24 (.9)	3.24 (.78)	3.23 (1.05)	.001
Response generation bias score	1.52 (.32)	1.51 (.21)	1.66 (.32)	1.59 (.36)	.81
Response evaluation bias score	1.71 (.57)	1.8 (.68)	1.8 (.54)	2.05 (.77)	.43

Standard deviations are included in parenthesis

Levene's tests were performed to check homogeneity of variance for each group on each of the studied variables. Results indicated no significant deviation from homogeneity of variance for any of the variables used. Normality procedures indicated generally satisfactory normality, except for time one scrambled sentence task scores. It was again decided that the robustness of the ANOVA model could be relied upon (particularly as there was a benefit to keeping statistical procedures in close alignment with the prior experiment) and no transformations were undertaken. As with the prior experiment, trimmed means showed little variation from untrimmed and so no outlier removal beyond participants failing to complete the measures correctly was implemented. However, again some exclusion was necessary on this basis where participants failed to complete measures correctly (undertaken on the same basis as the prior experiment), meaning that from the twenty three participants gathered for each experimental group final numbers were seventeen participants in the implicit CBM-I group, nineteen in the explicitly-instructed CBM-I group, twenty three in the open response control program and twenty in the explicitly-instructed open response CBM-I group.

State Anxiety

A four by two mixed ANOVA was conducted on the state anxiety data, with the between groups factor of intervention received (with the four levels of standard implicit CBM-I training, explicitly-instructed

CBM-I training, open-response control program and explicitly-instructed open response CBM-I training) and the within groups factors of time of testing (with the two levels of pre- and post-CBM-I program).

No significant main effect of time of test was observed, *F*(1, 78) = 3.79, *p* = .06, partial eta squared = .048.

No significant main effect of experimental group was observed, *F*(1, 78) = 1.68, *p* = .18, partial eta squared =.06.

No significant interaction effect between time of test and experimental group was observed, *F*(2, 78) = .47, *p* = .71, partial eta squared =.02.

Overall Interpretive Bias: Scrambled Sentence Resolution With and Without Cognitive Load

A four by two by two mixed ANOVA was conducted on the scrambled sentence data, with the between groups factor of intervention received (as with state anxiety, this had four levels: standard implicit CBM-I training; explicitly-instructed CBM-I training; open-response control program; explicitly-instructed open response CBM-I training), the within-groups factors of time of testing (with the two levels of pre- and post-CBM-I program) and the further within-groups factor of presence of cognitive load (with the two levels of cognitive load and no cognitive load).

No significant main effects or interactions were observed in this dataset at the p<.05 level. Table five below details the effects in question.

Table five: ANOVA table for scrambled sentence task data

Effect	*df*	*F*	*p*	*Partial eta squared*
Time of test main effect	78	1.33	.25	.02
Cognitive load main effect	78	1.21	.28	.28
Experimental group main effect	78	.34	.79	.01
Time of test and cognitive load interaction	78	2.78	.1	.03
Time of test and experimental group interaction	78	.73	.73	.03
Cognitive load and experimental group interaction	78	.5	.68	.02
Time of test and experimental group and cognitive load interaction	78	.21	.89	.01

Open Response task.

Bias in Response Generation.

A four by two mixed ANOVA was conducted on the response generation data, with the between groups factor of intervention received (with the distinct experimental conditions again forming the same four levels as applied in the ANOVAs for prior measures) and the within groups factor of time of testing (again with the two levels of pre- and post-CBM-I program).

A significant main effect of time of test was observed, $F(1, 75) = 42$, $p < .001$, partial eta squared = .15. t-test comparisons (table six) suggested this to be composed of a significant decrease in negativity in all experimental groups which was still present after Bonferroni correction.

Table 6: Pre- and post-training comparisons of social judgment-related negative material generated in response to open response task scenarios.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	.2(.15)	.06 (.12)	3.28**	16	.91
CBM-I with explicit instruction	.29 (.16)	.15 (.14)	3.4**	18	.9
Open response control program	.25 (.21)	.15 (.18)	3.12**	22	.46
Explicitly-instructed open response CBM-I	.29 (.24)	.16 (.12)	3.13**	19	.61

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parenthesis.

Higher scores indicate greater provision of SJN responses and thus greater negativity.

No significant main effect of experimental group was observed, $F(1, 75) = 1.34$, $p = .27$, partial eta squared =.05. This was accompanied by a further lack of a significant interaction effect between time of test and experimental group was observed, $F(1, 75) = 2.42$, $p = .07$, partial eta squared =.09.

Bias in Response Likelihood Approximation.

A three by two mixed ANOVA was conducted on the likelihood approximation data, with the between groups factor of intervention received and the within groups factors of time of testing, both featuring the same levels as those applied in the ANOVAs for generation.

A significant main effect of time of test was observed, F(1, 75) = 12.36, p = .001, partial eta squared = .15. Comparisons (table seven) suggested that only the open response control group displayed significant change, increasing in negativity to a significant extent alone (maintained after Bonferroni correction).

Table 7: Pre- and post-training comparisons of likelihood approximations made by participants (of social judgment-related negative material generated in response to open response task scenarios).

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	3.2 (.78)	3.62 (.88)	.9	16	.48
CBM-I with explicit instruction	3.24 (.9)	3.3 (.79)	.23	18	.07
Open response control program	3.24 (.78)	4.07 (.79)	4.95***	22	1.07
Explicitly-instructed open response CBM-I	3.11 (.95)	3.41 (1.12)	1.0	19	.31

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parenthesis.

Higher scores indicate greater likelihood approximation of SJN responses.

No significant main effect of experimental group was observed, F(1, 75) = 1.34, p = .27, partial eta squared =.05. Furthermore no significant interaction effect between time of test and experimental group was observed, F(1, 75) = 2.42, p = .07, partial eta squared = .09.

Bias in Response Evaluation.

A four by two mixed ANOVA was conducted on the state anxiety data, with the between groups factor of intervention received and the within groups factors of time of testing, each again featuring the same levels as those applied in the ANOVAs for generation. No significant main effects and no significant interaction effect were observed. Table eight details the investigated effects.

Table 8: ANOVA table for response evaluation data

Effect	*df*	*F*	*p*	*Partial eta squared*
Time of test main effect	78	1.25	.27	.02
Experimental group main effect	78	.94	.43	.43
Time of test and experimental group interaction effect	78	1.53	.22	.06

Additional Analysis: Comparison of Initial Anxiety Levels in First and Second Experiments of this Thesis

A significant difference was noted between the Spielberger state anxiety scores of participants at pre-training in the current experiment and the prior one detailed in chapter three. Participants in the current experiment had significantly lower state anxiety scores (M = 36.49, S.D = 8.8) than those in the prior experiment (M = 41.08, S.D. = 9.7), $t(140) = 2.94$, $p = .004$.

Discussion

The following discussion considers each task in turn before drawing these elements together to consider their overall implications.

Scrambled Sentence Task Performance

For scrambled sentence task performance, it was predicted that participants receiving explicitly-instructed closed-resolution CBM-I and a non-explicitly-instructed open resolution control condition would exhibit no significant change in interpretive bias. This was supported by the results of the ANOVAs conducted on the scrambled sentence tasks undertaken with and without cognitive load by participants.

For the remaining groups, it was firstly predicted that participants in the closed resolution, standard implicit CBM-I condition would show significant change from pre-to post training on the scrambled sentence task within-group across the time points and that this would be significantly greater than any (non-significant) change in the explicitly-instructed CBM-I group and the open response control group. This was not supported by the results gained.

For open response training meanwhile, the hypothesis was that the participants who received an open-response program with explicit positive interpretation instructions would show significantly greater reduction in negative interpretive bias on all measures post-training than the two closed resolution CBM-I groups and the open response control group, and that the change would be significant at a within-group level. This was not supported by the finding of a significant main effect of time only.

A supporting hypothesis made, for reasons of experimental control, was that there would be no significant change in state anxiety across the time course of the experiment, and it is of note that whilst this was supported by the ANOVA conducted, the outcome was very close to significance, indicating some rise in state anxiety and possible effect. Whilst the lack of significance at a preestablished level should be the key indicator here (alongside the further indicator of a modest effect size), suggesting state anxiety did not have a pivotal role in the other results gained, it may nonetheless form part of an explanatory context for the experiment (and its deviation in findings from the procedurally close prior study in this thesis, in which notably lower variation in state anxiety was observed). An element of the experiment, which state anxiety has some overlap with but does not comprehensively represent, is overall negative affect. This was uncontrolled for in the current design (and indeed the prior one) as the main focus was anxiety and the biases related to it, and overall negative affect has not generally been accounted for in contemporary designs (Hallion & Ruscio, 2011). This said, it may be the case that it is an element worthy of measurement or control. It becomes particularly prominent in fact when the role of affective priming is considered in the modifying of interpretation. This has been considered as a potential explanatory mechanism for at least some of the effects gained in CBM-I designs (and even by some as a feasible candidate for an operating mechanism for CBM-I; Clifton, Hedley, Mountier, Tiszai, & Grimshaw, 2015). This said, it is difficult to see where there was scope for much priming in the reverse direction of most of the training administered to take place. In terms of an effect from CBM-I this might be more feasible in conditions like the open response control condition. In this condition participants were free to make the interpretations they chose and could thus potentially condition themselves into further negativity over time, but little group difference was in fact observed. It may be that the lengthy procedure (circa an hour and a half for most participants, with a long period of repetitive training in the middle) induced negativity of mood and interpretation through, for example, a boredom effect and thus limited the effect of training. This is however something of a speculation given the lack of direct data in the current design, yet may form an element for control in future designs. The following series of experiments (chapter five) and cross experimental analysis (chapter six) do take up this general line of enquiry regarding the role of state affect in bias in further detail however.

Change in state affect aside, *initial* state anxiety upon beginning the experiment should also be considered for its ability to affect the results. Some further analysis was possible and revealed that participants in this experiment had significantly lower initial anxiety levels than individuals in the first experiment, and that levels of state anxiety were overall in the high-moderate region. Whilst CBM-I programs have been able to exert significant change on bias in high-anxiety individuals in a number of prior designs, this has some important qualifying factors. Firstly, studies have generally focused on trait anxiety, clinical diagnosis (as an initial selection criteria) and anxiety change over time and not accounted for initial levels as a moderator variable. Secondly studies have often used multiple training sessions to achieve such effects, which may accentuate training outcomes or at least allow participants to undertake training during some lower-state anxiety phases across the duration of administration.

Reasons for the difference in starting anxiety levels are difficult to ascertain. One of the few distinguishing characteristics between the studies is the time of year they were conducted at. As such participants' being undergraduates, may have had varying workloads or other general academic stressors (impending assessments for example) between these two general timepoints that meant initial participants approached experiment one with more moderate state anxiety levels than their contemporaries in the latter experiment.

The above rests on the assumption that anxiety levels are instrumental in determining bias (and thus that there is an overall cyclical relationship between bias and overall affect). This is more concordant with the "affective priming" explanation of interpretive bias offered in prior work (Mathews & Mackintosh, 2000). The cross experimental analysis detailed in the fifth chapter of this thesis explores this relationship in further detail, and to preview the results, does fundamentally does find a consistent relationship between state anxiety and bias on the scrambled sentence task (both with and without cognitive load), suggesting a substantial link here.

It is worth additionally giving some consideration to non-affect based explanations for the scrambled sentence test outcomes. Firstly, it should be noted that there remains the distinct possibility that the training administered was simply unable to exert a meaningful effect on participants in the experiment. Providing comprehensive reasons for this (distinct from the exploration of affective effects detailed above) becomes difficult in the light of the (more) supportive findings for CBM-I working in the first experiment, which the current study replicated in all but the control condition and extended in a simple and progressive way with its further conditions. Contemporary meta-analysis conducted on studies with moderate sample sizes such as the current pair naturally reveal considerable variations in demonstrated effect of this size and greater, and CBM-I techniques appear to be no exception to this (Cristea, Kok & Cuijpers, 2015).

The remaining explanation would be inter-experimental change in the participants' performance during the procedure itself. It is worth returning to the general theme of temporal variation in the population, considered in the affect-based explanations outlined above, but from a more cognitively-focused perspective. The vast majority of participants for both were undergraduate students, with comparable profiles on basic key variables such as age, gender and starting levels of the relevant variables. Affect aside, such populations have however been suggested to vary across academic semesters in their engagement with experimental procedures, an unmeasured capacity. There is however recent ongoing debate over the extent and overall significance of this for experimental procedures in general (see Zwaan, 2015), with it only being found to exert effects on certain study designs and not others. Whether CBM-I designs receive direct influence from these temporal factors remains to be directly evidenced, but such effects are feasible.

Distinctions between the two studies aside, the overall broader picture is that both displayed a considerably attenuated profile of performance for CBM-I programs than those in much contemporary published work for scrambled sentence task-measured bias.

Open Response Task Performance

Whilst some of the same explanations as those outlined above for the scrambled sentence tests may apply to the open response test outcomes, it is of note that findings for this were distinct from the SST results and even from those gained in the prior experiment. Additional design elements or further mechanisms which may have provoked these unexpected outcomes require scrutiny. Prior to this however, it is of note that the significant main effects indicating general increase in participant negativity on the response generation and response likelihood evaluation variables are concordant (on face value) with the "participant negativity increase" explanation offered earlier in the state anxiety section of this discussion. The further t-tests however do indicate considerable variation between groups occurring within this overall effect. In particular the open response control group appeared to account for a disproportionate portion of both effects, so it is worth considering what occurred in this group in more detail. It may be the case that participants who already had a degree of negativity to their overall interpretive tendency were able to accentuate this in the open response condition, with every negative response that they gave slightly increasing their overall negativity and priming them towards further negative responses. Thus by the end of the program this may have resulted in a considerable unplanned training effect.

The above said, other group changes did clearly contribute somewhat to the two overall main effects (or else a significant interaction would have been likely) so it is worth considering these for their individual significance and implications. The explicitly-instructed closed resolution training condition is a particular example: it exhibited significant change for generation of SJN responses. This was evidenced in the prior study as having no ability to exert significant effects on interpretive bias (at least in terms of the measures implemented). As is considered further in the following discussion this is not unusual: meta-analysis demonstrates that the same intervention may vary considerably in its demonstrated effect across trials. Alternatively or in addition this condition may, in a similar way to the uninstructed open response control program, sometimes simply serve as a platform for participants to get more negative. It may be fruitful to return to the role of temporal variables like state affect here: as mentioned in the section of this discussion dedicated to state anxiety, such an outcome could be via a temporal decline-type effect such as boredom or fatigue. As with the uninstructed open response program, the former is particularly feasible given that this version of the experimental program potentially required less engagement from participants. In the case of explicit training it is plausible that explicit knowledge of the rule they were going to need to implement at the start of the experiment negated the necessity for participants of considering many options for the completion of the word fragment. Positive solutions would be primed without much engagement. If CBM-I works primarily via a more profound production rule learning mechanism and not simply affective priming then positive priming could thus be a limiting and contradictory factor in this way, by inhibiting engagement in the rule learning process and limiting the transfer of a positive production rule.

Such findings should however be further contextualised in terms of the results of the first study. Here a small (significant) increase in generation also occurred in general, with explicitly-instructed CBM-I participants increasing but being the only individual group to increase non-significantly alone. As such there appears to be some variation, but a general tendency to produce more negative scenarios in response to ambiguity post-CBM-I training. It is difficult to explain this, but before attempting to do so it is useful to consider the results gained in terms of what they actually mean for the average participant. The observed outcomes appear to have been the result of a consistent small rise rather than one of large magnitude: the average participant in the current experiment produced point two one more social judgment related negative outcomes to every non SJN outcome for each scenario post-training, compared to pre-training. This means that the majority of participants, the majority of the time, did not even produce one more SJN response to *each scenario* they encountered, generally merely doing so consistently for around one scenario at each timepoint. Furthermore they only rated such SJN outcomes as just under six percent more likely to occur (zero point four of a unit on the scale employed). On the scale of change that CBM-I programs attempt to elicit, this is hardly a finding of magnitude and thus these results, whilst of statistical significance in places, should arguably be treated overall more like a null result than an inverse training effect and considered an artefact of natural noise variation on this variable with a limited sample size.

An under-emphasised element of the cognitive bias modification literature in general is the variation in participants' response to training. Participants in the current and prior study demonstrated notable inhomogeneity in their response to training, and participants existed in both datasets who performed in the reverse manner to the overall effects observed. This may not be an issue simply of participant disengagement with the procedure creating noise variance (as discussed above) but may indicate more substantial variation rooted in cognitive ability or personality variables.

There is an extensive literature on such moderators for other therapeutic techniques such as cognitive behavioural therapy (e.g.Hofmann, 2000; Zinbarg Uliaszek & Adler, 2008), so it is surprising that CBM-I, often cited as a potential clinical intervention, lacks a literature regarding what state and trait intrapersonal variables allow it to best exert effects. Considerable further research is necessary with the relevant target populations (on a scale beyond the scope of the current thesis) to establish any such individual differences-rooted moderating factors for CBM-I programs.

Linked to the above is the notion of a potential ceiling effect for certain participants. Participants were recruited for the study on the basis of trait social anxiety levels, with the assumption of a reasonable level of accompanying bias (whilst certain links have been established in initial interpretive bias studies, there has in fact been little large-scale correlational work on this association, and it is one of the aims of the cross-experimental chapter later in this thesis). However negativity of bias was slightly limited in many participants to begin with, meaning that many scenario resolutions during training would be concordant with the participants' first impressions and therefore might not be expected to result in a revolutionary modification effect. This is something particularly key if CBM-I does operate best by "surprising" participants with positivity rather than telling them to produce it. This might be particularly true for the explicitly-instructed open-ended training, as modification was rooted in the explicit instruction to produce a positive interpretation rather than in the positive constraints on a preset resolution.

Furthermore, statistical power may be an issue for the current study in terms of some of the null findings gained. At the time of inception, sample sizes were calculated based on effect sizes found in contemporary papers, in particular Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt's (2013) recent study. More recent review however (Cristea, Kok & Cuijpers, 2015) indicates CBM-I effect sizes in general to be more modest. Whilst the closest contemporary studies hold slightly more weight, due to procedural similarity, than the (notably procedurally heterogenous) field average in informing the current design, there is reason for caution here. It is possible that these studies were towards the upper end of any potential effect size distribution for such methods, were they to be replicated. This said, recruitment constraints would not have allowed a much bigger sample for the current study (as both this and the prior study sampled willing, highly socially anxious individuals this pool of participants was near exhaustion by the end of this second attempt).

Conclusion

The current study is thus interpreted primarily as a null result for the CBM-I training implemented. Whilst multiple explanations were explored, conclusive reasons for this were not forthcoming from the results gained. The key overall implication from the current study was careful design of further research to better account for the potentially extraneous elements that it was not possible to capture in the current design. The lack of control over state affect was a key factor of concern in the current design. Accordingly, it became necessary to consider a design in which state affect was either directly manipulated, explicitly controlled or more comprehensively measured. These considerations were highly influential in the design of the further research detailed in the following two chapters. The following chapter in particular also attempted to provide a degree of conceptual clarification on the operating mechanism of CBM-I, and to address concerns with ceiling effects and limited modifiability of bias in the participants studied. In this way, the potentially extraneous elements of the current design could be explored with specific and more detailed analysis.

Chapter Five: Experiments Three, Four and Five

Introduction

Prior experiments in this thesis provided mixed and primarily null results regarding the effectiveness of modifying negative interpretations of ambiguous material using cognitive bias modification for interpretation (CBM-I). Whilst some training variants administered demonstrated significant within-group change over time, experimental groups did not significantly differ from the relevant control groups. In the current chapter, a further three experiments are reported. Two of the experiments look to provide a degree of evidence on the mechanism of operation of CBM-I, and the bearing of work on embodied accounts of cognition on providing clarification on this debate. The remaining experiment returns to the investigation of standard implicit and explicit open response CBM-I begun in the prior chapter, and considers this in the novel procedural context implemented in the preceding experiments.

As noted in the introductory literature review of this thesis, there is a current lack of detail regarding the operating mechanism of CBM-I. Two accounts are outlined by Mathews and Mackintosh (2002). The first is a simple affective priming process, which would be a short-term general predisposition towards interpreting content in a manner concordant with the change in affect that the participant experiences. The second account, considered more plausible in the light of existing empirical work, is that CBM-I brings about the learning of a production rule for ambiguous content, which in its simplest manifestation would be summarised as “if I encounter ambiguity, I will create a positive resolution to this ambiguity”. This latter account is of greater clinical interest because if correct it suggests a relatively mood-independent and potentially more enduring cognitive process, which could be implemented by trained individuals regardless of transient situational factors.

Mathews and Mackintosh (2002) note however that these two processes may not necessarily be mutually exclusive, which makes empirical work to tease apart what effects of CBM-I are attributable to each more difficult. However, in the current chapter, it was decided to take an approach grounded in so-called “embodied” accounts (cf Barsalou, 1999) to provide evidence on a key conceptual factor with considerable practical implications for the implementation of CBM-I in a clinical intervention context.

To briefly give theoretical context to this approach, it is worth considering the context in which embodied accounts of cognition emerged. Such multimodal models of the close interplay between language, cognition, emotion and physical states are now prevalent and contemporary, due to a shift away from prior “symbolic” approaches to language, and yield a number of key insights that their predecessors were unable to (see Zwaan, 1999; Barsalou, 1999; 2008). Strict symbolic approaches to language cognition maintain that meanings are rooted in non-perceptual and amodal symbols. Embodied approaches meanwhile suggest that the interpretation of language is grounded in experience and perception, and stress the interconnectivity and integration of linguistic, perceptual and behavioural elements of cognitive processes in coming to an overall interpretive conclusion. They are rooted in a wide variety of more recent evidence, importantly from a variety of research designs. These include studies of the effects of motor actions on comprehension (e.g. Glenberg & Kaschak, 2002), the effects of spatial arrangement of objects on semantic judgments regarding those objects (e.g. Zwaan & Yaxley, 2003), observations of motor and premotor cortex activation when participants read action verbs (Hauk, Johnsrude & Pulvermuller, 2004), and findings that verbal stimuli referring to emotional expressions elicit similar facial muscle activity to that involved in performing expressions relevant to the emotion in question (Foroni & Semin, 2009).

Examples of the embodied, multimodal accounts of linguistic, cognitive and affective interplay that emerged to complement such findings include Barsalou’s (1999) highly influential theoretical account of conceptual understanding, which offers a detailed modelling of the storage and reactivation of perceptual symbols, and their organisation via simulators that integrate cross-modal aspects of phenomena. Perceptual symbols are emphasised as modal, and thus considered by Barsalou to be represented in the same cognitive system as the states that caused them to exist rather than distinct cognitive architecture. Specific incidents of perceptual input and the qualities perceived in response to them (the qualities “green” and “hot” are suggested examples) are organised in memory around a common frame, with selective attention analysing the perceptual components of the overall experience. Further incidences reactivate the sets of symbols and embellish upon the details of them, again in response to selective attention. As such, a simulator for such experiences is refined over time, which can be integrated with other simulators to form complex simulations. These can then be triggered by input from any modality, and activate associated elements across a range of other (importantly overlapping) modalities. The simulation is then constantly compared with incoming perceptual input, and modification of it, or activation of further simulators, occurs as a result.

Glenberg (1997) had previously introduced a similar idea to Barsalou with his concept of “mesh”, emphasising the central role of memory, and the balance between it and new environmental input, with both authors stressing the role of selective attention in this process. Zwaan (2003) advances the embodied perspective further with a particular focus on the comprehension of language. He proposes an “Immersed Experiencer Framework” lent to comprehension by multimodal overlap and integration, and demonstrates the advantages for comprehension that such a system would yield (thus making its existence and operation feasible in an evolutionary context). He emphasises (Zwaan, 2014) a word-learning example wherein a verbal label, stated function and known outcome for the use of a particular, previously unknown object are known by an individual. This information allows subsequent recognition (and correct verbal labelling) of this tool, despite it not having been directly perceived.

Details aside, the universal feature of such models is the suggestion of overlap between cognitive components of the relevant systems for linguistic, action-oriented and affective processing. Given this, it is somewhat surprising that no existing theoretical work (to the reviewer's knowledge) has considered cognitive biases from the explicit point of view of such multimodal interaction. In exploring and accounting for the role of embodied language processing in what are often linguistic tasks bias-focused researchers could feasibly improve the investigation of the biases considerably. Of particular salience is the way in which embodied accounts of cognition, language, behaviour and emotion can inform the experimental design of bias manipulation studies. Direct manipulation of simple actions, behaviours or expressions during CBM-I tasks for example may help to tease apart conflicting accounts of CBM-I, and thus elucidate the role of these potential interventions (as detailed below). Such investigation is concordant with the emphasis on caution with clinical implementation of CBM-I (in its current state) in this thesis, as outlined in the second chapter. To briefly summarise, statements of clinical suitability and utility have been made (The Economist, 2011) which are pre-emptive and in places unsubstantiated (Cristea, Kok and Cuijpers, 2015). It is argued that this lack of substantiation has two key components. Firstly, studies currently show considerable heterogeneity and no direct replications of key works exist in a published format, making comprehensive cross-study inference difficult (this is considered in further detail in chapters two and seven of the current thesis). Furthermore (and of greater relevance to the focus of the current chapter) there is an apparent paucity of bottom-up work on the systems involved in cognitive biases and their modification (and as such detailed theory on their operation), and this is particularly true for work concerning interpretive biases, and particularly salient for lexically-based ways of measuring and modifying them. Taking an embodied, or at least embodiment-informed, perspective on cognitive biases may provide a considerable contribution to the informed consideration of such systems in a body of work which currently lacks detailed consideration of the overlap of embodied behavioural, lexical cognitive and affective processes.

Applying such a critical eye (mindful of the interrelationship of the cognitive components of language, action and emotion) to CBM-I experimental designs may in particular prove useful for clarification of the relative roles of affective priming and rule-learning in the effects of CBM-I. If CBM-I primarily relies on an affective priming mechanism to exert its effect, then we would expect it to a). show effects similar to those demonstrated by interventions which are purely affective and b). be affected, in terms of its ability to exert change on interpretation, by short-term changes in affective state. A negative mood state would, for example, attenuate the effects of a positive CBM-I program. However if CBM-I mainly works via a production rule-learning process (something conceivably at a higher or more symbolic level of Zwaan's (2014) hierarchy) then we might expect it to outperform basic affective interventions in terms of effect on interpretations (and potentially other cognitive processes with scope for transfer) and operate relatively independently of affective priming from transient emotional stimuli, continuing to produce change in interpretation.

Certain studies have provided a degree of evidence for CBM-I being a rule-learning process and not a simple one of affective priming by making comparisons between it and simple mood inductions. For example, Standage, Ashwin and Fox (2010) found that CBM-I displayed a distinct pattern of effects to a music-based mood induction. The only explanation for such distinctness, if accurate is that CBM-I imparts a deeper process of rule-learning that is able to go beyond simple affective priming. It is important to investigate this further as simple embodied cognition tasks have been demonstrated to exert profound effects not just on mood but on basic interpretation processes in general, and elicit an emphasis on the overlap between these. Initial evidence was provided by Laird (1974) who applied electrodes to participants' faces (at positions on the eyebrows, mouth corners and jaw) and asked them to contract muscles at each of these points in two configurations, thus enabling the manipulation of facial muscles relevant to smiling or frowning, whilst viewing cartoons. Cartoons were rated funnier, and furthermore participants' mood improved, in the smiling condition compared to the frowning condition. Similarly Strack, Martin and Stepper (1988) had participants grip a pen in their mouth in either a way that would either facilitate or inhibit the facial muscles involved in a smile. They found this to exert a significant effect on the extent to which participants found a set of presented cartoons humorous. The effect has been replicated and extended beyond the specific domain of humour with a variant interference effect observed for valence in general. For example in an adapted task Havas, Glenberg and Rinck (2007) asked participants to make linguistic judgments about the valence and sensibility of emotionally valenced sentences whilst using the facial muscles involved in creating positive and negative expressions (via a similar pen task). They found consistent evidence over the course of multiple experiments for an interference effect: participants made these judgments faster when sentence valence and facial expression valence were congruent. The demonstrated effects are thus something that CBM-I needs to go beyond or at least account for as a valid clinical intervention candidate (this is particularly dealt with in the second experiment of this chapter, which considers the ability of such an intervention to interfere with or even accentuate the effects of a standard CBM-I program).

For CBM-I to work as a feasible intervention, it would thus need to not only be primarily distinct from affective priming but to go somewhat beyond the domain of linguistic-affective interaction and impart a highly concrete cognitive change in the form of a learned production rule. Studies of its potential for reducing clinical symptoms (e.g. Murphy, Hirsch, Mathews, Smith & Clark, 2007; Beard & Amir, 2008) can be seen as some evidence of this, but to a limited (and highly indirect) extent. Furthermore this production-rule account appears to have been assumed to be the mechanism, with little direct investigation in the literature. As such, direct manipulation which not just compares CBM-I with a mood intervention, but actively looks at its ability to resist interference from one, was deemed of considerable relevance.

A further related element of note in the current study was an interest in the breadth of the effects on processing initiated by participation in a course of CBM-I training. In addition to lack of detailed empirical information regarding how CBM-I procedures actually work there is the related question of the overall extent of their influence. CBM-I training outcomes have often historically been measured with tasks which are very “near” (in terms of format and modality) to the training issued. For example, CBM-I studies applying recognition tasks (see Salemink & Van den Hout, 2009) as bias measures apply scenario-based training, asking participants to complete partially obscured last words of scenarios that they read on a computer screen. Bias measurement then takes place via participants viewing the titles of each scenario followed by four distinctly-valenced variants of its ending, and rating these on a four point scale. As such participants may have (in the positive results gained) exhibited only a very limited change in one domain of processing and this may not transfer entirely to another. In the current study, more distal tasks were additionally sought, again to look at the limits of CBM-I.

There were a number of additional design-focused concerns that shaped the current set of studies. Firstly prior experimental work with CBM-I has looked at manipulation of interpretive bias in general towards both more positive and more negative interpretations and found both to be possible (e.g. Eysenck, 1991; see Hertel & Mathews, 2011 for a review). In the current experiment it was decided to attempt negative bias induction followed by a reversal of this with a subsequent positive induction. This was something that to the author’s knowledge has not been measured before (Certain CBM-I studies have attempted negative training and offered positive training afterwards for ethical reasons alone, without measurement). This was seen to yield two primary benefits. Firstly, as mentioned above, it was hoped that a suitable level of bias modification could be induced in participants both after the first intervention and following a second intervention of the inverse direction, thus better demonstrating the efficacy of the applied CBM techniques than single session training and potentially limited by ceiling effects. Secondly, in doing this, the research would be informative regarding the immediate plasticity and flexibility of interpretive bias. Furthermore, considering positive and negative training within this design allowed us to look at their interface with another key independent variable in this experiment: the embodied “valence priming” offered by the pen task, in more detail. Interference and facilitatory effects are therefore able to be considered in more depth, and in different combinations.

As the prior experiments had been concerned with the role that participants generating their own content might have in enhancing CBM-I training effects, this theme was also continued over to the current series of experiments and looked at via the addition of a further experimental condition. In terms of a unique element, as detailed above we were generally interested in the ability of participants to create a negative bias for themselves and then reverse this in this design. As such it became of interest whether the extent of either, or both, of these would be enhanced by having participants generate their own material rather than resolve pre-existing ambiguity. Furthermore there was interest in whether such training might be more or less resilient to intervening factors or open to facilitatory ones. There might, for example, be more scope for the affective priming task issued to take effect on participants’ interpretive processing when they generated their own ambiguity-resolving words rather than resolving preset word fragments. Candidate words of certain valence may become primed by a mood task for participants entering their own words, whereas this priming process would be limited by the constraints of the word fragment in the closed-resolution task. Conversely however, the expanded engagement necessary to complete the open response task might produce a more resilient bias and a more comprehensive rule-learning, less influenced by affective circumstance.

To summarise, the current studies were driven by a need to better understand CBM-I, its mechanism of action, and its limitations. Practically it was considered important to consider CBM-I’s comparative performance with a simple affective priming task, its ability to exert change in interpretive bias under potentially facilitatory and inhibitory affective conditions, and to compare the comparative efficacy of standard CBM-I and a potentially enhanced version developed for this thesis with a simple “valence priming” intervention. These were dealt with via distinct experiments. For the mood/interpretation valence priming, a simple behavioural task derived from Strack, Martin and Stepper’s (1988) procedure was applied, involving the innervation of muscles involved in facial expressions via a pen placed in the mouth. This allowed the manipulation of mood via a minimally invasive and disruptive intervention with prior empirical evidence for its effectiveness.

Such comparative performance was to be measured via both established and new, more comprehensive measures, and via tasks of near and far transfer effects. The scrambled sentence tasks with cognitive load and the open response interpretation measure (detailed in the prior experimental chapters and summarised again in the method section below) were again incorporated.

It was also hoped to do all of the above whilst quickly inducing an interpretive bias followed by a rapid reversal of this tendency in a single experimental session. A two-stage design and a number of specific hypotheses for their various outcomes were devised as a way of teasing apart the varying potential accounts of CBM-I outlined above. For clarity, these hypotheses are dealt with in the sections dedicated to the individual experiments below. However for further general design clarity figure seven below outlines the shared structure but distinct content of each of the constituent three experimental comparisons of this chapter (smaller versions of this diagram are included as reminders in each experimental section). As certain conditions are shared between experiments underlining, italicisation and bold font are used for ease of identification, with underlining representing inclusion in experiment one, bold font representing inclusion in experiment two and italicisation representing inclusion experiment three.

Figure 7: schematic of experimental conditions in this chapter, their combination into the three experimental comparisons, and participants' progress through the experimental design

<table>
<tr><td>Compared in experiment one</td><td colspan="2">Yes</td><td colspan="3"></td></tr>
<tr><td>Compared in experiment two</td><td></td><td colspan="3">Yes</td><td></td></tr>
<tr><td>Compared in experiment three</td><td colspan="3"></td><td colspan="2">Yes</td></tr>
<tr><td>Experimental group</td><td>Control: "Sham" CBM-I with mood induction</td><td>CBM-I only</td><td>CBM-I with negative mood induction</td><td>CBM-I with positive mood induction</td><td>Open response CBM-I with positive mood induction</td></tr>
<tr><td>Pre-experiment</td><td colspan="5">Consent form and standardised instructions</td></tr>
<tr><td>Affect measurement</td><td colspan="5">Fear of Negative Evaluation scale and PANAS</td></tr>
<tr><td>Bias measurement</td><td colspan="5">Scrambled sentence task

Open Response bias task</td></tr>
<tr><td>Training part one</td><td>CBM-I scenarios with unresolved ambiguity. Negative facial expression maintained with pen between lips</td><td>Negative resolution CBM-I</td><td>Negative resolution cognitive bias modification for interpretation. Negative facial expression maintained with pen between lips.</td><td>Negative resolution cognitive bias modification for interpretation. Positive facial expression maintained with pen between teeth.</td><td>Negative resolution open response cognitive bias modification for interpretation. Gripping of pen between teeth.</td></tr>
<tr><td>Affect measurement</td><td colspan="5">PANAS</td></tr>
<tr><td>Bias measurement</td><td colspan="5">Scrambled sentence task

Open Response bias task</td></tr>
<tr><td>Affect measurement</td><td colspan="5">PANAS</td></tr>
<tr><td>Training part two</td><td>CBM-I scenarios with unresolved ambiguity. Positive facial expression maintained with pen between lips.</td><td>Positive resolution CBM-I</td><td>Positive resolution cognitive bias modification for interpretation. Negative facial expression maintained with pen between lips.</td><td>Positive resolution cognitive bias modification for interpretation. Positive facial expression maintained with pen between teeth.</td><td>Positive resolution open response cognitive bias modification for interpretation. Gripping of pen between teeth.</td></tr>
<tr><td>Affect measurement</td><td colspan="5">PANAS</td></tr>
<tr><td>Bias measurement</td><td colspan="5">Scrambled sentence task

Open Response bias task</td></tr>
<tr><td></td><td colspan="5">Debrief</td></tr>
</table>

Experiment Three: Comparative performance of CBM-I and Pen Task

Design Rationale and Hypotheses

In this initial experiment, a simple direct comparison of CBM-I with a condition of affective priming and "sham" training was implemented, across one negative and then one positive training program.

Of note with the predictions made below is that they importantly allow for the pen task to potentially exert some influence on interpretation, something acknowledged as possible given the embodiment literature. The hypotheses thus emphasise quantitative superiority rather than qualitative difference. It was anticipated that such influence would be minimal given factors such as the closed-resolution nature of the "sham" CBM-I program completed by participants who received a pen task (as opposed to the free-interpretation tasks in which the influence of such tasks has been evidenced previously), but nonetheless there was considered scope for the pen task to exert some influence. What was instead predicted is that the CBM-I tasks would significantly exceed it.

Interpretive bias. It was hypothesised that the group receiving CBM-I only would show significantly more negative bias on the scrambled sentence task, and the generation, likelihood approximation and evaluation components of the open response bias task, than the group receiving the mood intervention only.

Regarding outcomes of the second CBM-I program, it was hypothesised that the group receiving CBM-I only would show significantly greater reduction in these negative bias measures than the group receiving pen task and "sham" CBM-I task.

Mood. Whilst there is some evidence that CBM-I is able to exert change on immediate experienced emotion, this is mixed overall (Hallion & Ruscio 2011), with significant changes usually occurring in studies with multiple training sessions. As such for the first training program it was hypothesised that the group receiving CBM-I would show no significant change in positive or negative mood, that the group receiving the pen task and inactive control CBM-I training would show a significant increase in negative mood and decrease in positive mood on the PANAS scale, and that this would be significantly greater than any change that might occur in the CBM-I group.

Regarding the second session of CBM-I or control program, it was again predicted that the group receiving CBM-I would show no significant change in positive or negative mood, and that the group receiving the (positive) pen task and inactive control CBM-I training would show a significant decrease in negative mood and increase in positive mood on the PANAS scale, and that this would be significantly greater than any non-significant change in the CBM-I group.

Method

Design

A two-part experimental design was used, applying two consecutive sets two by two analyses to analyse the two consecutive training programs applied separately.

For each program (negative valence and positive valence), time of testing again formed the within-groups independent variable, with two levels of measurement: pre-training, and post-training, and experimental condition formed a further independent variable, this time with two conditions of CBM-I alone and pen task alone with "sham" CBM-I task.

Dependent variables measured were positive and negative affect and anxiety (self-report), interpretive bias (as measured by a scrambled sentence task), interpretive bias in response generation, selection and evaluation (as measured by an open response bias measure).

Participants

Forty-four participants (thirty-three females, fourteen males) were recruited from the University of East Anglia campus via posters and electronic advertisement, including emailed bulletins and the School of Psychology online recruitment system. Participants had a mean age of 20 years one month, were all over 18, and had received no treatment for mental health problems (self-disclosed) in the past year. Participants were paid ten pounds for participation or alternatively opted to receive the equivalent in research participation credits from the School of Psychology system.

Materials

Social Anxiety Participants completed the Fear of Negative Evaluation Scale (FNE: Watson & Friend, 1969) in the same manner as the prior studies. However in the current study this measure was used to account for the role of trait social anxiety and provide data for subsequent cross-experimental analysis in this thesis and as such did not have a hypothesis directly relating to it in the current study.

State Affect. The Positive and Negative Affect Scale (PANAS: Watson, Clark & Tellegen, 1984) was issued to participants at four points in the experimental procedure. The PANAS is a twenty-item scale: each item is an adjective, next to which participants indicate how much it describes their current mood using a number from one (very slightly or not at all) to five (extremely). The PANAS yields two subscales: overall positive affect and overall negative affect. For reasons of experimental brevity (see discussion) two questions from the PANAS were also used as an indicator of participants' current state anxiety level: participants gave one to five ratings of "jittery" and "nervous".

Overall interpretive bias. As an overall (and literature-consistent) measure of interpretive bias, scrambled sentence tasks were administered to participants pre- and post-training as a pencil and paper task, in the format used by Standage, Ashwin and Fox (2010) with expanded content from Bowler, Mackintosh, Dunn, Mathews Dalgleish and Hoppitt (2012). This was in the same manner as the prior experiments (see chapter three for details of this task and exemplar content), except that participants only completed a task under conditions of load and no non-load task due to an already lengthy experimental procedure. Content of the tasks was again identical to that applied in the prior experiment and thus is included in appendix three.

Bias in interpretation generation, selection and evaluation. The open response task incorporated in the prior two experiments was additionally applied, in order to consider in more detail which aspects of processing, if any, were subject to interpretive bias in the participants at each point in the experiment. To summarise the aims of this measure (which are detailed elsewhere in this thesis in more detail), of interest was whether participants generated negative material, whether they perceived any negative material that they generated as more negative, and whether they perceived such material as more likely to occur. The task was administered at three points in the experimental design: pre-training, post-first CBM-I or control program and post-second CBM-I or control program. Figure seven illustrates participants' progress through the design in this way for clarity. Content for this task is included in appendix four.

Cognitive Bias Modification program. The procedure for modifying interpretive bias used was a computerised, scenario-based procedure, similar to that applied by Bowler, Mackintosh, Dunn, Mathews, Dalgleish and Hoppitt (2012) and is detailed in full in chapter three of the current thesis with exemplar content. Participants in the active training conditions received a program in which the word fragments constrained them to negatively resolving each scenario in the first training session, followed by one in which the fragments constrained them to positive resolutions of each scenario. Participants who received the inactive control version of this task received a version in which the word fragments were instead engineered so that they *maintained* the ambiguity of the scenario in the same manner as that issued in chapter three. Again, figure one illustrates participants' progress through the design in this way.

Pen task. Participants in one of the experimental groups completed two simple pen tasks during their "sham" inactive CBM-I program. During the program they were asked to grip a pen between their lips in a task that draws upon the facial musculature necessary for negative expressions of frowning or pouting in a manner demonstrated by the experimenter, as per the successful manipulation applied by Strack, Martin and Stepper (1988). During the second block of inactive CBM-I scenarios participants were then asked to grip their pen between their teeth (positive pen task) – a task that draws upon the facial musculature necessary for smiling (also demonstrated by the experimenter), but in an implicit way (participants are unaware of this manipulation, being told that this element of the study considers multitasking). Participants were told that they should keep the pen in position whilst answering the CBM-I scenarios, but that they could remove it in the breaks between each block of ten scenarios.

Procedure

Due to the multi-condition, multi-measure nature of the experimental procedure, participants' passage through the experiment has been re-summarised in figure 1.

Figure 8: the experimental procedure

Experimental group	CBM-I only	Control: "Sham" CBM-I with Pen task
	Consent form and standardised instructions	
Affect measurement	Fear of Negative Evaluation scale and PANAS	
Bias measurement (Randomised order)	Scrambled sentence task Open Response bias task Dot-probe task	
Training part one	Negative resolution CBM-I	CBM-I scenarios with unresolved ambiguity. Negative facial expression maintained with pen task.
Affect measurement	PANAS	
Bias measurement (Randomised order)	Scrambled sentence task Open Response bias task Dot-probe task	
Affect measurement	PANAS	
Training part two	Positive resolution CBM-I	CBM-I scenarios with unresolved ambiguity. Positive facial expression maintained with pen task.
Affect measurement	PANAS	
Bias measurement (Randomised order)	Scrambled sentence task Open Response bias task Dot-probe task	
	Debrief	

Results

Participant characteristics, Data Screening, Treatment and Exclusions

Levene's tests were performed to check homogeneity of variance for each group on each of the studied variables. Results indicated that there was no significant deviation from homogeneity of variance for any of the variables used. Normality procedures indicated generally satisfactory normality, except for time one scrambled sentence task scores and time two and three evaluation scores. Once again it was decided that the robustness of the ANOVA model could be relied upon (particularly as there was a benefit to keeping statistical procedures in close alignment with the prior experiment and nowhere did non-normal data constitute the majority of data for any one analytical technique) and no transformations were undertaken. No outlier removal was implemented beyond participants failing to complete the measures correctly was implemented. For the latter, participants' data was excluded for a measure if they had failed to correctly complete a measure according to the instructions given: this was particularly the case for the open response measure where not entering a complete response for at least two of the scenarios at each timepoint was considered to be failing to complete the measure. Furthermore two participants chose to leave the experiment at timepoint two and as such did not contribute data from this point onwards. At the end of this process thirty six participants remained for bias measure data. From the twenty two participants gathered for each experimental group final numbers were seventeen in the implicit CBM-I only group and nineteen in the group receiving control CBM-I and pen task only.

Participants in the two experimental groups had an average age of twenty years and did not differ significantly in their gender balance, initial levels of attentional control, negative affect, scrambled sentence score under conditions of cognitive load, and their initial interpretive bias response generation, response selection and response evaluation. Participants did differ in terms of their state positive affect, but due to the moderate nature of this and the robustness of the analytical procedure applied it was decided that this data would be analysed untransformed in the standard ANOVA model applied to the remainder of the variables. Table nine displays the relevant means, standard deviations and t-test outcomes for these characteristics.

Table 9: Pre-training means of attentional control, positive and negative affect, scrambled sentence test score (load), and response generation, selection and evaluation bias scores.

Variable	CBM-I only group	Sham CBM-I and mood induction group	*t*
State anxiety	3.31 (1.49)	3.42 (1.43)	.22
State positive affect	25.94 (5.96)	31.11 (6.4)	2.56*
State negative affect	12.29 (2.82)	12.62 (2.68)	.42
Scrambled sentence test score (load)	21.42 (13.67)	23.77 (22.1)	.4
Response generation bias score	.19 (.08)	.21(.09)	.69
Likelihood approximation bias score	3.64 (1.1)	3.06 (.94)	1.73
Response evaluation bias score	2.18 (.95)	2.09 (.65)	.33

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

Higher scores on the SST indicate higher proportions of negative resolutions made.

Emotion

Positive affect. A mixed ANOVA was conducted on the positive affect data from the PANAS scales administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 35) = 14.68$, $p = .001$, was observed. Corrected t-tests (table ten) suggested that within this there was a significant decrease in positivity of mood in the CBM-I only group alone, which withstood Bonferroni correction to remain significant. A significant main effect of group, $F(1, 35) = 10.33$, $p = .003$ was observed. Corrected t-tests (table ten) suggested that there were significant differences between groups at both time points, with the CBM-I only group being significantly lower in positive affect at both timepoints. No significant interaction effect between time and group was observed, $F(2, 35) = 2.2$, $p = .15$.

Table 10: Comparisons of positive affect scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only mean	25.94 (5.6)	21.41 (6.19)	4.19***	16	.81
Sham CBM-I with negative mood induction mean	31.1 (6.4)	29.1 (7.57)	1.55	18	.31
t	2.36*	2.82*			
df	34	34			
d	.92	.94			

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the on the positive affect data from the PANAS scales administered pre- and post-second (positive) CBM-I training session. A significant main effect of time, $F(1, 35) = 4.54$, $p = .04$, was observed. Corrected t-tests (table eleven) suggested that within this there was a significant decrease in positivity of mood in the inactive CBM-I, positive pen task condition alone. A significant main effect of group, $F(1, 35) = 6.9$, $p = .01$, was observed Corrected t-tests (table eleven) suggested significant differences between both groups at both time points, but that a stronger effect was present in the CBM-I only group which was still present after Bonferroni correction. No significant interaction effect between time and group was observed, $F(2, 35) = 1.03$, $p = .32$.

Table 11: Comparisons of positive affect scores pre- and post-second training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	20.47 (7.43)	19.68 (7.74)	4.19***	16	.1
Sham CBM-I with positive mood induction	27.83 (8.6)	25.61 (8.17)	2.16*	18	.26
t	2.79**	2.35*****			
df	33	33			
d	.99	.77			

* = p< .05, ** = p < .01 *** = p < .001.

***** Bold asterisks indicate that the test still reaches a significance level of p<.05 with appropriate Bonferroni correction applied.

Standard deviations appear in parentheses.

In overall summary for positive affect there was a general tendency for participants' mood to decrease across both training programs, although this was not significant at a within-groups level in the active implicit CBM-I condition across the second training program. Strong between groups differences were maintained across time points.

Negative affect. A mixed ANOVA was conducted on the negative affect data from the PANAS scales administered pre- and post the first CBM-I training session. No significant main effect of time, $F(1, 35) = .14$, $p = .71$, was observed. No significant main effect of group, $F(1, 35) = .81$, p = .38, was observed. A significant interaction effect between time and group was observed, $F(2, 35) = 12.88$, $p = .001$. Corrected t-tests (table twelve) suggested that within this there was a significant increase in negativity in the CBM-I only group and a significant decrease in negativity in the pen task with sham CBM-I group and that both were present after the appropriate Bonferroni correction.

Table 12: Comparisons of negative affect scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	12.29 (2.82)	13.59 (3.00)	2.49*	16	1.26
Sham CBM-I with negative mood induction	12.68 (2.69)	11.63 (2.65)	2.58*	18	.39

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the negative affect data from the PANAS scales administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 35) = .03$, $p = .86$, was observed. No significant main effect of group, $F(1, 35) = .23$, p = .63 was observed. No significant interaction effect between time and group was observed, $F(2, 35) = 1.1$, $p = .3$.

To provide an overall summary of negative mood distinct group performances across the first training program, with the CBM-I only group increasing and the pen task/sham CBM group decreasing in negativity, were not sustained over the second program in which no significant change occurred.

Anxiety. A mixed ANOVA was conducted on the anxiety only data from the PANAS scales administered pre- and post the first CBM-I training session (which featured training in negative resolutions for the active experimental group). A significant main effect of time, $F(1, 35) = 13.78$, $p = .001$, was observed. Corrected t-tests (table thirteen) suggested that there was a significant decrease in anxiety in the group receiving inactive CBM-I and a negative pen task only, which was still present after Bonferroni correction. No significant main effect of group, $F(1, 35) = .12$, p = .74 was observed. No significant interaction effect between time and group was observed, $F(2, 35) = 2.31$, $p = .14$.

Table 13: Comparisons of anxiety scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	3.31 (1.49)	2.94 (1.57)	1.38	16	.25
Sham CBM-I with positive mood induction	3.42 (1.43)	2.57 (1.12)	4.16***	18	.72

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the anxiety only data from the PANAS scales administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 35) = 1.26$, $p = .27$, was observed. No significant main effect of group, $F(1, 35) = .17$, p = .68, was observed. No significant interaction effect between time and group was observed, $F(2, 35) = .03$, $p = .87$.

To summarise anxiety findings, there was a tendency for anxiety reduction over the first program which was significant in the sham CBM-I and pen task group only. No such difference existed across the second program.

Interpretation

Scrambled sentence task data. A mixed ANOVA was conducted on the interpretive bias indices from the scrambled sentence tests administered pre- and post the first CBM-I training session. No significant main effect of time, $F(1, 35) = 1.42$, $p = .24$, was observed. No significant main effect of group, $F(1, 35) = .12$, p = .74 was observed. No significant interaction effect between time and group was observed, $F(1, 35) = 2.37$, $p = .13$.

A further mixed ANOVA was conducted on the interpretive bias indices from the scrambled sentence tests administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 35) = .01$, $p = .96$, was observed. No significant main effect of group, $F(1, 35) = .58$, p = .45, was observed. No significant interaction effect between time and group was observed, $F(1, 35) = .26$, $p = .62$.

Scrambled sentence scores thus demonstrated no significant differences

Generation. A mixed ANOVA was conducted on the generation scores from the open response bias measures administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 35) = 12.2$, $p = .001$, was observed. Corrected t-tests (table fourteen) suggested that there was significant increase in the amount of negative endings generated by participants in the CBM-I group alone, still present after Bonferroni correction. No significant main effect of group, $F(1, 35) = 1.35$, $p = .25$ was observed. No significant interaction effect between time and group was observed, $F(2, 35) = .72$, $p = .4$.

Table 14: Comparisons of generation bias negativity scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	.21 (.09)	.3 (.17)	2.81*****	16	-1
Sham CBM-I with positive mood induction	.19 (.08)	.24 (.12)	2.05	18	-.71

* = p< .05, ** = p < .01 *** = p < .001.

***** Bold asterisks indicate that the test still reaches a significance level of p<.05 with appropriate Bonferroni correction applied.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the generation scores from the open response bias measures administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 35) = .58$, $p = .45$, was observed. No significant main effect of group, $F(1, 35) = 2.6$, $p = .12$ was observed. No significant interaction effect between time and group was observed, $F(2, 35) = .22$, $p = .64$.

In summary of response generation findings, the only finding of statistical significance was that there was a tendency for increase in the generation of social judgment-relevant negative responses over the first program which was significant in the sham CBM-I and pen task group only.

Likelihood Approximation. A mixed ANOVA was conducted on the likelihood estimate scores from the open response bias measures administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 35) = 5.71$, $p = .02$, was observed. Corrected t-tests (table fifteen) suggested that there was significant increase in the perceived likelihood of negative endings by participants in the CBM-I group alone which was within significance when a Bonferroni correction was applied. No significant main effect of group, $F(1, 35) = 1.8$, $p = .19$, was observed. No significant interaction effect between time and group was observed, $F(2, 35) = .92$, $p = .34$.

Table 15: Comparisons of likelihood approximation bias negativity scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	3.06 (.94)	3.68 (1.14)	2.74*****	16	.45
Sham CBM-I with positive mood induction	3.64 (1.1)	3.91 (1.13)	.91	18	.24

* = p< .05, ** = p < .01 *** = p < .001.

***** Bold asterisks indicate that the test still reaches a significance level of p<.05 with appropriate Bonferroni correction applied.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the likelihood estimate scores from the open response bias measures administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 35) = .2$, $p = .65$, was observed. No significant main effect of group, $F(1, 35) = .28$, $p = .6$, was observed. No significant interaction effect between time and group was observed, $F(2, 35) = .43$, $p = .52$.

To summarise significant likelihood findings briefly, there was an increase in participants' perceptions of likelihood at after the first training program which reached significance in the CBM-I only group alone.

Evaluation. A mixed ANOVA was conducted on the ending evaluation scores from the open response bias measures administered pre- and post the first CBM-I training session. No significant main effect of time, $F(1, 35) = .66$, $p = .42$, was observed. No significant main effect of group, $F(1, 35) = .17$, $p = .68$, was observed. No significant interaction effect between time and group was observed, $F(2, 35) = .07$, $p = .8$.

A further mixed ANOVA was conducted on the ending evaluation scores from the open response bias measures administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 35) = 3.14$, $p = .09$, was observed. No significant main effect of group, $F(1, 35) = .39$, $p = .54$, was observed. No significant interaction effect between time and group was observed, $F(2, 35) = .16$, $p = .69$.

As such, no significant findings were gained from data on participants' evaluations of the material they generated pre-and post-either training program.

Discussion

The outcomes of the comparison between the CBM-I only group and the control "sham" CBM program partially supported the overall hypothesis that the participants in the CBM-I only group would show significantly greater modification of bias than those in the pen task only (with inactive control CBM-I) condition. Specifically, significant increases in bias were demonstrated for participants' generation of negative responses and likelihood perception of negative responses in the group who received active CBM-I training, but not for their overall evaluation of these responses or the overall valence of their ambiguity resolution on the scrambled sentence task. Furthermore, these effects were specific to the first (negative) training program only.

For mood meanwhile, predictions regarding between group differences in mood change over time were not supported. What was found instead was a general tendency for participants to become more negative across both conditions, accompanied by some maintenance of initial between-group differences in affect throughout the experiment. There are a number of potential explanations for this. One may lie in the experimental design itself – it may be that participants found this tedious, tiresome or anxiety-invoking in general. Alternatively (or in addition), another explanation may lie in the fact that both groups completed negatively-valenced tasks during the experiment. The active CBM-I task involved exposure to consistently negative material. The pen task was expected to induce a negative mood, so under one interpretation the first set of results for this group are concordant with this. The unexplained element is why participants' mood did not recover in response to the second pen task. Possible explanations include fatigue. Whilst regular breaks were incorporated (as detailed in the method section for this experiment these were at the same times as the breaks in the blocks of displayed ambiguous scenarios that participants completed, so occurred every ten scenarios) several participants self-reported that the pen task was nonetheless tiring for their facial muscles, and this may have contributed to further decrease mood.

Aside from cross-group effects, if we consider the pen task intervention to have worked well at time one and then participants to have struggled to recover from this (possibly due to a cumulative effect of fatigue or a similar temporal variable with affective effects), other explanations become possible as contributors for the outcomes for the CBM-I group. Use of negative resolution CBM-I is relatively rare and may be more affectively salient to participants in general. It may create surprise:

participants in the current experiment were drawn from the normal population and overall did not have extreme levels of negative interpretive tendency at the beginning of the experiment. As such being confronted with repeated negative resolutions, in direct contrast with their expectations, may have been particularly salient for them. Furthermore, there may be a primacy effect in which exposure to initial negative material establishes a lasting cognitive state for the entire experiment which is difficult to recover from (and potentially compounded by boredom, or small increases in negative emotions like anxiety as the experiment progressed).

The between-group inhomogeneity in mood is slightly problematic: an extraneous starting difference in mood was present between the groups (detail) with persisted over time. Its effect is hard to assess, and dependent upon the mechanism of action of CBM-I as considered in the introduction of this section. A rule-learning explanation (which as outlined is currently favoured within and receives a small amount of supportive evidence from contemporary literature) would suggest minimal influence. However if a reasonable role for affective priming is assumed in interpretive biases such a difference may have reduced training effects and potentially even prevented a significant interaction from being observed. This may have been an aspect which a larger sample size would have standardised and eradicated but was not feasible due to recruitment constraints.

Further combined discussion on the outcomes of the current and following experiments is undertaken at the end of this chapter. The subsequent experiment goes beyond the comparison undertaken in the current chapter and existing work (e.g. Standage, Ashwin & Fox, 2010) to look at the ability of mood manipulation to accentuate or limit CBM-I outcomes.

Experiment Four: Interaction of CBM-I and Mood Intervention

Design Rationale and Hypotheses

In this experiment, a number of further issues were addressed. CBM-I training programs designed to impart both positive and negative biases were applied in combination with pen tasks of concordant and discordant valence, and compared with a control condition of the CBM-I tasks only. As such, further evidence would be provided regarding the independence of CBM-I from mood, which as the introduction to this chapter details is important both pragmatically and conceptually. The independence account was thus considered via a test of the following hypotheses.

Mood. It was anticipated that the pen task would exert an effect on overall positive and negative affect when implemented. Accordingly it was predicted that the group receiving the positive pen task would exhibit significant decrease in negative mood and increase in positive mood from pre-to-post training at both time points (with the concordant significant differences between change in these three conditions at each time point). It was predicted that the group receiving the negative pen task would exhibit significant decrease in negative mood and increase in positive mood from pre-to-post training at both time points. Lastly it was predicted that the group receiving CBM-I only with no pen task would not exhibit any significant change in mood over the time course of the experiment.

Interpretive bias. Regarding change over the duration of the first, negative, training program it was predicted that all groups would exhibit a significant increase in negative interpretive bias (again as evidenced by the scrambled sentence task score and the generation, likelihood approximation and evaluation elements of the open response bias task). It was assumed that CBM-I would operate via a mechanism that would be resistant to the mood manipulation exerted by the pen task, and as such it was predicted that there would be no significant between-groups differences in the amount of this change in the three groups considered.

For the second training, further training valence-concordant change in interpretive bias was predicted. As such it was predicted that there would be a significant decrease in negativity of interpretive bias (again as measured by all four of the bias indices) in all three groups post-training. It was again predicted that there would be no significant between-groups differences in the amount of this change in the three groups considered.

Method

Design

An experimental design was used, applying a double three by two design to analyse the two consecutive bias training (negatively-valenced and positively-valenced) programs applied separately.

For each program, time of testing formed the within-groups independent variable, with two levels of measurement: pre-training, and post-training.

Experimental condition formed a further independent variable, with three conditions: word completion CBM-I with “positive” pen task, word completion CBM-I with “negative” pen task, word completion CBM-I only.

Dependent variables applied were identical to those issued in the first experiment.

Participants

Forty further participants (twenty-eight females, twelve males) were recruited from the University of East Anglia campus via posters and electronic advertisement, including emailed bulletins and the School of Psychology online recruitment system, using the same criteria as those outlined for the prior experiment. These were compared with the eighteen participants (twelve females, and six males) from the group receiving the active CBM-I program from the prior experiment. Participants had a mean age of nineteen years and eight months.

Materials

Measurement materials used were identical to those applied in the prior experiment, with the same basic order, and counterbalancing procedures applied. The only distinction was the experimental conditions applied. For these, participants in two new CBM-I groups did the simple pen tasks described in the prior experiment whilst they completed active CBM-I training. They did not switch pen tasks like the group in the prior study, instead each group had a single pen task to do throughout both blocks of CBM-I training. As such one group did a negative pen task throughout, one group did a positive pen task throughout, and these were compared with the existing group who completed CBM-I with no pen task from the prior experiment.

Procedure

A further procedural diagram is presented below in figure nine with the updated conditions of this experiment included.

Figure 9: the experimental procedure

Experimental group	CBM-I with positive pen task	CBM-I with negative pen task	Control: CBM-I only
	Consent form and standardised instructions		
Affect measurement	Fear of Negative Evaluation scale and PANAS		
Bias measurement (Randomised order)	Scrambled sentence task Open Response bias task		
Training part one	Negative resolution cognitive bias modification for interpretation. Gripping of pen between teeth.	Negative resolution cognitive bias modification for interpretation. Gripping of pen between lips.	Negative resolution cognitive bias modification for interpretation.
Affect measurement	PANAS		
Bias measurement (Randomised order)	Scrambled sentence task Open Response bias task		
Affect measurement	PANAS		
Training part two	Positive resolution cognitive bias modification for interpretation. Gripping of pen between lips.	Positive resolution cognitive bias modification for interpretation. Gripping of pen between teeth.	Positive resolution cognitive bias modification for interpretation.
Affect measurement	PANAS		
Bias measurement (Randomised order)	Scrambled sentence task Open Response bias task		
	Debrief		

Results

Participant Characteristics Data Screening, Treatment and Exclusions

Identical exclusion criteria were applied to the prior experiment. At the end of this process, fifty seven participants contributed data, which dropped slightly to fifty five for the PANAS scores due to participant omission of necessary items.

A further set of Levene's tests were performed to check homogeneity of variance for each group on each of the studied variables. Again results indicated that there was no significant deviation from homogeneity of variance for any of the variables used. Participants did not differ significantly in their initial levels of positive affect, negative affect, scrambled sentence score under conditions of cognitive load, and their initial interpretive bias response generation, response selection and response evaluation. Table sixteen displays the relevant means, standard deviations and analysis of variance outcomes for these characteristics. Normality procedures indicated generally satisfactory normality, except for some deviations at the second and third points of measurement for the CBM-I only and pen task/sham CBM-I groups respectively for the evaluation variable. Due to primarily normal data for this variable (only one quarter of each of the necessary datasets was thus not normal due to the analysis necessary, as detailed in the design and results sections below) the robustness of the ANOVA model was again relied upon with no transformations undertaken. Outlier removal gain consisted of only those participants who failed to complete measures correctly. The result was a reduction from the twenty participants gathered for each experimental group to final numbers of seventeen in the implicit CBM-I only group and twenty in the group receiving control CBM-I and pen task only and twenty in the implicit CBM-I plus negative pen task.

Table 16: Pre-training means of state affect, scrambled sentence test score (load), and response generation, selection and evaluation bias scores.

Variable	CBM-I only	CBM-I with positive pen task	CBM-I with negative pen task	F
State anxiety	3.31 (1.49)	2.9 (1.25)	2.8 (1.23)	1.45
State positive affect	25.94 (5.6)	27 (6.08)	27.11 (4.53)	.27
State negative affect	12.29 (2.82)	12.3 (4.08)	12.57 (3.92)	.02
Scrambled sentence test score (load)	21.42 (13.67)	29.99 (18.36)	19.3 (11.02)	3.04
Response generation bias score	.21 (.09)	.24 (.1)	.22 (.07)	2.74
Likelihood approximation bias score	3.06 (.94)	3.36 (.97)	2.93 (.9)	3
Response evaluation bias score	2.09 (.65)	1.8 (55)	1.77 (.58)	.1

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parenthesis.

Higher scores on the SST indicate higher proportions of negative resolutions made.

Emotion

Positive affect. A mixed ANOVA was conducted on the positive affect data from the PANAS scales administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 55) = 33.2$, $p < .001$, was observed. Corrected t-tests (table seventeen) suggested that this was comprised of significant negative change in all three CBM-I groups, all of which withstood Bonferroni correction. No significant main effect of group, $F(2, 55) = .21$, p = .82, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = .06$, $p = .95$.

Table 17: Comparisons of positive affect scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	25.94 (5.6)	21.41 (6.19)	4.19***	16	.81
CBM-I with positive pen task	27 (6.08)	21.85 (7.33)	3.08**	19	.84
CBM-I with negative pen task	27.15 (4.53)	22.47 (6.52)	3.37**	18	1.02

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the on the positive affect data from the PANAS scales administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 55) = .96$, $p = .33$, was observed. No significant main effect of group, $F(1, 55) = .17$, p = .85, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = 1.03$, $p = .36$.

The sole significant finding to emerge from the analysis of participants' positive affect was thus that there was a significant decrease across the first training program.

Negative affect. A mixed ANOVA was conducted on the negative affect data from the PANAS scales administered pre- and post the first CBM-I training session. No significant main effect of time, $F(1, 55) = 3.84$, $p = .06$, was observed. No significant main effect of group, $F(2, 55) = .05$, p = .95, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = .92$, $p = .41$.

A further mixed ANOVA was conducted on the negative affect data from the PANAS scales administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 55) = 1.62$, $p = .21$, was observed. No significant main effect of group, $F(2, 55) = .54$, p = .59, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = .24$, $p = .79$.

No significant change was thus observed in the negative effect data.

Anxiety. A mixed ANOVA was conducted on the anxiety only data from the PANAS scales administered pre- and post the first CBM-I training session. No significant main effect of time, $F(1, 55) = 2$, $p = .16$, was observed. No significant main effect of group, $F(2, 55) = .6$, p = .55, was observed. No significant interaction effect between time and group was observed, $F(1, 55) = .23$, $p = .8$.

A further mixed ANOVA was conducted on the anxiety only data from the PANAS scales administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 55) = .41$, $p = .53$, was observed. No significant main effect of group, $F(2, 55) = .53$, p = .59, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = .04$, $p = .96$.

As with negative affect, no significant effects were observed throughout the anxiety data.

Interpretation

Scrambled sentence task data. A mixed ANOVA was conducted on the interpretive bias indices from the scrambled sentence tests administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 55) = 5.04$, $p = .03$, was observed. Corrected t-tests (table eighteen) suggested that this was not composed of any significant effects for individual groups. No significant main effect of group, $F(2, 55) = 3.15$, p = .05, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = .42$, $p = .66$.

Table 18: Comparisons of scrambled sentence scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	21.42 (13.67)	29.44 (7.23)	2.02	16	.58
CBM-I with positive pen task	29.99 (18.36)	36.5 (25.18)	1.23	19	.36
CBM-I with negative pen task	19.29 (11.02)	21.83 (19.29)	.67	18	.23

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the interpretive bias indices from the scrambled sentence tests administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 55) = .87$, $p = .35$, was observed. No significant main effect of group, $F(2, 55) = 2.4$,

$p = .1$, was observed. No significant interaction effect between load and group was observed, $F(3, 55) = .25$, $p = .78$.

As such the only significant finding for scrambled sentence task performance, of increase over the first training program in level of bias, did not reach significance at individual group levels.

Generation. A mixed ANOVA was conducted on the generation scores from the open response bias measures administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 55) = 21.34$, $p < .001$, was observed. Corrected t-tests (table nineteen) suggested that there was significant increase in the amount of negative endings generated by participants in all three groups, but that this did not meet significance after a Bonferroni correction for multiple tests was applied in the group receiving the positive pen task. No significant main effect of group, $F(2, 55) = .37$, $p = .69$, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = .1$, $p = .91$.

Table 19: Comparisons of generation bias scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	.21 (.09)	.3 (.17)	2.81*****	17	1
CBM-I with positive pen task	.24 (.1)	.32 (.14)	2.38*	18	.84
CBM-I with negative pen task	.22 (.07)	.29 (.12)	2.84*****	20	1.04

* = p< .05, ** = p < .01 *** = p < .001.

***** Bold asterisks indicate that the test still reaches a significance level of p<.05 with appropriate Bonferroni correction applied.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the generation scores from the open response bias measures administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 55) = 2.57$, $p = .12$, was observed. No significant main effect of group, $F(2, 55) = .85$, $p = .43$, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = 1.04$, $p = .36$.

In summary a small increase in number of negative endings generated was present in all groups after the first training program. These, and the associated main effect. formed the only significant differences in this analysis.

Likelihood Approximation. A mixed ANOVA was conducted on the likelihood estimate scores from the open response bias measures administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 55) = 14.2$, $p < .001$, was observed. Corrected t-tests (table twenty) suggested that there was significant increase in how likely participants estimated negative endings in the CBM-I only and CBM-I with negative pen task groups, and not in the CBM-I with positive pen task group, but that none of these groups reached significance at a p<.05 level when a Bonferroni correction was applied. No significant main effect of group, $F(2, 55) = .39$, p = .68, was observed. The difference between group change over time was not sufficient to produce a significant interaction effect between time and group, $F(3, 55) = .33$, $p = .72$.

Table 20: Comparisons of likelihood approximation bias scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
CBM-I only	3.06 (.94)	3.68 (1.14)	2.7*	17	.66
CBM-I with positive pen task	3.36 (.97)	3.8 (1.39)	1.46	18	.45
CBM-I with negative pen task	2.93 (.9)	3.68 (1.45)	2.6*	19	.83

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the likelihood estimate scores from the open response bias measures administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 55) = .25$, $p = .62$, was observed. No significant main effect of group, $F(2, 55) = .05$, p = .91, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = 1.25$, $p = .3$.

To summarise likelihood findings, a general tendency to increase estimations of the likelihood of social judgment was present but did not reach significance in the group who received CBM-I with a positive pen task.

Evaluation. A mixed ANOVA was conducted on the ending evaluation scores from the open response bias measures administered pre- and post the first CBM-I training session. No significant main effect of time, $F(1, 55) = .22$, $p = .64$, was observed. No significant main effect of group, $F(2, 55) = 1.66$, p = .2, was observed. No significant interaction effect between time and group was observed, $F(3, 55) = .27$, $p = .76$.

A further mixed ANOVA was conducted on the ending evaluation scores from the open response bias measures administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 55) = 4.89$, $p = .03$, was observed. No significant main effect of group, $F(2, 55) = .49$, $p = .62$, was observed. No significant interaction effect between load and group was observed, $F(3, 55) = .08$, $p = .92$.

No effects of significance were thus found in participants' evaluation data.

Discussion

In terms of bias, the CBM-I training appeared to generally perform similarly regardless of the pen task implemented. Across time however this was composed of a variety of primarily un-hypothesised effects. No significant change over time was found for scrambled sentence resolution or evaluation of material. After the first training program, approximation of likelihoods of negative outcomes occurring rose significantly in the groups receiving CBM-I only and CBM-I with a negative pen task, and not in the group receiving CBM-I with a positive pen task. Taken alone this suggests a limited capacity of the pen task to interfere with performance on this variable.

This said, the pen task does not appear to have been an ideally consistent mood manipulation. Whilst it was maintained for reasons of design consistency (and the fact that full analysis had not been undertaken before the additional condition was implemented) in the remaining experimental comparison in this chapter (detailed below), alternative candidates for future mood manipulations are considered for their feasibility in the general discussion at the end of this chapter, alongside a more detailed explanation of the current experiment's results in the light of those from the prior and subsequent studies.

For generation meanwhile, all three groups generated significantly greater numbers of negative resolutions to ambiguous scenarios after the first training program, and did not change significantly after the second training program. This is a phenomenon likely to be attributable to a temporal cognitive/affective disengagement effect and, as not unique to the current experiment, is considered in the more comprehensive general discussion at the end of this chapter.

Following the unexpected null result of experiment two, a further priority of this thesis was to give appropriate additional experimental coverage to the feasibility of open response training (to provide a more conclusive ruling on the feasibility of such tasks). It was thus decided to run a further experiment to this end. The remaining experiment, detailed in the following section, applies open and closed-response CBM-I alongside each other to compare their effectiveness (in a conceptual replication of experiment two) and resistance to the mood intervention (in an extension of the current experiment).

Experiment Five: Comparative Performance of Implicit and Open Ended CBM-I under Mood Manipulation

Design Rationale and Hypotheses

In this remaining part of the experimental series, focus returned to the open ended training trialled in the second experiment in this thesis. As mentioned in the prior section it was felt that a partial (conceptual) replication was necessary to confirm the feasibility (or lack thereof) of implementing this training to exert change on interpretive bias. The open response training from experiment two was thus modified into two shorter negative and positive programs and applied in the design format of experiment four, and compared with the existing equivalent closed-resolution condition from experiment four, with both applied alongside pen tasks of opposing, and then complementary valence to their own (negative training and positive pen task followed by negative training and negative pen task. The groups and tasks completed are detailed in figure ten in the design section below for clarity.

Accordingly an additional condition was put together for a further group of participants, with a new open-ended program (described in the materials section below) combined with a positive pen task. This was compared with the CBM-I with positive pen task condition applied in the previous experiments, with the following further hypotheses incorporated for testing. Of note is that although it was suspected that the CBM-I training in general would be resistant to the mood manipulation provided by the pen task, it was nonetheless considered necessary to compare both closed-resolution and self-generated CBM-I under pen task conditions to ensure this. Looking at any differences here would further clarify the operating efficacy of these two procedures, and indicate their potential robustness in the context of potential accentuation and interference from the pen task as described for the previous experiment.

Interpretive bias. For interpretation after the first training program, it was predicted that both groups would exhibit a significant increase in negativity of interpretive bias, as measured by the scrambled sentence task and the three elements of the open response bias measure. Furthermore it was predicted that the group receiving open-ended training would exhibit significantly greater modification of bias, and thus would show a significantly greater post-training increase in negativity than participants in the closed-resolution CBM-I group.

Mood. It was predicted that both groups would exhibit significant decrease in negative mood and increase in positive mood from pre-to-post training at both time points.

Method

Design

An experimental design was used, applying twin two by two designs to analyse the two consecutive training programs applied separately.

For each program, time of testing formed the within-groups independent variable, with two levels of measurement: pre-training, and post-training.

Experimental condition formed a further independent variable, with two conditions: standard word completion CBM-I with "positive" pen task and open-ended CBM-I with positive pen task.

Dependent variables applied were identical to those issued in the first experiment.

Participants

Twenty-two further participants (fifteen females and seven males) were recruited using the criteria established in the prior experiments and compared to the existing CBM-I only group used previously. These participants were compared to the pre-existing group of twenty participants (thirteen females and seven males) who received closed-response CBM-I with positive mood task in the prior experiment. Together participants had a mean age of nineteen years and five months.

Materials

Measurement materials were again identical to those used in the prior experiments in this series. The only difference was in the modification materials that one new experimental group received a self-generation version of CBM-I training. In this open response version of the task participants received the same scenarios but were asked, via standardised instructions at the start of the procedure, to generate the entire word to end each scenario themselves.

For the first training program participants were instructed to only provide words which would bring a negative resolution to each scenario. In the second program they were asked to only provide words which would provide a positive resolution to each scenario. The final paragraph of the instructions displayed to them in these programs thus became:

Your job is to use the description you have just read to help you complete the word ***[negatively/positively]****, so that it finishes the last sentence in a* ***[negative/positive]*** *way. When you know what the incomplete word is, press the advance key. Then enter the letters of the missing word. Finally, you will be asked a question about the description to check that you understood it.*

Procedure

A procedural diagram updated with the conditions compared in this experiment is included in figure ten.

Figure 10: the experimental procedure

Experimental group	CBM-I with positive pen task	Self-generation CBM-I with positive pen task
	Consent form and standardised instructions	
Affect measurement	Fear of Negative Evaluation scale and PANAS	
Bias measurement (Randomised order)	Scrambled sentence task Open Response bias task Dot-probe task	
Training part one	Negative resolution CBM-I with positive pen task	Negative resolution self-generation CBM-I with positive pen task
Affect measurement	PANAS	
Bias measurement (Randomised order)	Scrambled sentence task Open Response bias task Dot-probe task	
Affect measurement	PANAS	
Training part two	Positive resolution CBM-I with positive pen task	Positive resolution self-generation CBM-I with positive pen task
Affect measurement	PANAS	
Bias measurement (Randomised order)	Scrambled sentence task Open Response bias task Dot-probe task	
	Debrief	

Results

Data Screening, Treatment and Exclusions

The same exclusion criteria were applied to the additional condition in this experiment as those used in prior experiment. At the end of this process, nineteen further participants contributed data, which was added to that for the twenty existing participants in the standard CBM-I condition.

Levene's tests indicated that there was no significant deviation from homogeneity of variance for any of the variables used. Participants did not differ significantly in their initial levels of positive affect, negative affect, scrambled sentence score under conditions of cognitive load, and their initial interpretive bias response generation, response selection and response evaluation. Table twenty one displays the relevant means, standard deviations and t-test outcomes for these characteristics. Standard ANOVA usage (a small minority of data for the evaluation and generation variables did not meet normality expectations) and exclusions were justified on the same grounds as the prior experiments, resulting in twenty individuals in the closed resolution CBM-I group with positive pen task and twenty one in the explicitly-instructed open resolution CBM-I group with positive pen task.

Table 21: Pre-training means of state affect, scrambled sentence test score (load), and response generation, selection and evaluation bias scores.

Variable	CBM-I with positive pen task	Open Response CBM-I with positive pen task	*t*
State anxiety	2.9 (1.25)	3 (1.02)	.15
State positive affect	27 (6.08)	28.24 (6.88)	.61
State negative affect	12.3 (4.08)	13.38 (3.4)	.92
Scrambled sentence test score (load)	29.99 (18.36)	27.6 (16.9)	.42
Response generation bias score	.24 (.1)	.18 (.08)	.73
Likelihood approximation bias score	3.36 (.97)	3.64 (1.1)	.78
Response evaluation bias score	1.8 (55)	2.18 (.95)	2.14

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parenthesis.

Higher scores on the SST indicate higher proportions of negative resolutions made.

Emotion

Positive affect. A mixed ANOVA was conducted on the positive affect data from the PANAS scales administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 40) = 31.1$, $p < .001$, was observed and t-tests (table twenty two) suggested that this was composed of significant decrease in both groups that was maintained when a Bonferroni correction was applied. No significant main effect of group, $F(1, 40) = .06$, p = .81 was observed. No significant interaction effect between time and group was observed, $F(2, 40) = .6$, $p = .44$.

Table 22: Comparisons of positive affect scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
Closed Response CBM-I with positive pen task	27 (6.08)	21.85 (7.33)	3.08**	19	.85
Open Response CBM-I with positive pen task	28.24 (6.88)	21.43 (5.6)	5.01***	20	.99

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the on the positive affect data from the PANAS scales administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 40) = 3.04$, $p = .09$, was observed. No significant main effect of group, $F(1, 40) = .36$, p = .55, was observed. No significant interaction effect between time and group was observed, $F(1, 40) = .04$, $p = .85$.

In summary, a significant main effect of time was upheld by both groups individually, each of which decreased in positive affect across the first training program but neither of which changed significantly across the second program.

Negative affect. A mixed ANOVA was conducted on the negative affect data from the PANAS scales administered pre- and post the first CBM-I training session. No significant main effect of time, $F(1, 40) = 3.3$, $p = .08$, was observed. No significant main effect of group, $F(1, 40) = 1.12$, p = .3, was observed. No significant interaction effect between time and group was observed, $F(2, 40) = .24$, $p = .63$.

A further mixed ANOVA was conducted on the negative affect data from the PANAS scales administered pre- and post the second CBM-I training session. A significant main effect of time, *F*(1,

40) = 4.5, *p* = .04, was observed. Corrected t-tests (table twenty three) suggested that neither group changed significantly alone. No significant main effect of group, *F*(1, 40) = 1.81, p = .19, was observed. No significant interaction effect between time and group was observed, *F*(2, 40) = .07, *p* = .8.

Table 23: Comparisons of negative affect scores pre- and post-second training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
Closed Response CBM-I with positive pen task	12.3 (4.08)	12.85 (5.0)	1.74	19	.2
Open Response CBM-I with positive pen task	13.33 (3.15)	12.27 (2.09)	1.35	20	.34

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

A tendency to become more negative at the level of a main effect was observed after the second experiment only but not significant at the level of individual groups.

Anxiety. A mixed ANOVA was conducted on the anxiety only data from the PANAS scales administered pre- and post the first CBM-I training session. No significant main effect of time, *F*(1, 40) = 1.9, *p* = .18, was observed. No significant main effect of group, *F*(1, 40) = .001, p = .97, was observed. No significant interaction effect between time and group was observed, *F*(2, 40) = .06, *p* = .81.

A further mixed ANOVA was conducted on the anxiety only data from the PANAS scales administered pre- and post the second CBM-I training session. No significant main effect of time, *F*(1, 40) = 1.36, *p* = .25, was observed. No significant main effect of group, *F*(1, 40) = .77, p = .39, was observed. No significant interaction effect between time and group was observed, *F*(2, 40) = .23, *p* = .64.

As such, no significant effects were observed for anxiety at a within or between groups level or that of an interaction.

Interpretation

Scrambled sentence task data. A mixed ANOVA was conducted on the interpretive bias indices from the scrambled sentence tests administered pre- and post the first CBM-I training session. No significant main effect of time, *F*(1, 40) = 3.02, *p* = .09, was observed. No significant main effect of group, $F(1, 40) = .047$, p = .83, was observed. No significant interaction effect between time and group was observed, $F(2, 40) = .004$, $p = .95$.

A further mixed ANOVA was conducted on the interpretive bias indices from the scrambled sentence tests administered pre- and post the second CBM-I training session. No significant main effect of time, $F(1, 40) = 5.18$, $p = .03$, was observed. No significant main effect of group, $F(1, 40) = .1$, p = .75, was observed. No significant interaction effect between time and group was observed, $F(2, 40) = .67$, $p = .42$.

As with anxiety, there were thus no significant effects at a within or between groups level or that of an interaction for scrambled sentence task data.

Generation. A mixed ANOVA was conducted on the generation scores from the open response bias measures administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 40) = 22.56$, $p < .001$, was observed. Corrected t-tests (table twenty four) suggested that there was a significant increase in the amount of negative endings generated for participants in both groups. No significant main effect of group, $F(1, 40) = 1.11$, p = .3, was observed. No significant interaction effect between time and group was observed, $F(2, 40) = 1.7$, $p = .2$.

Table 24: Comparisons of generation bias scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
Closed Response CBM-I with positive pen task	.24 (.1)	.32 (.14)	2.38*****	18	.34
Open Response CBM-I with positive pen task	.18 (.08)	.32 (.14)	4.39***	20	.43

* = p< .05, ** = p < .01 *** = p < .001.

***** Bold asterisks indicate that the test still reaches a significance level of p<.05 with appropriate Bonferroni correction applied.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the generation scores from the open response bias measures administered pre- and post the second CBM-I training session. A significant main effect of time, $F(1, 40) = 4.23$, $p = .05$, was observed. Corrected t-tests (table twenty five) suggested that there were no significant changes in generation in either group alone. No significant main effect of group, $F(1, 40) = .06$, p = .8, was observed. No significant interaction effect between time and group was observed, $F(2, 40) = .01$, $p = .99$.

Table 25: Comparisons *of generation bias scores pre- and post-first training program, split by experimental condition received.*

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
Closed Response CBM-I with positive pen task	.31 (.03)	.26 (.02)	1.45	19	.34
Open Response CBM-I with positive pen task	.32 (.02)	.28 (.02)	1.46	18	.43

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

To summarise results for participants' generation of negative material relating to social judgment then, main effects of time indicating increase and then decrease in this tendency were observed but reached within group significance for both groups only after the first training program.

Likelihood Approximation. A mixed ANOVA was conducted on the likelihood estimate scores from the open response bias measures administered pre- and post the first CBM-I training session. A significant main effect of time, $F(1, 40) = 10.74$, $p = .002$, was observed. T-tests (table twenty six) suggested that the time effect was significant in the open response training group which maintained significance under a Bonferroni correction. No significant main effect of group, $F(1, 40) = 1$, p = .32, was observed. No significant interaction effect between time and group was observed, $F(2, 40) = .88$, $p = .35$.

Table 26: Comparisons of likelihood approximation bias scores pre- and post-first training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
Closed Response CBM-I with positive pen task	3.36 (.97)	3.8 (1.39)	1.46	19	.45
Open Response CBM-I with positive pen task	3.52 (1)	4.31 (1.35)	3.52*****	18	.79

* = p< .05, ** = p < .01 *** = p < .001.

***** Bold asterisks indicate that the test still reaches a significance level of p<.05 with appropriate Bonferroni correction applied.

Standard deviations appear in parentheses.

A further mixed ANOVA was conducted on the likelihood estimate scores from the open response bias measures administered pre- and post the second CBM-I training session. A significant main effect of time, *F*(1, 40) = 5.6, *p* = .02, was observed. T-test comparisons (table twenty seven) suggested that the open response group alone demonstrated a significant difference at the within-groups level, but that this did not maintain significance after Bonferroni correction.

Table 27: Comparisons of likelihood approximation bias scores pre- and post-second training program, split by experimental condition received.

Experimental condition	Pre-test mean	Post-test mean	*t*	df	*d*
Closed Response CBM-I with positive pen task	3.8 (1.39)	3.37 (1.01)	1.38	19	.31
Open Response CBM-I with positive pen task	4.31 (1.35)	3.76 (1.14)	2.15*	18	.36

* = p< .05, ** = p < .01 *** = p < .001.

Standard deviations appear in parentheses.

No significant main effect of group, *F*(1, 40) = 1.4, p = .24, was observed and no significant interaction effect between time and group was observed, *F*(2, 40) = .01, *p* = .91.

In summary, a tendency to assess social judgment-related negative events as less likely to occur after the first training program was significant in the group receiving open response training only and not reflected after the second training session.

Evaluation. A mixed ANOVA was conducted on the ending evaluation scores from the open response bias measures administered pre- and post the first CBM-I training session. No significant main effect of time, *F*(1, 40) = 1.5, *p* = .23, was observed. No significant main effect of group, *F*(1, 40) = 1.16, p = .29, was observed. No significant interaction effect between time and group was observed, *F*(2, 40) = .15, *p* = .71.

A further mixed ANOVA was conducted on the ending evaluation scores from the open response bias measures administered pre- and post the second CBM-I training session. No significant main effect of time, *F*(1, 40) = .33, *p* = .57, was observed. No significant main effect of group, *F*(1, 40) = .12, p = .74, was observed. No significant interaction effect between time and group was observed, *F*(2, 40) = 1.49, *p* = .23.

No significant differences were thus observed in evaluation data.

Experiment Five Discussion

The results did not support the hypotheses that open response training would have a greater effect on interpretive bias than a standard CBM-I training program. Specifically, no significant change was found for scrambled sentence tasks scores or evaluation of responses made to the open response task in response to the training variants administered. Participants in both groups generated more negative resolutions to the scenarios of the open response task after the first training session only. Participants in the closed resolution CBM-I group perceived negative endings to open response task scenarios as significantly more likely to occur following the first training program only.

As such the results are primarily null and generally concordant with those gained in the prior studies. In this way they provoke a number of technical questions about the nature of CBM-I. As considered in the prior chapter concerning open response training, if we take the “deep” effect CBM-I position outlined in the introduction, and assume a learned production rule as in Mathews and Mackintosh’s (2002), we might expect, in the absence of other information, an open-response training program to enhance a CBM-I effect beyond the level of a closed-resolution, word completion program. This is because we would assume that participants would not just learn a rule for completing pre-prescribed, constrained, pseudo-ambiguous material in a positive way, but would gain production rules for the *full generation* of material when confronted with ambiguity. If a learned production rule-based account remains the case, in the light of the current results, then this may be confined to simple resolution of ambiguity and not able to be built upon in any meaningful way by incorporating self-generation. This discussion is furthered in the general discussion below.

General Cross-Experimental Discussion: Other Limitations, Alternative Explanations and Implications

As mentioned in the discussion regarding the first experiment, it may be the case that the training administered needs alteration in order to successfully and comprehensively induce the sought effects. CBM-I effects have been indicated in designs of similar length (e.g. Mathews & Mackintosh, 2000), but some of the more impressive results gained have been attained using longer sessions of training. This could be in the form of extension of the length of training as this experiment used a relatively short number of scenarios per valence type, with seventy scenarios of each valence being completed by participants. Such long training was avoided in the current experiment because there were two programs administered to participants in a single session: anything notably longer would have been unfeasible for a single experimental session, and because significant “proof of concept” results have been gained with even shorter training programs previously, for example in Mathews and Mackintosh’s (1998) foundational work. Alternatively or in addition, multiple training sessions could be implemented as distributed practice has been suggested to bring about enhanced and sustained effects of CBM-I in a number of designs (e.g. Amir et al., 2009; Schmidt et al., 2009).

Considerable individual differences were present in certain variables in this dataset, for example scrambled sentence task performance. As such an element of statistical noise was present which, if it could be fully accounted for, might have some bearing on the results gained. Whilst differences in starting levels of variables like interpretive bias and mood are of some relevance here (and may have created floor effects in certain participants), it is perhaps the differences in changes in these variables that are the most salient factor, as they imply considerably different responses to the training undertaken. This issue is linked in some cases (floor effects may naturally limit further training success for certain participants) but distinct in many others (certain participants showed little change despite having considerable potential change on each measure). An implication of this for future work, especially as it is largely absent in the existing interpretive bias literature, is that studies need to better address moderating variables for CBM-I effects. Such research would have considerable potential to clarify the research base, and would provide a basic set of control variables that all future CBM-I studies could account for in interpreting their results. Any further variables that might modify CBM-I would depend somewhat upon its mechanism of action, which the current study does not provide conclusive evidence regarding due to the specific nature of the results gained.

CBM-I training may even vary not just in the extent but in its mechanism of action with different participants. Some participants may engage deeply with the training, gain production rules and mood effects, and maybe even also experience some valence priming (the two accounts of action mechanism need not be after all be entirely exclusive). These participants might exhibit large gains from the procedure, with dramatically altered biases and mood after even short periods of training. Other participants might not engage as well with the procedure, perhaps for reasons of attentional control, boredom effects, state or trait affect, or other cognitive facilitatory variables. They might gain little to nothing from the training and might show a small to non-existent change in bias. As mentioned, there was considerable inter-individual difference on absolute levels of bias at both time points and on change in bias so this is a highly feasible explanation (one caveat is of course that we expect a reasonable degree of variance in the most controlled and fine-grained designs, and such individual data points should not be interpreted too conclusively in isolation, as elements like order effects would be present in individual data that would be counterbalanced in the full analysis itself). A concordant individual differences-focused approach to biases is further discussed (with particular reference to concordant recent research with cognitive behavioural therapy) in greater detail in chapters six and seven of the current thesis.

In terms of potential further sources of error in the experimental design, there are a few aspects that may have allowed elements of statistical noise into the dataset. In retrospect one minor element that could have been better controlled would have been the open response task – instead of version-based counterbalancing wherein a participant received one of three sets of questions at each timepoint, full item-based counterbalancing that varied each constituent item and the order in which it was presented within the set of items issued could have occurred. It is feasible that participants could have responded more negatively or positively to certain sets of items not just per se (which the implemented counterbalancing should counteract for) but at specific time points (having for example been primed by the CBM-I or mood task they received) and if a third of all participants took one version of such a measure at a key time point this could feasibly add up to a moderate amount of noise variance.

Although a key strength of the current study is that it used multiple measures of bias as outcome indicators, future studies could still attempt to triangulate the results gained in the present one using different combinations of measurement and modification methods. The study faced pragmatic participation constraints and ideally two or three shorter studies each featuring more in-depth measurement could perhaps have achieved its goals with slightly greater efficacy. The average participant took around two hours fifteen minutes to complete participation from start to finish, and this varied considerably. Due to this negative temporal factors like individual differences in cognition and affect (for example boredom) were a concern in the current design, and, whilst direct data is not available, the affect data is concordant with this in the current design. Such potential boredom effects may have flattened potential effects overall and/or varied on an inter-individual level like the variables considered earlier in this discussion, to produce noise variance. Indeed the self-report of several participants after the experiment indicated considerable variation in how engaging they thought the design was: many were enthusiastic at the end of the experiment, but a similar number were unengaged by the design and as such a shorter design with a more stable impression on participants may have been better for the reduction of the potential extraneous variance exerted. This would not have been achievable without either dividing up the experiment into smaller ones or sacrificing measures or other content however.

Power should receive brief mention. CBM-I effect sizes appear to vary considerably (Cristea, Kok & Cuijpers, 2015) and whilst sample sizes are consistent with contemporary CBM-I work and calculations for this study were made using data from the closest possible existing designs (e.g. Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt, 2013), the fact remains that this experiment is distinct in design and nature from other CBM-I works. As such a slightly higher sample size may have helped, although given the strong effects observed in parts of the experiment and almost complete absence in others this may not have made much difference.

In the absence of comprehensive and conclusive evidence for the desired effects from the pen task it may be the case that an alternative method of valence priming is required, in order to fully test CBM-I's resistance to such interference. Candidate procedures would need to be minimally invasive and compatible (procedurally that is) with the participant simultaneously completing the CBM-I task - the difficulty in implementing an empirically supported task of this nature was part of the reason that such a simple and basic pen task was chosen originally. Conceivably participants could perhaps instead receive electronic muscle stimulation or similar with minimal procedural disturbance to the CBM-I design, so such a technique could be an element of future work.

The mood measurement appears to have been appropriately sensitive to change in general: overall there appeared to be general strong changes over time in this experiment, particularly increases in negativity of affect. Similarly for the open response measure distinct changes (albeit not in response to the experimental manipulations as envisaged) were present in generation and likelihood approximation of responses. These may have helped to override between-groups effects and may have several explanations. Comparatively little research on producing negative interpretations (as opposed to positive ones) has taken place experimentally. It may simply be the case that introducing negativity into designs such as this one is potent and that having any kind of negative stimulus, be this a CBM-I task or a pen task, is enough to create negativity in interpretations which is then generally not significantly overridden by a positive task, either concurrently or subsequently. As such the large general time effect between times one and two of measurement may in fact be composed of at least two distinct effects – negative training and negative pen task, that were particularly potent. Conceptually this argument for the particular salience and effectiveness of negative tasks is hard to justify however, particularly for a nonclinical sample with varying levels of social anxiety. It seems more likely that a simple temporal cognitive-affective effect like boredom and/or disengagement was occurring here.

In summary, the set of primarily un-hypothesised results from the experiments undertaken and detailed in this chapter provided inconclusive data on the relationship between CBM-I and mood in particular, and potential method-rooted explanations were explored. As such a method, less reliant on active manipulation was sought and implemented in the next chapter, which integrates, combines and analyses initial data from the prior experiments to consider this relationship in further detail.

Chapter Six: State and Trait Affect and Interpretive Bias: Cross Experimental Analysis

Introduction

Previous chapters in this thesis displayed mixed and primarily null results for single-session cognitive bias modification for interpretation (CBM-I) programs. In response to this and in accordance with the dual modification and measurement focus of this thesis, in the present chapter further modification was not attempted, in favour of detailed scrutiny of the measurement of interpretive bias. At the level of the primary research conducted in the current thesis, this was considered important for clarifying the (often null) findings gained in the current series of studies (moreover improving bias measurement is an under-represented facet in the contemporary literature, which generally often uses poorly-substantiated methods of assessing intervention outcomes in general (Cristea, Kok & Cuijpers, 2015). In particular such investigation helps to clarify further whether such findings were attributable to shortcomings in the measurement or modification implemented, and whether these are limited to method-based and unforeseen flaws in the series of studies implemented or are reflective of wider issues with measurement in the implementation of interpretive bias-focused studies.

Concerning the latter, it is of note that little such clarification exists in the literature as it currently stands. As discussed in the introductory chapters of this thesis, measures of interpretive bias are inhomogenous (Cristea, Kok and Cuijpers, 2015) and are not often subject to scrutiny with respect to key constructs including validation (Rohrbacher & Reinecke, 2014). Methods of modification are similarly unstandardised, diversely implemented, and in many places unsubstantiated with direct data. As such, the research contained in the current chapter is considered a further unique contribution to knowledge in the relevant fields, in addition to underpinning and enhancing the implications of the prior experimental chapters.

Accordingly with the above it was considered key to look at the affective predictors of interpretive bias, as measured by both the scrambled sentence test and the open response measure. A fundamental assumption of interpretation-focused cognitive bias work is that interpretive bias has a close association with trait affect, but is relatively independent of state affect. This “trait –associated” perspective has a number of associated postulations. Key amongst these include the assumption (with accompanying tentative evidence) that the bias develops gradually in a developmental context (e.g. Field & Lester, 2010) and the further assumption it only alters in response to significant aversive events (an assumption that has a paucity of direct evidence currently, perhaps due to difficulty with study design), or targeted therapeutic interventions such as CCBT or CBM-I (e.g. Bowler, Mackintosh, Dunn, Mathews, Dalgleish & Hoppitt, 2012).

The counterpart to the above is not expecting interpretive bias to vary significantly with short-term state affect. Were this the case, then interventions targeting mood would serve the majority of the possible scope of CBM-I programs, and interpretive bias would have a less significant role in the long-term affective processing of individuals. There is a small amount of direct evidence which suggests that is not the case. Certain bias-focused interventions have been found to operate independently of explicit changes in mood (Mathews and Mackintosh, 2002), and mood manipulations appear to exhibit distinct effects to CBM-I programs when directly compared (Standage, Ashwin & Fox, 2010). Critically however, mood changes were observed with CBM-I in the latter experiment and not in the former, suggesting further investigation is required (and hence the uptake of the investigation of a mood and bias relationship both in the current chapter and chapter five of this thesis). Whilst state affect-independence from interpretation was also investigated in part by a distinct experimental chapter, it was considered further necessary to triangulate this overall with the large sample gained by combining participants across the initial two experiments (and to do so using a different analytical approach). Furthermore, whilst much manipulation work exists, few large-scale investigations have been conducted concerning the interrelationship of trait and state affect and bias. As such providing direct analysis of these, via the data from the constituent studies of this thesis, was considered an important standalone contribution.

In investigating the relationship between affect and interpretive bias, a further element of interest was any difference in this relationship for the different measures of interpretive bias implemented. Throughout the experimental components of this thesis, the measures employed (scrambled sentence task and open response task) were intentionally implemented as near-diametrically opposed measures of interpretive bias. This was partially to offer the comprehensive measurement of bias as an outcome measure that it was felt was lacking in many contemporary designs, but was also partially to consider the distinctions between these two aspects, or conceptualisations, of interpretive bias.

The scrambled sentence task has the properties of being time-limited and fast (and thus potentially but not definitively a more “on-line” measure), closed resolution (with participants only able to complete each item in one of two ways), and being a “gross” measure of bias (providing a single numerical index). The open response task thus opposes it on these aspects – being slower to complete (with participants providing (potentially) more considered answers), open resolution (with participants generating their own resolutions in response to open-ended stimulus material), and subdividing bias in overall interpretation into its potential facets of generation, likelihood approximation and evaluation, with distinct indices for each of these. As such the two tasks allow a greater capturing of bias across interpretation than in many prior designs when applied together. However in doing so they also provide contrasting measurement of very distinct elements of interpretation, which may have their own distinct operation and associations, and even interface with automatic and controlled processing. The comparison of these was a further aim of this chapter in particular, in order to provide a more detailed modelling of the interpretive processing of socially anxious individuals.

Design Rationale and Hypotheses

The potential presence of a trait and state affect relationship with interpretive bias was investigated further in this chapter by looking at the relationship between initial levels of trait state anxiety and interpretive bias variables in the population studied. This took place via the combination of data from the samples from the initial two experiments (which featured homogenous measurement and when combined allowed a sample size of over one hundred and forty participants for the further analysis).

A fundamental assumption of interpretive bias measurement and modification work is that a participants' level of interpretive bias is relatively stable over time and associated (albeit potentially with complex and cyclical causality) to their longer term anxiety reactivity. Put simply, were these not the case interpretive bias measurement would be of considerably limited use and modification would be near futile. Such an assumption is central to the underlying theory from which CBM-I emerged (for example Beck, Emery & Greenberg, 1985; Rapee & Heimberg, 1997). It has received some evidence from small comparisons of high and low anxiety individuals for social anxiety (e.g. Butler & Mathews, 1983; Mathews, Richards and Eysenck, 1989; Beard & Amir, 2009), but has yet to be confirmed with larger sample, correlational work. As such the current experiment provided direct testing of this assumption. In accordance with the theoretical assumption and initial empirical evidence, it was hypothesised that trait affect would significantly predict interpretive bias on all of the measures implemented.

It was further predicted that state affect (state anxiety in experiment one and two combined dataset and overall positive/negative affect in the experiment three dataset) would not predict or act as a significant moderator of this relationship. Both this hypothesis and its corresponding null hypotheses have evidence that could be interpreted as supporting them in the current literature, and this hypothesis was designed to tease apart conflicting accounts of what occurs in interpretation tasks. In support of the hypothesis is the further assumption from CBM-I literature, that cognitive biases are not only trait associated but are relatively state independent. They have been suggested to be distinct from mood priming and the product of more profound and less state-responsive processes in prior work, with limited direct investigation supporting this (Standage, Ashwin & Fox, 2009). The rival account (see Mathews & Mackintosh, 2002) meanwhile suggests that interpretive biases, and change in them, are primarily artefacts of short-term mood states. Although there is no evidence from direct empirical investigation that supports this position (hence its inverse being the prediction for the current study) it is of note that a considerable body of literature considers the interplay between primed, short-term mood states and interpretation in general. This includes Strack, Martin and Stepper's (1988) studies suggesting interplay between simple affective tasks and elements of interpretation (with participants rating cartoons as more humorous when making a facilitating facial expression) and work by Havas, Glenberg and Rinck (2007) who found a facilitating effect for speed of interpretation of emotional material when participants made a facial expression of matching valence. The interpretive bias change resulting from CBM-I then, if a consistent, stable and useful construct for clinical intervention, should be minimally influenced by such factors and display an association with fundamental trait reactivity and a distinction from state. In the current design the hypothesis that each bias index would display a significant positive predictive relationship with level of trait anxiety and no relationship with state anxiety was assessed. More broadly however, there was considerable interest in the comparative associations of each of the different bias measures and the conclusions which could be drawn from these regarding the nature of interpretation in such tasks.

Method

Design

A series of regressions, one per each interpretive bias variable for the measures (scrambled sentence performance with and without cognitive load and generation, likelihood approximation and evaluation of social judgment-related negative responses on the open response task), were performed on the data from the experiments contained in chapters three and four of this thesis. The interpretive bias variables were thus the target variables to be predicted in each of these. Trait Fear of Negative Evaluation score (FNE: Watson & Friend, 1969) and Spielberger STAI state anxiety score (STAI: Spielberger, Gorsuch & Lushene, 1970) were employed as predictors, alongside the further predictor of the interaction between these two variables in order to account for the moderating influence of state anxiety on trait anxiety's ability to predict interpretation.

Participants

One hundred and forty-two participants contributed data to this analysis. Participants featured a Fear of Negative Evaluation Scale (FNE: Watson & Friend, 1969) score of fifteen or over, indicating moderate to severe (nonclinical) social anxiety. All participants were over 18 and were not undergoing treatment for a mental health condition.

Materials

Participants received a copy of the FNE in order to measure this key component of their trait anxiety reactivity. Full psychometric properties of this instrument are detailed in the initial experimental chapter of this thesis (chapter three, page sixty nine).

Participants completed a copy of the state portion of the Spielberger STAI. Full psychometric properties of this instrument are again detailed in the initial experimental chapter of this thesis (chapter three, page sixty nine), but it is a widely used and appropriately validated measure of participant state anxiety.

For the first aspect of bias measurement, participants completed two scrambled sentence tasks (SSTs). Each of these was one of four variant lists of twenty six-word sets. Again full detail on the content of these tasks is included in chapter three, the first experimental chapter using them (page seventy).

To provide further measurement of interpretive bias, participants completed one of two open response measures, adapted from those applied in Dodd, Stuifzand, Morris and Hudson's (2015) recent work. Again full detail on this measure is included in chapter three (pages seventy one to seventy two). The task results in bias indices relevant to participants' generation of negative content, their approximation of how likely it is to occur, and their evaluation of how negative they would feel if it did occur. As such generation produces a score between zero and one indicating the proportion of SJN content generated, zero being none and one being entirely social judgment related negative content. Likelihood produces a score of between one and seven, representing the rating for the average SJN scenario generated by each participant, one being extremely unlikely and seven being extremely likely. Evaluation also produced a score of between one and seven, with one representing extremely unpleasant and seven representing extremely pleasant.

Procedure

Participants in the experiment contained in chapter three, from which this data set was partially gained, were sent an information sheet and consent form and then completed the FNE and

Open Response task prior to attending an experimental session. They then completed the SSTs and the STAI at the beginning of their experimental session.

Participants in the second experiment (which contributed the remainder of the data and is detailed in chapter four) read the information sheet and completed the consent form and all measures at the beginning of their experimental session. This always began with the FNE task (this was used to check their eligibility for participation in the experimental session that followed, as detailed in the second experimental chapter of this thesis) but was followed by the other three tasks in a randomised order.

Participants completed an experimental session after providing the above data, and received comprehensive debrief and payment afterwards as detailed in the respective chapters for the two experiments (chapters three and four of this thesis).

Results

Normal Q-Q plots suggested that the data generally met the assumption of normality of residuals, with some minor divergence from this for the generation and evaluation variables. Due to the relative robustness of linear regression however, and its amenability to moderation analysis it was decided to conduct initial analysis using this model and incorporate custom models at a later stage as necessary. Confidence intervals were bootstrapped, in order to reduce the influence of non-normality.

Interpretive bias: Scrambled Sentence Scores

Table twenty eight displays the affect predictors of interpretive bias as measured by the scrambled sentence task without conditions of cognitive load. Both Spielberger state score and Fear of Negative Evaluation score were significant predictors of interpretive bias. Furthermore, there was a significant moderation relationship.

Table 28: predictors and moderators of SST scores without cognitive load.

Variable	B (unstandardised)	SE B	*T*
Constant	31.84 (28.97, 34.72)	1.45	21.91***
FNE score (centred)	1.27 (.87, 1.68)	.20	6.2***
Spielberger state score (centred)	.66 (.33, .98)	.17	3.96***
FNE score x Spielberger State score	.05 (.006, .1)	.02	2.21*

Model	R	R squared	SE	F
Overall Model Summary	.55	.31	294.47	18.35***

* = p< .05, ** = p < .01 *** = p < .001.

Confidence intervals are included in brackets next to Beta outcomes.

Further analysis using the Johnson-Neyman technique showed that a significant (p<.05) positive relationship between FNE score and Scrambled Sentence Task score began at a Spielberger State value of 26.41. Prior to this point no significant relationship existed (9.15% of the overall data) and significance increased steeply in the remaining 90.85% of data with increasing levels of the moderator.

Table twenty nine displays the affect predictors of interpretive bias as measured by the scrambled sentence task under conditions of cognitive load. Both Spielberger state score and Fear of Negative Evaluation score were significant predictors of interpretive bias under conditions of cognitive load. No significant moderation relationship was observed.

Table 29: predictors and moderators of SST scores under conditions of cognitive load.

Variable	B (unstandardised)	SE B	*t*	
Constant	30.92 (28.48, 33.36)	1.23	25.1***	
Spielberger state score (centred)	.41 (.15, .66)	.13	3.11**	
FNE score (centred)	.79 (.32, 1.17)	.19	4.21***	
FNE score x Spielberger State score	.005 (-.03, .04)	.019	.25	
Model	R	R squared	SE	F
Overall Model Summary	.45	.2	199.39	12.87***

* = p< .05, ** = p < .01 *** = p < .001.

Confidence intervals are included in brackets next to Beta outcomes.

Interpretive Bias: Response Generation

Table thirty displays the affective predictors of interpretive bias as measured by participants' generation of SJN responses to the open response task. None of the proposed predictors had a significant association with interpretive bias in response generation.

Table 30: predictors and moderators of interpretive bias in participant response generation.

Variable	B (unstandardised)	SE B	*T*
Constant	.22 (.19, .25)	.015	15.33***
FNE score (centred)	-.0001 (-.005, .004)	.002	.057
Spielberger state score (centred)	.0001 (-.003, .003)	.001	.063
FNE score x Spielberger State score	-.0001 (-.0005, .0004)	.0002	.22

Model	R	R squared	SE	F
Overall Model Summary	.02	.001	.02	.02

* = p< .05, ** = p < .01 *** = p < .001.

Confidence intervals are included in brackets next to Beta outcomes.

Interpretive Bias: Response Likelihood Approximation

Table thirty one displays the affect predictors of interpretive bias as measured by participants' likelihood approximation of SJN responses to the open response task. Trait Fear of Negative Evaluation was the single significant predictor of interpretive bias in response likelihood approximation.

Table 31: predictors and moderators of interpretive bias in participant response likelihood approximation.

Variable	B (unstandardised)	SE B	*T*
Constant	3.39 (3.22, 3.55)	.0822	41.18***
FNE score (centred)	.046 (.02, .07)	.01	4.06***
Spielberger state score (centred)	.005 (-.01, .03)	.01	.54
FNE score x Spielberger State score	.001 (-.001, .004)	.001	

Model	R	R squared	SE	F
Overall Model Summary	.33	.11	.83	5.5***

* = p< .05, ** = p < .01 *** = p < .001.

Confidence intervals are included in brackets next to Beta outcomes.

Interpretive Bias: Response Evaluation

Table thirty two displays the affective predictors of interpretive bias as measured by participants' evaluation of SJN responses to the open response task. None of the proposed predictors had a significant association with interpretive bias in response evaluation.

Table 32: predictors and moderators of interpretive bias in participant response evaluation.

Variable	B (unstandardised)	SE B	*T*	
Constant	2.02 (1.86, 2.19)	.08	24.07***	
FNE score (centred)	-.0006 (-.02, .02)	.01	-.06	
Spielberger state score (centred)	0.002 (-.02, .02)	.009	-.21	
FNE score x Spielberger State score	-.0002 (-.002, .002)	.0011	-.19	
Model	R	R squared	SE	F
Overall Model Summary	.02	.001	.84	.02

* = p< .05, ** = p < .01 *** = p < .001.

Confidence intervals are included in brackets next to Beta outcomes.

Discussion

Scrambled Sentence Task Performance

The hypotheses that trait anxiety (FNE) would predict interpretive bias as measured by the scrambled sentence task was supported by the analysis. What was not hypothesised were the findings that state anxiety was also a significant predictor of this bias and that state anxiety significantly moderated the influence of trait anxiety on interpretive bias in the task undertaken with conditions of cognitive load.

The trait association finding supports the eligibility of the scrambled sentence task for the measurement of a social anxiety-associated bias. Despite the potential shortcomings explored in the introductory section of this chapter, there does appear to be a tendency towards negativity in this aspect of interpretation in socially anxious populations. This does not guarantee ecological validity however, and how meaningful such a tendency is (in particular whether it actually extends to a diverse array of social situations) remains without direct evidence.

In terms of implications for the other empirical work in this thesis, the trait finding in particular provides evidence for one particular explanation for the mixed and primarily null results gained. In reinforcing the basic legitimacy of the SST as a bias measure as considered above, it suggests that the CBM-I programs administered simply did not reliably work to induce change in interpretive bias.

The state association meanwhile suggests that variation in bias over time may be at least partially related to fluctuations in affect, in this case anxiety. Caution should of course be taken in attributing causality in this association, as interpretation is likely to exist in a complex and potentially cyclical relationship with anxiety. This said, the mere lack of independence shown can be interpreted as tentative evidence for a role for state affect as a potential determinant of interpretive bias (although caution should be applied with simple causality here). Studies suggesting the independence of cognitive bias and state affect such as Standage, Ashwin and Fox's (2009) work, do exist however. It is notable that such studies focused on general positive and negative affect and did not isolate anxiety specifically however. To confirm the relative role of affective priming (including both positive and negative affect in general and anxiety specifically) in determining interpretive bias, static levels of initial bias and affect are insufficient and consideration of their change across the time course of experiments is necessary. This formed part of the rationale for the further exploration of general positive and negative affect and mood undertaken in the experiments reported in the third experimental chapter.

The moderation relationship observed was also of note, particularly the distinction between load and non-load conditions. Whilst in the load condition fear of negative evaluation and Spielberger state anxiety scores simply had independent and cumulative relationships with scrambled sentence score, in the non-load condition a significant moderation relationship was also found. A higher Spielberger state score appeared to amplify the effect of trait FNE anxiety reactivity. In itself this is theoretically and logically consistent: trait anxiety reactivity provides a baseline level of bias and a general cognitive framework for reacting in an anxiety-inducing situation and the associated interpretive bias is brought to bear increasingly with activation from state affect, which asserts its own additional independent effect. What is notable is the distinction between the load and non-load conditions. The implication is that cognitive load facilitates the moderation of the effect of general trait anxiety reactivity, via state anxiety level. The general application of cognitive load in such tests is to limit the role of executive processes by placing a constraint upon phonological working memory and thus produce a more “implicit” result. It is thus necessary to assume that a moderation relationship has a strong “top down” element: dual processing accounts, such as that outlined by Evans (2003) posit that automatic processes rooted in prior beliefs and automaticity (system one) and controlled processes driven by more detailed, deductive and controlled logic, supported by working memory (system two) are shown to compete for control of responses. Removal of a portion of system two resources (via the occupation of working memory) may thus result in a relative increase in the superiority of system one-based accounts. Regarding the data in question in the current study, a feasible account for the lack of interaction between state and trait under cognitive load and its presence without conditions of load may emerge. The reliance on more automatic, system one influence may not affect the roles of state and trait affect much per se, but may limit conscious, executive consideration or “weighing up” of the roles of prior experience and current (affective) experience in determining the response given. It is also worthy of brief emphasis at this point that the results, when viewed from this perspective, suggest that cognitive load has a key role in making the scrambled sentence task as automatic as possible, implying that participants generally do not consider the non-loaded version at a purely automatic level but instead give it a level of executive consideration as an appropriate answer.

Of further note (and concordant with the general account) is the fact that the predictive relationships between both trait and state affect (whilst highly significant in both load and non-load conditions) were marginally stronger under conditions of cognitive load, which may represent these independent, automatic tendencies being drawn upon more in the absence of controlled, system two-based interactive consideration.

Whilst the associations found are highly significant, caution should be applied in the attribution of causality: interpretive bias may have a cyclical and complex relationship with trait and state affect (the data gained has a large portion of unexplained variance which would allow for such a model). The associations gained thus function primarily to link these variables together at a broad level and do not imply simple causation or the overall directionality of the effect found. Indeed contemporary models such as that of Rapee and Heimberg (1997) and the example given from Bower’s (1981) model in the introductory chapter of this thesis emphasise complex causality in such relationships.

In addition it should be acknowledged that there is considerable divergence between predictors and interpretive bias despite significant associations, particularly with the ability of trait fear of negative evaluation alone to predict interpretive bias (the original hypothesis and an assumption of much of the research base). Singular coefficients for this variable of .38 (load) and .45 (non-load) indicate small to medium-sized effects. These results thus indicate that certain participants for example, despite a high trait FNE score, did not feature a correspondingly high level of initial interpretive bias. As such, interpretive bias is affirmed as not only a part of a much greater array of associated factors with an individual's social anxiety level, but as varying somewhat in its association inter-individually.

One of the implications of this for the modification-focused portions of this thesis is further general support for a particular explanation of results from the prior chapter: that certain participants thus may have had an inherent ceiling effect placed upon the amount of change that was possible for them. Indeed there were participants for whom fear of negative evaluation and scrambled sentence scores were highly disparate. The initial experiments may have displayed attenuated effects for the interventions implemented, and in modification work in general it may be more advisable to pay close attention to such baseline levels before implemented in any intervention. Even in the current series of experiments this distinction was observed reasonably early on, and helped to inform the design of the subsequent series of experiments. Here, negative bias manipulation was instead implemented first and a two-part design of negative-then-positive manipulations implemented in order to minimise the effect of such ceiling effects.

Open Response Task Performance: Bias in Generation of Responses

The hypotheses that trait and state anxiety would predict interpretive bias in participants' generation of social judgment-related negative responses to the open response task were not supported by the analysis. The associated hypothesis that state anxiety would moderate the influence of trait anxiety on such an element of interpretive bias was also unsupported.

The absence of any affective association for participants' capacity to generate SJN responses suggests that such content is not, overall, more salient or accessible to high FNE participants. In Dodd, Stuifzand, Morris and Hudson's (2015) implementation of an open response bias measure, in addition to overall generation of responses participants' first responses were measured. This was in order to consider, further to reviewing participants' general generation of content, whether such a bias existed in the most initially salient explanation only. This was not feasible in the current series of investigations due to the specificity of the thesis: participants did not consistently produce enough SJN responses as a first answer to make such measurement possible. Such a compromise was necessary - the open response scenarios were designed and piloted to, overall, elicit a range of social judgment-relevant and non-social judgment responses from participants in order to allow overall generation to be measured - but does mean that this further element of generation was not directly accounted for.

The above said, the scrambled sentence task arguably provides some indication of participants' initial tendency to interpret an initial negative outcome, and its strong associations with state and trait affect suggest that a bias in initial response is indeed present and associated with fear of negative evaluation. Overall generation meanwhile may not be a useful target, for example, for cognitive bias modification procedures for the socially anxious, with first response being the key way in which their processing generally differs from less socially anxious individuals. Overall generation might be a less fast, on-line capacity (indeed the open response task may have elicited this kind of thinking from participants, and the general discussion section below considers this further at the general level).

Of note is that such overall generation does not appear to be a static, invariant capacity: it did exhibit a tendency to change over the course of experimental intervention in several of the experimental and control conditions implemented, generally increasing in negativity. Why this is the case remains unknown, as analysis conducted on the data from the latter experiments suggests that this change is not significantly predicted by overall state positive and negative affect. This is the case both when analysed in terms of the relationship between initial baseline levels of each bias variable and PANAS-measured affect, and correlation in change over time of the same bias variables and affect scores. The capacity to generate negative social-judgment content appears to be independent and thus perhaps reliant on an unmeasured cognitive variable, or an experimental procedural element. Candidates for either of these are hard to suggest, particularly as change in generation varied considerably between experimental groups, and sometimes between the same group when replicated. The cross-condition effects could be a result of differences in how engaging the experimental conditions participants received were, but the lack of replication of this suggests this is not the case and that generation simply varies greatly. One implication of such variability is that experimental sample size was not big enough for conclusive results regarding this variable in the experimental work so this outcome may need to be treated with caution. This was one limiting factor in using the open response measure to provide training outcomes: it is previously un-trialled in a pre- and post-intervention format and thus direct effect size estimation was difficult. Reliability may also be an issue here.

Generation was calculated as a relative proportion (of SJN responses) of the overall number of scenarios generated by each participant. This was necessary in order to account for differences in overall number of scenarios that each participant was able to generate, and any overall variation in these over time. It was necessary to also keep the variable specific, as opposed to considering general negativity, in order to maintain the focus on self- oriented, social-judgment-related negativity that is salient to socially anxious individuals (and as such that the thesis as a whole emphasises and that the other variables from the open response task also featured. One practical implication of this however is that a participant could generate a greater amount of SJN content but also a greater amount of general, non-SJN negative content and the latter would limit the effect of the former in increasing the generation index. Whilst this has slightly greater implications for the experimental portions of this thesis than the current chapter, it is still important to consider here. This is because participants may vary in the specificity of their interpretive bias. Some participants might for example only feature a high fear of negative evaluation and no other anxiety (or other negatively-preoccupied mental health condition such as depression) symptomology. Their bias might be very specific. Other individuals however might exhibit a more general anxiety reactivity, or an accompanying depressive inclination, and their bias might be more general. This would thus obscure a specific SJN bias even if this existed. Participants were screened for clinical diagnoses but this may have been, in retrospect, insufficient. However, it would be difficult to completely avoid such noise variance without screening for individual differences in general anxiety, depressive symptoms and a host of other cognitive attributes which may predispose an individual to general negative interpretations, and thus severely limiting the possible sample size (which was restricted considerably already).

Open Response Task Performance: Bias in Likelihood Approximation of Responses

The hypothesis that trait anxiety would predict interpretive bias in participants' likelihood approximation of social judgment-related negative responses to the open response task was supported by the analysis. Furthermore no consistent state predictive or moderational relationship was observed. The trait association found suggests likelihood to be a better example of a social anxiety-accompanying bias. These results are consistent with a small number of studies which have previously suggested such a likelihood bias in socially anxious populations. Those studies in existence that do concern the approximation of probability in socially anxious individuals are reviewed by Nelson, Lickel, Sy, Dixon and Deacon (2010), who conclude tentatively in favour of such a bias overall.

Such a bias in likelihood is concordant with prominent theories of anxiety. In reinforcement sensitivity theory (Gray & McNaughton, 2000) for example, emphasis is placed on anxiety as an uncertainty and goal conflict emotion. In the current investigation, participants (who all had reasonably high FNE scores) featured a mean likelihood rating for SJN content of 3.41 (from a possible 7), with a standard deviation of .96. This remains moderate and far from "conclusive" in favour, overall, of such events regularly occurring. As such, the increased likelihood approximation of SJN outcomes demonstrated by individuals with higher fear of negative evaluation may not represent any degree of certainty that events in which they will be socially judged in a negative manner will occur (which would fall under a distinct fear and avoidance response in Gray's model). Instead it suggests a *reasonable possibility* of such events occurring, rather than a firm conclusion. As such it is concordant with RST's emphasis on the individual becoming anxious when *deciding between* potential approach, avoidance or freeze responses for the potentially threatening scenario, rather than in response to the scenario being definitively necessary to avoid completely.

To contrast and clarify this point, if the current series of studies were conducted with fully phobic individuals (with high levels of the conceptually distinct *fear* and *avoidance*), considerably different results might be expected to occur. This population could be expected to feature considerably higher bias in likelihood (to a much more conclusive level, with perhaps a mean of five to six) indicating a more conclusive decision that negative outcomes will ensue in situations of uncertainty. This might be accompanied by greater catastrophisation in evaluation of these responses also, which would further the necessity of being over-cautious with avoidance (and may help to explain the lack of findings for evaluation detailed in the below section).

Open Response Task Performance: Bias in Evaluation of Responses

The hypotheses that trait and state anxiety would predict interpretive bias in participants' evaluation of the social judgment-related negative responses to the open response task were not supported by the analysis. The associated hypothesis that state anxiety would moderate the influence of trait anxiety on interpretive bias was also unsupported. Evaluation, when measured in this way, (and accounting for the results of the experiments described in the third experimental chapter) appears to be entirely independent of trait or state affective influence (the latter receiving further affirmation in the latter section of this chapter). This is an unexpected result, given the existing literature regarding anxious individuals' inclination to catastrophise (Nelson, Lickel, Sy, Dixon & Deacon, 2010). It may be that the open response task, in attempting to capture a number of aspects of interpretive processing in a single task, did not allow for the comprehensive measurement of evaluation, and reasons for this are considered below. However at "face value" as evaluation showed no significant association with participants' scores on any of the trait or state measures it thus, like generation, appears to represent a relatively independent aspect of the way in which individuals process ambiguous material. Assuming no mediating or moderating factors it is therefore necessary to tentatively conclude against evaluation of potential outcomes being an instrumental associated factor in an individual's social anxiety, whilst exploring reasons why this might not be the case.

For such evaluation, it may be that the differences between the existing studies and the current body of work lie in the method of measurement. In several of the mentioned contemporary studies, evaluation was conceptualised as "cost". Measurement across these was notably inhomogeneous, as Nelson, Lickel, Sy, Dixon and Deacon (2010) note. However it tended to focus on longer-term costs. The current empirical work took a slightly more present and state affect-focused approach, asking participants about the severity of how they would feel if the outcome in question did occur. In retrospect, a longer-term component would have been useful to account for any potential distinction here. Nonetheless, the variable targeted is subtly distinct which may explain some of the difference in results gained.

Furthermore, the current series of studies used participant-generated content whereas contemporary prior work has focused on preset scenarios. This may have had considerable implications for outcomes. Firstly, it should be noted that it has the potential to amplify the relevance of each situation to the individual participant (a key reason for incorporation of the open response measure). In doing so it does allow for a greater range of content to be generated, from very mild to extremely negative social judgment outcomes. As Nelson, Lickel, Sy, Dixon and Deacon (2010) consider, when a range of severity of outcomes has been incorporated, and changes in probability bias have been accounted for (a variable found to be key in the current work as well), the "cost" bias measured was no longer instrumental (see McManus, Clark & Hackmann, 2000). This evidence for probability rather than magnitude of outcome as the instrumental factor is thus highly consistent with the results gained in this chapter.

General Discussion

The overall findings present an expanded portrayal of the interpretive processing of individuals high on fear of negative evaluation. Higher FNE individuals appear to have increased levels of state and trait anxiety-associated interpretive bias in their fast, binary resolution of ambiguous content (as measured by the SST), which features a significant moderation interaction between these variables without conditions of cognitive load only. They also appear to have increased levels of a trait-associated bias in their approximation of the likelihood of negative social judgment-related outcomes occurring in ambiguous scenarios. They do not think of more of these scenarios when confronted with ambiguity, and those that they do think are not perceived to be more catastrophic than those of individuals with lower FNE scores.

In addition to those specific to each variable, the findings have a number of important general caveats. One lies in the population used. For a number of practical reasons at the time of inception the studies were run with nonclinical individuals and thus may not have comprehensive comparability with the contemporary studies using clinical populations. This said, a number of key measurement and modification studies in the interpretive bias literature have taken place with such samples, with generally positive results (see Eysenck, 2013). Nonetheless however if social anxiety is best considered as a continuum (and presence of the associated biases similarly conceptualised), then the analysis included in this thesis may present an attenuated version of the results that would be found with clinical populations.

The focus on social judgment is an aspect of further note, as prior studies have generally looked at wider implications of social anxiety symptomology. Again this was a deliberate choice in the current work to provide focus on a single instrumental aspect of social anxiety throughout the thesis, but does mean a further divergent element of method from prior work and may be responsible for some of the similar divergence in results found. Participants may display broader social anxiety-associated interpretive biases (for example concerning anxious physiological sensations) but may not consistently display a “fear of negative evaluation”-focused bias in all of the measured facets. Furthermore this is something which may vary inter-individually, and the following chapter considers the role of individual differences in interpretive bias research in greater detail.

The analysis conducted in this chapter is that it has a larger sample size than prior work. Existing measurement studies have focused on smaller comparisons between affectively distinct groups such as high and low anxiety students (N = 94: Constans, Penn, Ihen & Hope, 1999) and clinically socially anxious individuals pre-and post-cognitive therapy (N = 36: Wilson & Rapee, 2005). Furthermore the current chapter applies multiple measures across the targeted subpopulation of high, nonclinical socially anxious individuals. The open response measure applied is a particular potential key advantage over contemporary work. In its basic format applied by Dodd, Stuifzand, Morris and Hudson (2015) it allowed comprehensive measurement of generation across the breadth of participants’ responses, and accounted for first responses and evaluation of participants’ perceived most likely responses. This thesis further expands this measurement of evaluation to all the potential outcomes considered by participants, and looks at likelihood not simply at the level of a single, designated “most likely” response but by accounting for the likelihood of all perceived possible outcomes. This was considered key to the investigation of anxiety as an emotion evoked by the uncertainty of considering *multiple* possible outcomes, *some* of which involve negative social judgments. This slight redefinition (whilst, as acknowledged, not able to account for the biases in relation to the first response to “spring to mind” for each participant) allowed the measure to investigate the constituent biases in considerably greater detail and with considerably greater statistical power than prior work. Indeed, this allowed the evidence for a bias in overall likelihood approximation to be provided in the current chapter. Whilst null results were gained for generation and evaluation in the same way, this evidence is considered important in itself, if preliminary.

In addition to the variable-specific outcome discussed in dedicated sections, there are however certain general limitations of the open response task which may have exerted effects to produce the outcomes observed in this chapter of analysis. Firstly, the task, whilst implemented as a way of increasing the overall ecological validity of bias measurement in this thesis, may have encouraged greater ruminative processing than an individual may undertake in a socially ambiguous situation. When confronted with ambiguity, a socially anxious individual may choose to focus on conflict between the probabilities of, perhaps one salient benign and one salient negative outcome. The fact that the open response task featured ten boxes may have encouraged participants to think laterally and creatively about the outcomes that they gave, and to consider obscure possibilities. Participants were not told to generate as many responses as they could but, in retrospect, the task could have featured a less response-soliciting format, perhaps with a single large box for participants to enter all their answers and ratings in.

Even if the above were not the case and the task is accurately reflective of processing, individuals with initial cognitive biases may, if given enough time and situational scope, simply be able to correct their initial tendency to interpret negativity. Initial negative responses could be counteracted by participants “talking themselves out of negativity” with subsequent responses. Participants may even have adaptive existing strategies to limit the impact of their social anxiety over the longer term. They may, in their generation of response, be able to “talk themselves out” of an initial bias with further benign responses, or by rating these as less likely or less significantly catastrophic than they might initially perceive them to be. In this way, even the completion of a task which asks participants to expand upon and clarify the nature of their anxious cognition may in fact even exert small “training effects” for certain participants. If socially anxious individuals’ biases are constrained to their earlier responses and better defined this way, the open response task may not be an optimal method of measurement for them. Accordingly a task that appears to be relatively reliant on system two (2003) processing may not be ideal here. Interpretive bias studies in general do not account for individuals’ potential capacity to self-regulate the impact of their interpretive biases. Indeed, this may partially explain the generally modest clinical improvements achieved in studies in which considerable change on bias is evidenced (Cristea, Kok and Cuijkpers, 2015).

In terms of its general (cognitive load independent) nature, the scrambled sentence task also has some general limitations of relevance. In being closed-resolution and categorical in each item's measurement and in allowing only a single-response per item, its limitations are primarily of scope and ecological validity. Whilst these may remain the case, the evidence gained in the current chapter supports its basic validity as a measure of a trait social anxiety-associated bias. However, this is not to understate the importance of the state association also found, and the significant state moderation found for the task without conditions of cognitive load. Scrambled sentence responses are affected by state affect and thus not necessarily comprehensive indicators of a stable, FNE-associated bias.

It may be the case that, like the trait and state affect measured in the current analysis, interpretive bias too may have both invariant and more flexible, responsive component. Fast, relatively on-line responses like those that the loaded SST aims to capture may have a relatively state, responsive and "primable" element, hence their additional state association. More trait-associated and potentially more ruminative interpretive tendencies appear to also exist, such as the trait-only association for likelihood found. This said, a considerable amount of variance in likelihood was unexplained by the model implemented and there may be other instrumental cognitive variables requiring investigation here.

For the scrambled sentence task, the finding of a strong trait association helps to generally affirm the potential of the task as a reasonable measure of a social anxiety-associated bias in experimental work. The presence of a state association in addition however suggests caution here and that, for example, variation in participant short-term anxiety levels during an experimental procedure may be enough to exert considerable extraneous variance on bias measurement outcomes. The overall tentative conclusion is thus that the scrambled sentence task may not be an ideal indicator of a stable interpretive tendency and other measures may need to be applied.

The distinct results gained for likelihood measurement meanwhile indicate a trait-only associated bias (although, as noted, one with considerable unexplained variance). Such a finding could nonetheless be followed with further investigation. In particular, bearing in mind the failure of the experiments in this thesis to significantly influence likelihood, it may be the case that dedicated training targeting this bias exclusively is required. This might, for example, take the form of having participants estimate likelihoods of negative social events, and then showing them how likely these, on average, really are. Given the potential instrumentality of likelihood approximation in anxiety as outlined earlier in this discussion, such procedures could prove instrumental in the further modification of interpretation and the alleviation of maladaptive anxiety symptomology.

The distinct trait/state associations found for the above biases demonstrates the variation in nature of bias in these two distinct components of overall interpretation, and suggests variance in their drawing upon system one and system two (Evans, 2003) processes, particularly the apparently heavily automatic loaded scrambled sentence task and the more ruminative open response task. It adds evidence to the general cautionary emphasis of this thesis regarding bias measurement techniques, suggesting that their associations and determinants, and constituent cognitive processes could receive greater scrutiny in the design of interpretive bias empirical work in the future.

The absence of any significant predictive relationship between trait and state anxiety and response generation and evaluation meanwhile suggests that (in the absence of any clear and feasible moderating variables) these are not candidate faculties for a social anxiety-associated bias and that future research might be more productively implemented elsewhere.

For the exploration of new and more fine-grained cognitive abilities for biases associated with social anxiety, this chapter, in combination with the prior ones, suggests that such a search should be primarily implemented elsewhere. Individuals with high nonclinical social anxiety only appear to show higher levels of likelihood approximation for social judgment-related negative events, and do not differ in how many of these they generate or how they approximate their severity. Future work could affirm that this is the case for clinical samples also, and explore the likelihood approximation in greater detail. It may be more productive however to explore potential biases in other facilities, for example biased approximation of covariation, or expanding upon recent insights into probabilistic learning regarding self –relevant social judgments in socially anxious samples, as per Button, Kounali, Stapinski, Rapee, Lewis and Munafo's (2015) (which is concordant with the findings regarding likelihood approximation in the prior chapter). Whilst beyond the scope of the current series of empirical investigations (which focused on participant-generated content, making covariation and similar variables difficult to manipulate), these may prove a productive focus for future work.

At theoretical level, what the results suggest are that further empirical research into biased interpretive processing in socially anxious individuals needs to inform and be informed by a substantial body of further theoretical work. In particular, typical processes of interpretive decision making could be made clearer and the roles of fast, online biases and slower, more ruminative ones comprehensively mapped out. Whilst current models (e.g. Hofmann, 2007; Clark & Beck, 2010) are useful as fundamental, broad schematics, many of these lack detail on the cognitive processes involved. In order to consider the temporal sequence of such events it may be necessary for such theory to draw more from structural and functional imaging work (in order to capture the nature and location of the components of this rapid series of cognitive occurrences), and to link attention, interpretation, memory and other faculties of relevance in a single, more comprehensive model.

Chapter Seven: Conclusion

This chapter brings together the findings of the thesis in relation to the initial areas for investigation identified in chapter two. It contextualise the results gained in terms of these and practical constraints on the thesis, and provides recommendations for future research.

Summary of Prior Chapters and their Focus

The first introductory chapter of this thesis outlined key cognitive models of emotional pathology, the host of biases that they imply, the measurement of these biases and how the field of interpretive bias modification developed in response to attempts to formalise modification of these biases. In the second introductory chapter, the latter field was noted as having a number of shortcomings and the primary ones to receive consideration in the current thesis were outlined as:

- A rush to clinical application, with a need for replication and conceptual clarity regarding mechanism of effect before clinical implementation.
- Limited outcome measurement including consideration of near and far transfer and automatic and controlled processing.
- Potential for method improvement in CBM-I designs via the consideration of implicit/explicit nature of tasks and use of participant-generated content.

These were then incorporated in feasible ways into the empirical work of the current thesis. All of the experimental studies incorporated a strong element of replication, and near-replication and extension of prior work, using multiple and varied measures to better track outcomes and provide a degree of conceptual clarity both within experiments (chapters three, four and five) and in cross experimental analysis (chapter six). New methods featuring explicit instructions and participant-generated content were trialled alongside existing ones (chapters three and four). Conceptual clarity regarding the roles of trait and state affect and attempted insights into the likely mechanism of effect of CBM-I were investigated in greater detail via the manipulation of mood during a CBM-I procedure in a series of experiments (chapter five) and with cross-experimental analysis of pre-training data from the experiments detailed in the third and fourth chapters (chapter six).

The current chapter thus summarises and integrates these findings, considers their potential limitations, and then details their overall implications for theory and experimental practice.

Summary of Results

The first experiment of the current thesis (chapter three) considered the role of explicitness in CBM-I training. It was considered necessary to partially replicate and extend Mobini, Mackintosh, Illingworth, Gega, Langdon and Hoppitt's (2013) findings with broader outcome measures, and to provide further clarity on the role of explicit and implicit processing (and implicit vs explicit instructions within this) in CBM-I designs. Findings indicated marginal superiority of standard, implicit CBM-I training on scrambled sentence measure-based biases with and without conditions of cognitive load, but not on biases measured by an open response task and a lack of statistical significance at a between-group level.

The second experimental chapter extended the experimental work of chapter one to consider the role of self-generated content in CBM-I designs, whilst providing replication of the key existing experimental conditions from the first experiment. Results were primarily null, with no significant differences, and the limited pattern of significant findings from the previous experiment did not replicate. Possible reasons for this were considered, including ceiling effects and the role of state affect, which informed the design of the constituent experiments in the following chapter.

Informed by the prior results, the third experimental chapter directly investigated a number of remaining aims and clarificational issues, implementing a set of three experiments. All the experiments followed a unique two-stage design of negative followed by positive bias modification. The first considered the comparative effectiveness of a standard closed-resolution CBM-I condition against a condition in which control "sham" training was implemented and participants instead received a simple mood induction. The second compared the same CBM-I program alone with identical training implemented simultaneously with a positive or negative mood induction. The third experiment looked at the comparative performance of the standard CBM-I program with its open response equivalent under these mood inductions. Findings across these experiments were again mixed and primarily null. The small array of significant effects observed was constrained to the first (negative) training program implemented in each of the experiments, and to the open response task and not the scrambled sentence bias measure. For the first experiment the group receiving CBM-I only generated significantly more social judgment-related negative endings, and rated such content as significantly more likely to occur after training (in comparison with their pre-training performance). This was not however significantly different from the outcomes in the mood induction and sham training group (although these groups demonstrated non-significant within-group change).

In the second experiment, three groups receiving the standard CBM-I program were compared. Two of the groups additionally received a basic embodiment-based mood induction task derived from Strack, Martin and Stepper's (1988) work, and were compared to a control group receiving the CBM-I intervention only. All three groups generated increased amounts of social judgment-related negative material, and the groups receiving negative mood induction and no mood induction approximated such scenarios as being more likely to occur post-training whilst the group receiving no mood induction did not (although this was in comparison to their pre-training ratings and did not form a significant between-groups difference).

In the final experiment the standard CBM-I group who received a positive pen task were compared, again in terms of their performance in response to negative and then positively-valenced CBM-I training, with a group receiving an equivalent open response CBM-I intervention which used participant-generated content (and who also received an identical positive pen task). Participants in both groups generated more of these scenarios after training and perceived social judgment-related endings as significantly more likely to occur after the first training, with no significant differences between them.

A fourth analytical chapter brought together findings from across the first pair of experiments to consider baseline levels of bias in the population in greater detail, and to look further at possible non training-based antecedents of bias change. Findings from the first section of analysis indicated a close association between fear of negative evaluation and scrambled sentence task score, and state anxiety and scrambled sentence task score, independent of presence of cognitive load. In addition, a strong association between fear of negative evaluation and perceived likelihood of generated social negative events was observed.

The following section contextualises these findings in light of the aims of the thesis and considers the way in which the research undertaken has furthered and clarified each. Furthermore, implications for further work and theoretical considerations are expanded upon.

Method and Practical Experimental Issues

In designing the constituent studies of this thesis, a number of inevitably competing concerns were navigated, with a similarly inevitable element of compromise. In this section the key conflicts and compromises that became salient in the creation of the current thesis are highlighted, the positions reached on them detailed and justified, and their implications for the thesis outlined.

Selection of training and measures in this thesis was designed to balance the needs for a reasonable anticipated effect size from the active manipulations implemented, with practical constraints on session length. Temporal effects such as boredom and disengagement from the repetitive nature of CBM-I were a concern. This and the fact that it had been decided that participants would complete several outcome measures of considerable length meant that balancing the various tasks that participants would complete in the experimental paradigms was necessary. As the studies were considered primarily proof of concept and not trials of clinical efficacy, single-session training was considered appropriate. Furthermore there is not strong evidence that multiple training sessions enhance interpretive bias beyond the levels achieved in a single session (Cristea, Kok and Cuijpers 2015), and they may simply serve to consolidate bias longevity rather than enhance bias. In addition, whilst they might have provided some longitudinal insights such prospective paradigms may have complicated findings by exerting between-session noise variance into the outcomes gained.

In order to achieve a meaningful sample size for multiple experiments it was necessary to conduct the experiments with individuals from the general population, self-recruiting these through a variety of means. Initial experiments were run with participants meeting minimum criteria in terms of Fear of Negative Evaluation score primarily in order to ensure that the work remained a reasonable analogue for clinical samples and to ensure a workable level of negativity of interpretation in the samples gained. This was removed and replaced with initial training aiming to induce negative interpretation after the first two experiments for two main reasons. The first was concern regarding “ceiling effects” with certain individuals who clearly showed limited bias despite a high indicated level of fear of negative evaluation (the prior chapter shows that FNE score, whilst showing considerable association with starting bias for many measures, did not predict the majority of the variance for the bias measures implemented), thus limiting the demonstrated effect of the active interventions implemented to reduce negative bias. Secondly, limited amounts of volunteer participants scored above the cut-off on the FNE measure (as might be expected, volunteer applicants for the experiments tended to have low fear of negative evaluation). For practical recruitment reasons recruitment of individuals from the general population without an anxiety minimum was considered. It was anticipated that such individuals might not have sufficient levels of negative interpretive bias at the start of the experiment, hence the implementation of initial negative training.

Inevitably, a further issue with running a series of quantitative experimental investigations such as those contained in this thesis is the negotiation of both acceptable ecological validity and control. Regarding the former, one of the key reasons for implementing the variants of CBM-I training and interpretive bias measurement used was the enhancement of ecological validity. For training, it was noted that the constrained resolution of a pre-set fragment is not a particularly accurate analogue for the situation that an individual faced with ambiguity experiences. Whilst some promising results with such procedures have been found, it was thus hoped that an open-ended, generative program would better model this situation.

Correspondingly with the above, for bias measurement the creation of sentences from sets of words in the scrambled sentence task was also not considered to provide a comprehensive measurement of bias. This was primarily due to its lack of an analogue for the cognitive situation facing an individual, when dealing with an ambiguous situation (the introductory chapters detail this concern with measures of a closed resolution, single-answer and single-index nature for the field in general). As such, the open response task was adapted and incorporated to provide a potentially more ecologically valid method of measuring outcomes. The failure of two of the constituent variables to display consistent association with FNE score or modification suggests that it may not have been entirely suitable however, as discussed above and in the discussions of the experimental and cross-experimental chapters.

One element which was apparent from the start of the design of this project was the absence of direct data on the reliability of the open response task. Whilst such data would have been highly useful, it was not feasible to gather due to the nature of the task. In particular the facts that participants generated their own responses to items, and provided multiple responses to singular items, meant that the implementation of conventional indicators of reliability was not possible. Whilst the benefits of ecological validity and expanded bias measurement with multiple indices were considered to outweigh this, nonetheless this decision did result in a potential lack of experimental control for the variables involved. Future designs may benefit from expanded, but more standardised, reliability-assessed measures to ensure such control.

Despite the efforts to enhance ecological validity, and the potential loss of reliability in using the open response task to measure biases, an acceptable standard of overall control was maintained. The implementation of explicitly-instructed training for example, whilst suggested by prior work to be a worthwhile investigation in itself, also functioned as a control condition and as such was particularly instrumental in facilitating the isolation and consideration of the combinations of variables implemented in this experiment. It allowed the provision of explicit instructions alone to be reasonably accounted for. Furthermore, in combination with the control condition in which an unconstrained, uninstructed open response program was completed, it allowed every combination of implicit/explicit and closed/open resolution to be studied. Although outcomes in practice were limited due to other factors (discussed elsewhere), this element nonetheless allowed more conclusive implications to be drawn from the method implemented.

An additional notable aspect of the experiments in this thesis is the alignment of study design, with measures remaining similar and experimental conditions providing the main variations between studies. Again this was implemented to facilitate control and more concrete cross-experimental comparisons. Furthermore, the alignment of design allowed, in the first two experiments, an element of replication to be present for the key experimental conditions, substantiating the generally null results gained in the first experiment.

A further cross-chapter issue for the current thesis is specificity of focus. Again the main concern here was control over variables, and the decision to focus on social anxiety and in particular the key Fear of Negative Evaluation component was a result primarily of a need to eliminate as much extraneous variance as possible. Cognitive biases have been evidenced in many cases as extremely content-specific (Stopa & Clark, 2000), and it was considered feasible that the conflation of distinct processing tendencies here could obscure the specific mechanisms involved. It may be however that this focus was too specific as other studies have found effects whilst looking at arguably more general inclinations towards negativity. It may be for example that individuals do not consistently show a bias in fear of negative evaluation-evoking content or in other cognitive aspects of social anxiety but that instead there is a general tendency across these faculties, which could vary inter-individually. It may also be the case that fear of negative evaluation has significant mediating or moderating cognitive variables in a feasibly complex relationship with interpretive bias, and this is discussed further in a section below.

Summary of Practical and Theoretical Implications

The need for conceptual clarity before widespread clinical application. This initial observation has been substantiated at the general level by the evidence gathered in this thesis. Salient here is the distinction between the two bias measurement tasks implied and the implications this has for theory concerning cognitive biases. The results from the scrambled sentence task are generally concordant with Williams, Watts, MacLeod and Mathews (1988) application of Bower's (1981) model. They emphasise early, and overwhelmingly threat-determined allocation of early processing resources, modulated primarily by trait anxiety. The scrambled sentence task data demonstrated this, with a strong trait association for negative outcomes from this early-stage processing task. The task may even be such early stage processing that it is arguably more based in an attentional bias than an interpretation bias per se (as mentioned in the introductory chapter there is potential for considerable interrelationship and overlap in these processes). The participant is presented with a set of six words and, assuming the accuracy of attentional bias studies, particularly those applying visual search and dot probe paradigms (see the introductory chapter for details on these tasks and their findings), is likely to pay disproportionate attention to any word that is a potential threat. Importantly, many of the words in the scrambled sentence task are overtly negative alone, while others are only negative in context. An example of the former would be:

Stupid say rarely something often I

An example of a sentence which does not have an overtly negative word and only gains negativity at a latter stage of unscrambling would be:

up phone never friends always me

As such there is a strong early attentional and automatic component to participants' initial consideration of some of the items of this task which may influence the subsequent arrangement of the words (and the competition of benign and threat-relevant explanations for the ambiguity they present, as per Mathews and Mackintosh (1998)). Beyond the determining effect of single words however, high trait anxiety participants may broaden their search to include clusters of words with overall negative implications before completing the sentence, enhancing the effect of such relatively early-stage processing biases further. Applying eye tracking during such a task would provide further insight into the early attentional determinants of scrambled sentence task performance.

The finding that training was unable to exert influence on the above processing but that state anxiety showed an association with it also has considerable implications for theory. It suggests an amenability of early-stage interpretive bias to mood states (albeit chapter five suggesting the pen task did not perform as a reliable manipulation of these) and a resistance to external influence. In doing so, a partially state affect-based account of interpretive bias (at least early interpretive bias) is supported whilst a production rule-based account (e.g. Mathews and Mackintosh, 2002) of its modification did not receive support in the current series of work. Note that this does not mean that a mood priming account of CBM-I's effect in bias change was supported, as no evidence for this emerged from the dedicated investigation in chapter five, which remained inconclusive regarding accounts of mechanism of action of bias change. The affect-rooted account is highly concordant with Bar-Haim et al.'s (2007) theorisation, which is the closest model to an embodiment-based account to be found in the interpretive bias literature. This emphasises the instrumentality of state affective arousal early in processing. They suggest that the activation of short-term affective arousal following pre-attentive threat detection is integral to the allocation of further cognitive resources (including subsequent cognitive consideration).

The open response task meanwhile demonstrated the existence of a relatively late-stage bias in the approximation of likelihoods of relevant negative events in high socially anxious individuals, resistant to the basic CBM-I training applied. In addition it did not support the notion of bias in the overall nature of the responses that such individuals generated and their affective evaluation of such scenarios. The resulting emphasis on bias in likelihood approximation (particularly its moderate nature, as discussed in the prior chapter) is concordant with theoretical positions designating anxiety as an emotion of cognitive goal conflict (e.g. Gray & McNaughton, 2000), as well as indications from studies which have asked participants to estimate the probabilities of preset negative events occurring (e.g. Foa, Frankin, Perry & Herbert, 1996; Smári, Bjarnadóttir, and Bragadóttir, 1998). Where the current thesis goes beyond this is to evidence bias in multiple participant-generated events in response to the same initial stimulus, a design which potentially amplifies ecological validity considerably. Furthermore, the open response task featured no time limit or cognitive load and thus allowed for greater ruminative and later-stage processing. Whilst it is noted that proportions of automatic and controlled processing (and even the constituent cognitive processes for generating responses) may have varied between participants, the overall outcomes of this task are taken to indicate a late-stage, reflective bias in individuals with high social anxiety. As such, theories such Wells and Matthews' (1996) cognitive model become relevant for their emphasis on conscious decisions to remain vigilant. It may even be that the decision of socially anxious individuals to remain vigilant that is emphasised in this model is related to these individuals' consistent belief that the occurrence of negative events is more feasible.

Whilst a likelihood approximation bias may manifest at a later stage of processing, it is important not to consider it as existing in isolation. Indeed, the kind of early-stage, automatic selective attention processes evidenced in the research base (Bar-Haim, Dominique, Pergamin, Bakermans-Kranenburg & Van Ijzendoorn, 2007) and posited as influential in scrambled sentence task performance above may be influential here. The above said, the overall implication however is that investigators of interpretive bias (including those applying measures as outcomes of training) need to account for biases at both early and late stages of processing, something which the application of the simple tasks applied in many prior studies may have failed to adequately portray in appropriate detail.

An element which became apparent in the analysis of data overall for this thesis was the presence of individual differences not just in levels of bias (as discussed elsewhere in the context of potential ceiling effects in the early studies), but in change in bias regardless of initial levels, with various participants displaying change in inverse directions to the targeted valence of the training. This makes the notions of responders and non-responders, and sufficient conditions for change, from the CBT-focused literature (e.g. Wilson, 2006; Bryant et al. 2008; Konarski et al., 2009) a salient possibility for CBM-I research also, and even a potential explanation for the mixed findings of Cristea, Kok and Cuijpers' (2015) analysis. At the very least, the inter-individual variables involved in receiving successful bias modification from a single-session scenario CBM-I intervention need considerable exploration, and to be accounted for in a standardised way before such interventions could receive widespread implementation. This is a key implication: the investigation of what cognitive characteristics might produce a CBM-I responder may be a fruitful pursuit for researchers looking to provide meaningful, ecological valid and efficacious interventions focused on cognitive biases.

In order to provide this area of research with a comprehensive theoretical underpinning, the cognitive models outlined in the introductory chapter of this thesis may need to be expanded, adapted and synthesised to provide a coherent and comprehensive account of the role of cognitive bias. Whilst they make a number of valid general statements regarding the cognitive nature of anxiety, contemporary anxiety theory is arguably not specific enough to account for the minutiae of the interface between specific cognitive processes, biases in them, their interrelationship, and the interface of these elements with key cognitive attributes (for example attentional control, or perceived self-efficacy). It may be the case that an individual, or at least individual differences-centred approach to anxiety and processing biases could extend even to the application of such models. By way of example, it may be that one participants' social anxiety is maintained primarily through a very limited internal feedback loop of bodily sensations and the interpretation of these which they find difficult to disengage from. Another participant meanwhile may have an array of externally-focused processing biases in their automatic ambiguity resolution processing and their likelihood approximation, of the kind demonstrated in the current body of work. Furthermore, participants may vary in the cognitive variables contributing to or otherwise associated with these variables for example need for cognition, self-efficacy or motivation, elements which are currently unaddressed in CBM-I work.

In the above context, whilst their specificity and simple nature is considered a highly positive aspect of CBM-I interventions, it becomes clear there is some potential for further debate here too. In particular such debate and the accompanying investigation might be productive if it considered the interface of interpretive bias (and indeed broader cognitive biases) with personality, and the extent to which CBM-I procedures could be inter-individually customised to account for this. By way of example future, more efficacious design for computerised interventions targeting biases might thus involve an initial assessment phase for a variety of biases and influential cognitive variables. This could then be followed by algorithmically-adaptive training designed to target as much of each individual's array of biased cognition as possible.

The latter series of experiments was particularly designed to provide insight into the independence of mood and bias. Unfortunately the primary implication of the evidence gained is that the interventions implemented were unable to exert significant effects on these variables. As such specific and detailed implications from this work are not forthcoming. However, the fact that training effects were primarily unable to be achieved over the considerable experimental session implemented does reinforce the need for caution with CBM-I techniques. As discussed in the corresponding chapter, there may be intervening factors that limited training effects. Fundamentally however the fact that even what (non-significant) change was observed was not in the directions that would be expected leads to the necessity of taking this experiment at face value as a failure to implement cognitive bias modification. It thus has a cautionary implication overall. At the very least, further variants of CBM-I need to be reliably applied across multiple studies, and importantly with homogenised, agreed-upon measures of outcomes before it can be considered a reliable clinical intervention.

The assessment of the nature and limitations of current methods of measuring cognitive biases. This element was addressed in the thesis via the implementation of two highly distinct bias measurement tasks. To recap briefly: the former, the scrambled sentence task is a simple, existing measure focusing on binary resolution of scrambled word sets into sentences with positive or negative implications. It is thus, as outlined in the introductory chapter, a closed resolution task. It is time-limited and so elicits participants' first, minimally-ruminative responses. Participants provide a single response to each item. The task then provides a single summary index of bias. At the outset of this thesis all of these aspects were considered, whilst potentially pragmatic at the time, as less than ideal for a comprehensive measure of bias. As such the task was incorporated but contrasted with the second measure: the open response task.

The open response task was implemented as the near inverse of the scrambled sentence task in these respects. It is open-resolution, with participants free to provide the answers they choose, and as many of these as they are individually able. There is no specific limit on completion time (in order to allow for the varying breadth of responses that participants provided), and it provides multiple indices of bias. These elements were all intentional and incorporated in an attempt to measure bias in a broad and comprehensive manner with enhanced ecological validity for each participant. They allowed all the outcome options that a participant might consider to be listed and the perceived likelihoods and affective impact of these to be approximately gauged. Furthermore, in allowing a participant to provide their own content and not respond to a potentially irrelevant preset answer, it was hoped that bias measurement with a greater resemblance to each individual's real-life processing would be achieved. It is worth considering the limitations of such an approach however. Certain potential limitations are discussed in the empirical chapters themselves including how establishing reliability is difficult for such a task, how increases in general as opposed to social negativity could limit findings, and the general sacrifice of control in favour of ecological validity that using open response measures necessitates, with particular concerns around participants elaborating upon and "talking themselves out of" any initial bias.

It is nonetheless useful to take an alternative perspective and take the open response task at face value as a reasonably effective measure reflective of processing in socially anxious individuals confronted with ambiguity in order to fully explore the potential implications of findings gained with it. It is necessary to consider the separate variables taken from the open response task individually here, especially given the results of the cross-experimental analysis. Whilst none of them varied significantly in response to training, there were key differences in the anxiety associations they demonstrated. Likelihood approximation is particularly salient here. This variable demonstrated a significant association with fear of negative evaluation in the cross-experimental analysis. Likelihood approximation may thus be a genuine candidate for a further fear of negative evaluation-associated bias, and this is consistent with a small body of existing literature suggesting (using closed-resolution techniques) that socially anxious individuals have biased likelihood approximation (Nelson, Lickel, Sy, Dixon & Deacon, 2010). It would thus be useful to explore such a bias further, potentially with more targeted, direct manipulation. There may be more effective methods of directly manipulating likelihood approximation under experimental conditions, for example having participants directly approximate likelihood under constrained circumstances during a training program.

Generation meanwhile displayed no significant association with trait or state anxiety, or amenability to training. As such it appears to be a highly independent capacity. It does however appear to generally exhibit an increase in negativity of bias across time in many cases within the specific experimental designs implemented. Reasons for this are difficult to ascertain. It may simply be an effect of the way in which it was necessary to create the generation variable. An element of compromise was necessary in this. It was necessary to have a ratio of SJN content to non-SJN content and not simply a number of SJN scenarios (which might provide too much noise variance due to individual creative capacities of participants in generating material in general). This inevitably meant that differences in general negativity could have some influence on results however: a decrease in general negativity and/or general positivity would result in a greater proportion of social judgment-related negativity. Participants’ positivity may for example have been attenuated by temporal effects such as boredom within the experimental designs.

The lack of significant association with affect however suggests that generation (at least when measured in this way) is unassociated with social anxiety and potentially one of no direct relevance to it. High and low-socially anxious individuals may have very similar overall interpretive generative inclinations when attempting to resolve ambiguous social situations. Differences may lie more in the way that they assess the likelihood of these resolutions actually occurring.

Evaluation was similarly unassociated with bias, and displayed little temporal variation (both generally and in response to experimental manipulation). Again taking the open response task at face value, but bearing in mind the caveats expressed above, it is necessary to tentatively conclude in favour of it not being instrumental in fear of negative evaluation. This may be an artefact of the samples taken. In participants with fully clinical social anxiety, it may be the case that evaluative catastrophisation is more prominent. In high nonclinical samples however, the anxiety may be more focused around moderately negative events and the relative likelihood of these to occur.

For generation and evaluation then, it may simply be the case that individuals high on fear of negative evaluation genuinely do not significantly differ in much of their subsequent, more ruminative processing (or at least in terms of their generation of competing explanations and their catastrophisation of negative ones). The outcomes gained suggest that they may feature a fast, unreflective and potentially implicit initial bias (that measured by the scrambled sentence task), and this may be limited to the binary positive/negative nature of the situation and not its severity. It appears resistant to modification via the techniques implemented. The outcomes gained do also suggest however that individuals with a higher FNE score tend to have a similarly training-resistant bias in the way they assess the likelihood of negative situations occurring.

It should be re-emphasised at this point that an interpretive bias was defined at the outset as an overall interpretive tendency that is maladaptive (preventing optimal cognitive functioning, albeit in the more limited way of the nonclinical analogues who participated in these experiments), involves a preoccupation with negatively-valenced stimuli, and displays a consistent association with anxiety. It is important to maintain these criteria when considering the measurement component of this thesis. With them in mind, it is important to tentatively conclude in favour of the scrambled sentence task (despite its initial perceived limitations) overall being the better indicator of a bias in the defined sense, even though it did not show significant variation in response to training. This could be interpreted as a lack of sensitivity in the measure itself. However as considered below, it seems the more likely outcome that training simply did not produce significant change. Fundamentally, the SST exhibits a consistent association with anxiety (albeit with state as well as trait anxiety, as discussed in the prior chapter).

This is not to imply a discarding of the value of open response task in the current project however. The data collected on likelihood approximation across the experiments demonstrates some promise for this variable as a further index of bias. Furthermore the lack of an association for generation and evaluation is an important initial finding of value in itself, providing tentative evidence that these are faculties with independence from fear of negative evaluation and helping to rule them out (at least in the format implemented in this thesis) as an instrumental factor in bias interventions. The statements in favour of open response, multi-response and multi-bias index measurement of interpretive biases made at the beginning of this thesis still stand on theoretical grounds, and with a degree of empirical evidence: such measures appear to have been of use in detecting the overall likelihood approximation bias (which would have been obscured if closed, single-resolution measurement had been implemented) and also suggesting a lack of bias in generation and evaluation (thus highlighting the instrumentality of likelihood in anxiety) when multiple responses are considered (an important finding in itself). They may not be an ideal measure of interpretive bias outcomes for CBM-I however. They have the potential to be adapted to other self-report bias indices like covariation or memory bias (e.g. having participants recall multiple memories of salient social incidents). They do however need to be refined, standardised beyond the proof of concept-oriented implementation in this thesis and Dodd, Stuijfzand, Morris and Hudson's (2015) work and comprehensively replicated and validated. At the level of the field they then need to be applied in a standardised manner across multiple trials to achieve the homogeneity emphasised as currently lacking by Cristea, Kok and Cuijpers (2015). Furthermore, it may be useful for similarly standardised measures of automatic, system one-rooted bias to be applied alongside them as this task, whilst still arguably providing a more ecologically valid experimental approximation of a real life ambiguous social situation, does appear to capture primarily late-stage processing tendencies.

Potential for improvement to CBM-I designs. The two specific improvements trialled: closed-resolution explicitly-instructed training alone and explicitly instructed open response training, are dealt with separately here, despite their relatively comparative ability to exert change in bias, as each has different implications. In terms of the former, across the initial pair of experiments no evidence was found for superiority or comprehensive equivalence of explicitly-instructed closed resolution training, with implicit, closed-response training being slightly superior for within-group bias change in the first experiment only and both groups being broadly equivalent overall.

These findings thus contrast somewhat with the results gained by Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt (2013), but require replication with different CBM-I experimental designs (the particular training administered does not appear to have been sufficient) for clarification on whether training in which participants are explicitly aware of their task is an equally valid way of implementing bias change. It was hoped that it would be achieved within the first and second experiments. The lack of effect of training however thus elicits experimental redesign, perhaps with repeated training sessions or a different training format, and potentially even different outcome measures. This is of considerable importance and requires more than just Mobini, Mackintosh, Illingworth, Gega, Langdon & Hoppitt's (2013) study for substantiation as it could conceivably limit (or even enhance, if these results represent a more moderate portrayal of explicit CBM-I's effect) the application of CBM-I considerably. Explicit instruction is of course somewhat distinct from knowing the full aim of a CBM-I program (indeed in the current series of experiments, full awareness of the design was checked for in the debriefing of participants, resulting in post-participation exclusions). Nonetheless explicit instruction to interpret positively can be considered a potentially influential "step" in this direction and in reducing the active engagement of participants (without invalidating an experimental design due to demand characteristics), and full awareness may place even more severe limitations on effect: such techniques, if successful, would gain a reputation and information about them would conceivably spread amongst service users, who would fail to see benefit themselves if approaching the techniques from an informed position, replacing genuine implicit engagement with demand characteristics.

For open response CBM-I training meanwhile, there was similarly no conclusive evidence (regardless of whether the training was implemented to train a positive or negative bias, or with socially anxious individuals or those from the general population) for the superiority of such a method of modifying interpretation over traditional closed-resolution methods. As potential limitations to the implementation of training per se in this thesis have been raised and discussed, it would be important to trial open-ended training with other CBM-I formats before reaching a conclusive position on this method of administering training overall. However, the administration of this variant and subsequent production of highly limited results across two experiments in this thesis is not as informative a start on this as originally envisaged.

At a more general level of interpretive bias training per se, taking the most evidence-concordant explanation for the overall findings of the thesis, and bearing in mind that such a bias as that measured by the SST does not need by definition to be consistently modifiable by the techniques applied (and that the task has a strong association with FNE score over a large sample), brings a further tentative conclusion. This is that the modification implemented in this thesis failed to work. Potential reasons for these overall outcomes are varied.

The general training format implemented may have not been sufficient. Participants received one hundred scenarios in a single session in the first two experiments, and two sets of seventy scenarios (one program of negative content, followed by one program of positive content for active condition participants) in a single session in the latter studies. Whilst many CBM-I studies have used such numbers of scenarios in single-session training, it may have been necessary to include more to provide a statistically significant effect. Resource constraints on repeated training sessions, and early self-report from certain participants of boredom and disengagement effects towards the end of the one hundred scenario training prevented larger numbers of scenarios in the planning of the current series of studies. In terms of the literature, repeated training session-based studies may produce more consistent modification but overall there is not a significant association between number of training sessions and demonstrated effect size (Cristea, Kok & Cuijpers, 2015).

The interface of the outcome measures and the training should receive brief mention here: many contemporary CBM-I studies use outcome measures that are extremely close to their training materials, including word-sentence association procedures which use word-sentence association as outcome measurement (Beard & Amir, 2009) and studies in which differently-valenced versions of training scenarios are endorsed by participants for their similarity to content encountered in training (e.g. Clerkin et al., 2011). These thus demonstrate near transfer only. The interpretive bias tasks implemented in the current series of studies were procedurally more distal and the scrambled sentence task, as considered in the prior chapter, may in fact draw more upon attentional processes than full interpretation. The open response task meanwhile may be too distal and “system two” rooted to display such a bias, as prior studies have tended to not only apply measurement tasks with high procedural similarity to their training materials, but also to make these tasks arguably highly system one-rooted, with little necessary engagement of executive processes or working memory (for example stating whether words and sentences are related or not).

Furthermore the outcomes are not entirely unreflective of published contemporary literature. During the undertaking of this thesis, meta-analytic evidence (Cristea, Kok & Cuijpers, 2015) emerged suggesting considerably more limited effects than those found and assumed by the majority of studies in the field. Whilst the review used strict inclusion criteria, focused on clinical outcomes for the bias techniques implemented and not on bias outcomes, and in places conflated quite distinct manipulation techniques, it nonetheless provides a thorough and critical overview of the field. The key implications were that prior meta-analyses exhibited distorted estimations of effect owing primarily to a very small set of extreme outlier studies (studies in which effect sizes were over ten times the remainder of their contemporaries), and that considerable publication bias was present in the literature, with social anxiety-focused CBM studies being no exception. As such CBM-I as a whole may have considerably more limited operational capacity than has previously been estimated. This failure to achieve significant results from modification in the current series of studies also align with the authors’ recent, more conservative approximations of CBM-I’s effect size (which are in the “small” range for most clinical symptomology, especially when missing studies are imputed). There are implications for elements such as sample size here (as well as further support for exploring the techniques' interface with personality variables in order to limit noise variance as mentioned previously). The majority of CBM-I studies are conducted with samples of between twenty and thirty participants per group and have generally produced significant results. However imputed missing studies by Cristea, Kok and Cuijpers (2015) suggest that this is not consistently the case and that larger samples (plus greater control over a variety of elements of experimental design) may be necessary to detect such modest effects. These should ideally be applied with better-substantiated and homogenous inter-study measures. Whilst diverse and innovative "proof of concept studies" remain useful in areas, the standardisation of outcome measures and procedures considered earlier in this conclusion becomes of particular importance, due to the necessity of appropriately powerful meta-analysis at a field level.

Summary and general conclusion

The current thesis overall provides evidence for caution in the application of interpretive bias modification techniques with a set of primarily null results in experiments applying them, and a set of specific insights into the nature of interpretive bias measurement. It provides some supportive evidence for the validity of the scrambled sentence task as a measure of social anxiety-associated overall bias, although not necessarily as a training outcome indicator. It evidences a social anxiety-associated likelihood approximation bias. The results also suggest a lack of association between individuals' ability to generate social judgment-related negative material and evaluate it and their level of social anxiety.

In accordance with the above, the thesis must conclude in favour of greater investigation of the methods of measuring biased interpretation, including standardisation, validity and (where possible) reliability confirmation for candidate measures and tasks. This should be accompanied by detailed cognitive theorisation on mechanisms of operation of cognitive biases and mechanisms of effect of attempts to modify them. It is also necessary to conclude in favour of an accompanying caution with method innovation in CBM-I procedures (and particularly the clinical application thereof) until this is gained. Furthermore, a strong critical emphasis on trial design should be upheld and extended, and CBM-I tasks should receive multiple, replicated trials with standardised training and outcome measurement before their status as a valid clinical intervention candidate can be accurately concluded upon.

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Appendices

Appendix One: Information and debrief sheets issued to participants in experiments one and two

Participant Information Sheet

University of East Anglia

Thank you for your interest in this study. Before you decide whether to take part, please read the following information carefully (this sheet is for you to keep). You may ask me any questions if you would like more information.

What is this research looking at?

I am a psychology student studying for the degree of Doctor of Philosophy, with a research interest in the ways in which people comprehend situations and resolve ambiguity.

What will happen if I agree to take part?

We are looking to investigate the responses of participants within a specific range of scores on an initial questionnaire which we will email to you. This questionnaire will ask you how you feel about different social situations. Should you fall within this range you will then be asked to attend a short experimental session in a psychology research laboratory at the University of East Anglia. If your score does fall within the range we are interested in and you attend a session, you will be asked to complete a computer-based programme in which you are asked to read and imagine yourself in descriptions of social scenarios. You will also be asked take part in a task in which the objective is to create sentences from sets of words, and to complete short questionnaires asking you about how your mood and feelings. You will be informed fully about what each of the tasks is about and why it was necessary, with an opportunity to ask questions, after you have completed them. The entire procedure is expected to take around one hour.

Are there any risks involved?

There are no major risks involved in this experiment. However, answering questions about your mood and imagining yourself in different social situations might create a degree of anxiety for some people. As such, it is important that you are aware throughout that this is a voluntary study and you are free to leave the experiment at any point without any repercussions.

How will you store the information that I give you?

Any information given will be stored electronically on a password protected computer, and physically in a locked filing cabinet. Data will be untraceable back to the specific participant who submitted it, and accessible only the researcher, the research supervisor, and possibly members of a small research team with similar training and objectives.

How will the data be used?

The data will be used for statistical analysis that will form a component of the researcher’s studies towards the degree of Doctor of Philosophy in the School of Psychology. Should particularly important or relevant information be found, the data may be presented in part or its entirety in one or more academic journals and/or at conferences. In such a situation, the anonymity of participants detailed above will be sustained, with no individual data being identified to any individual person.

What happens if I agree to take part, but change my mind later?

Returning the initial questionnaire will be interpreted as consent for the initial phase of the study. You will receive a payment of £2 for this part of the study regardless of whether you decide to carry on with the study. If you attend the main experimental session, read this information sheet, sign the consent form, and begin participation, you will still be free to withdraw at any time within the duration of the experimental procedure, and will be reminded of this right at the halfway point of the procedure. Once all the data has been gathered, it will no longer be possible for individual participants to withdraw their specific data due to the anonymity measures being taken in this research to protect the identity of yourself and the other participants. At the end of the session, you will be given a debrief sheet and this will be your final opportunity to withdraw from the study. You will receive £5 for your participation in this part of the study.

How do I know that this research is safe for me to take part in?

This research is based on prior methodology that has been both successful and safe for its participants. Furthermore, it has received scrutiny from the School of Psychology ethics committee, and found to be acceptable to run on members of the public.

Contact details:

Research supervisor: Dr. Charles Seger Researcher: Benjamin Marshall

Email: C.Seger@uea.ac.uk Email:Benjamin.Marshall@uea.ac.uk

Head of Research Ethics Committee : Dr Piers Fleming

Email: P.Fleming@uea.ac.uk

Head of School of Psychology: Professor Kenny Coventry

Email: Kenny.Coventry@uea.ac.uk

Participant Debrief Sheet

Thank you for taking part in this experiment. Your participation is now finished. Beyond this point you will no longer be able to withdraw data from the study, in order to preserve the anonymity of all participants, so please let me know at this stage if you wish to withdraw your data. In the experiment, I was interested in comparing three groups of participants, each of which completed a different computerised training program. In each program the people taking part received a set of scenarios, such as the below:

An opportunity arises for a summer research studentship in your university department. You've heard there will be lots of competition, but ask for more details about it. When you hear what would be expected of applicants, you think that if you applied for the studentship you would be

we--o—d

In one group the scenario has an incomplete end word, and this fragment can only be made into a full word by adding a letter that would make the meaning of the word and scenario positive. In this way, participants in this group became used to resolving situations in a positive manner. A further group did the same but received instructions to only resolve the scenarios positively. In another group, participants received open-ended scenarios and were asked to generate images themselves that would end the scenario, and in a final fourth group participants did the same but were asked to only generate images with endings that brought a positive outcome to the situation described.

Before and after these scenarios you took part in a task where you had to unscramble lists of words into sentences. In fact you might have noticed that unscrambling these words could lead to sentences that had either positive or negative meanings. I was interested to see whether or not the two groups of participants who were asked to generate positive endings in the computer task (either by themselves or helped by the word fragments) would start to spontaneously unscramble the words to make more positive sentences than the group that was given no direction in terms of generating positive meanings. I also asked you to fill out the questionnaires about your mood to see how you were feeling when you began and finished the experimental session.

If you as a participant have any questions about the experiment, contact details for two members of the research team are available on your information sheet. Note that, because the data is de-identified once you have completed the study, we are not able to provide any individual feedback regarding scores on questionnaires or other measures.

Should you happen to be concerned about your own anxiety or that of others, the university offers a number of services, several of which are detailed below. However, if you are concerned we recommend that your first point of contact should be your G.P.

University of East Anglia Mental Health Services:

University counselling service

Tel: 01603 592651
Email: csr@uea.ac.uk
Website: https://www.uea.ac.uk/counselling

University medical service

Tel: 01603 251600 (NB. This is not a UEA telephone number and needs to be dialled in full.)
NHS Direct Tel: 0845 4647 (24 hours)
Email: umsuea@nhs.net
Website: www.umsuea.co.uk

Experiment Contact details:

Research supervisor: Dr. Judi Walsh

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Researcher: Benjamin Marshall

Email: Benjamin.Marshall@uea.ac.uk

Head of Research Ethics Committee : Dr Piers Fleming

Email: P.Fleming@uea.ac.uk

Head of School of Psychology: Professor Kenny Coventry

Email: Kenny.Coventry@uea.ac.uk

Appendix two: Information and debrief sheets issued to participants in experiments three, four and five

School of Psychology

University of East Anglia

'Social judgements and multitasking'

Participant Information Sheet

Thank you for your interest in this study. Before you decide whether to take part, please read the following information carefully (this sheet is for you to keep). You may ask me any questions if you would like more information.

What is this research looking at?

This research looks at how people make sense of social information and how they are able to multitask whilst doing so.

Do I have to take part?

It is up to you to decide to join the study. We will describe the study and go through this information sheet. If you agree to take part, we will then ask you to sign a consent form. You are free to withdraw at any time, without giving a reason. This would not affect you in any way.

What will happen if I agree to take part?

You will be asked to complete some short questionnaires about your mood and a task that asks you to unscramble sets of words into sentences at several points in the following session. You will also complete a computer program in two stages that asks you to finish the descriptions of scenarios displayed on screen. In addition, *some participants will be asked to place their pen in certain positions on their face at key points in the experimental procedure.*

Are there any problems with taking part?

You should not encounter any significant problems, physical, emotional or otherwise, in the course of taking part in this experiment. All participants are different however, and if you feel unpleasant during the procedure for any reason, or simply wish to leave, please inform the experimenter immediately, who will be able to stop the procedure if necessary.

It is important to note at this point that by taking part you are confirming that you are not currently receiving treatment for a mental health condition. This is very important as, although the procedure is very safe for use on a general population, we do not want to cause undue discomfort to anyone suffering from such a condition.

Will it help me if I take part?

The only direct immediate benefit you are likely to receive from this work is the monetary/credit compensation for taking part (your experimenter will explain how you will receive this compensation). However you will be contributing to an original piece of academic work that will be furthering knowledge in an important field with a view to improving certain people's lives. If you are interested in the research, its aims and outcomes, you can chat to the experimenter after completing your participation (when they will be able to give you the full picture of what the study was trying to achieve), and you can request the results of the study when these emerge.

How will you store the information that I give you?

All information which you provide during the study will be stored in accordance with the 1998 Data Protection Act and kept strictly confidential. The chief investigator will be the custodian of the anonymous research data. All data will be kept for 5 years, and will then be securely disposed of. Electronic data will be stored on a password protected computer or external hard drive. Paper data will be stored in a locked storage area in a locked office. All data will be anonymous as soon as you submit and will not feature your name, being referred to only by numbers. Only a small research team will have access to the data.

How will the data be used?

The anonymised data will be analysed to form part of the experimenter's PhD project. Parts of this project may be submitted to academic journals for publication, or presented at relevant conferences.

What happens if I agree to take part, but change my mind later?

You are free to leave the experiment at any time. Please let the experimenter know and they will end the experiment and give you a short debrief. Should you wish to remove your data from the experiment after it has been collected please promptly contact the experimenter via the following address:

Benjamin.marshall@uea.ac.uk

How do I know that this research is safe for me to take part in?

All research in the University is looked at by an independent group of people, called a Research Ethics Committee, to protect your safety, rights, wellbeing and dignity. This research was approved by the Psychology Research Ethics Committee at the University of East Anglia.

You are under no obligation to agree to take part in this research.

If you do agree you can **withdraw at any time without giving a reason.**

Contact details:

Experimenter: Benjamin Marshall (Benjamin.marshall@uea.ac.uk)

Research supervisor: Dr. Charles Seger (C.Seger@uea.ac.uk)

Do also contact us if you have any worries or concerns about this research.

School of Psychology Ethics Committee:

ethics.psychology@uea.ac.uk; Phone 01603 597146

Head of School Professor Kenny Coventry:

k.coventry@uea.ac.uk; Phone 01603 597145

Participant Debrief Sheet

Thank you for taking part in this experiment. Your participation is now finished. In the experiment, I was interested in comparing four groups of participants, three of these groups completed a computerised training program designed to change the way that they interpreted social information, and one group completed a comparison program that was not designed to train them. In each program the people taking part received a set of scenarios, such as the below:

An opportunity arises for a summer research studentship in your university department. You've heard there will be lots of competition, but ask for more details about it. When you hear what would be expected of applicants, you think that if you applied for the studentship you would be

we--o—d

In three groups the scenario has an incomplete end word, and this fragment can only be made into a full word by adding a letter that would make the meaning of the word and scenario positive ("welcomed" in this case). In this way, participants in these groups became used to resolving situations in a positive manner. Two of the groups of participants (and the one group of participants who did not take part in training) were given a pen and asked to hold it between their lips or teeth – these have been shown to induce a negative or positive mood (respectively). We were interested in whether this task would interfere with, impair or enhance the effect of the training compared to the group who just did the training and the group.

Before and after these scenarios you took part in a task where you had to unscramble lists of words into sentences. In fact you might have noticed that unscrambling these words could lead to sentences that had either positive or negative meanings. We were interested to see whether or not the groups of participants who were encouraged to generate positive endings in the computer task would start to spontaneously unscramble the words to make more positive sentences than the comparison group, and how the pen task would affect this. You were also asked to fill out questionnaires about your mood to see how you were feeling when you began and finished the experimental session.

If you as a participant have any questions about the experiment, contact details for two members of the research team are available on your information sheet. Note that, because the data is de-identified once you have completed the study, we are not able to provide any individual feedback regarding scores on questionnaires or other measures.

Should you happen to be concerned about your own anxiety or that of others, the university offers a number of services, several of which are detailed below. However, if you are concerned we recommend that your first point of contact should be your G.P.

University of East Anglia Mental Health Services:

University counselling service

Tel: 01603 592651
Email: csr@uea.ac.uk
Website: https://www.uea.ac.uk/counselling

University medical service

Tel: 01603 251600 (NB. This is not a UEA telephone number and needs to be dialled in full.)
NHS Direct Tel: 0845 4647 (24 hours)
Email: umsuea@nhs.net
Website: www.umsuea.co.uk

Experiment Contact details:

Experimenter: Benjamin Marshall (Benjamin.marshall@uea.ac.uk)

Research supervisor: Dr. Charles Seger (C.Seger@uea.ac.uk)

Do also contact us if you have any worries or concerns about this research.

School of Psychology Ethics Committee:

ethics.psychology@uea.ac.uk; Phone 01603 597146

Head of School Professor Kenny Coventry:

k.coventry@uea.ac.uk; Phone 01603 597145

Appendix Three: Scrambled Sentence Task Content

SCRAMBLED SENTENCE TEST

Unscramble the sentences to form statements. Each of the scrambled sentences contains six words. Unscramble five words in each sentence by placing a number over each of the five words indicating the proper order.

For example:

3 4 2 1 5

has green child the eyes blue

Each sentence can be unscrambled into more than one statement, but you should choose only <u>one</u> statement to unscramble. You have 4 minutes to unscramble as many sentences as possible. Work as quickly and as accurately as possible.

apprehensive people with relaxed new I'm

meetings tense relaxed I at feel

can errors cannot social make I

others foolish I to sensible appear

uneasy other with easy I'm people

won't fool will a I look

think worthwhile others worthless I am

I'm people stressed relaxed other with

approaching fine scary people is new

superiors my I please will displease

occasions avoid I social enjoy usually

opinions do me bother others' don't

people usually from withdraw don't I

gatherings dread love usually I social

sizing is up me noone someone

attend I social avoid usually functions

me good of bad think others

things say wrong right I the

avoid wish people to I meet

occasionally strangers never I with talk

Write the 6 digit number in the box below

SCRAMBLED SENTENCE TEST

Unscramble the sentences to form statements. Each of the scrambled sentences contains six words. Unscramble five words in each sentence by placing a number over each of the five words indicating the proper order.

For example:

3 4 2 1 5

has green child the eyes blue

Each sentence can be unscrambled into more than one statement, but you should choose only <u>one</u> statement to unscramble. You have 4 minutes to unscramble as many sentences as possible. Work as quickly and as accurately as possible.

indifferent I'm others' opinions worried about

attributes my will people shortcomings notice

careful with am carefree others I

talking people to dislike like I

faults notice other merits my people

are other judging watching people me

occasions I enjoyable find social upsetting

meeting dislike enjoy new I people

uncomfortable company comfortable in I'm others'

people I'm new with confident nervous

strangers relaxed feel with tense I

engagements usually social endure I enjoy

with relaxed tense my I'm superiors

me fault won't people other will

comfortable circles in anxious social I'm

please must may I people other

me others will won't disapprove of

make bad I impression good a

me people approve other of disapprove

relaxed meeting people me tense makes

SCRAMBLED SENTENCE TEST

Unscramble the sentences to form statements. Each of the scrambled sentences contains six words. Unscramble five words in each sentence by placing a number over each of the five words indicating the proper order.

For example:

3 4 2 1 5

has green child the eyes blue

Each sentence can be unscrambled into more than one statement, but you should choose only <u>one</u> statement to unscramble. You have 4 minutes to unscramble as many sentences as possible. Work as quickly and as accurately as possible.

others is hard speaking easy with

going love I parties to hate

never people always me other like

making I'm good conversation at terrible

up phone never friends always me

make I rarely mistakes frequently foolish

presentation a giving stressful is exciting

work my pleases others displeases usually

new phoning daunting people fine is

stupid say rarely something often I

frequently others quiet around I'm occasionally

enjoyable functions is to intimidating going

people me other seldom like usually

acquaintances enjoyable is eating worrying with

me will other dislike won't people

things correct incorrect say I the

how considered is I'm important isn't

avoid people rarely I other often

scary new people meeting fun is

me other people worthwhile think worthless

Write the 6 digit number in the box below

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people other pleasant think unpleasant I'm making at bad I'm friends good

liked am I peers disliked by

apprehensive feel I calm others around

groups speaking stimulating to terrifying is

starting intimidating conversation a enjoyable is

mix I well groups poorly with

flaws people always qualities notice my

people me first invite last out

of me do opinions don’t matter

invitation party a excitement provokes dread

me well nobody thinks everybody of

I'm with comfortable other uncomfortable people

events hate attending love I socia

favourably others usually unfavourably me evaluate

making errors acceptable social is unacceptable

Appendix Four: Open response measure

In this task, you will read six scenarios. Each scenario is unfinished and ends with a question, and underneath it are ten spaces, each with two boxes beside. Your task is to:

1. Generate as many answers to the question as you can in the spaces.
2. In box 1, provide a rating (from 1 to 7) of how likely this answer is to happen, given only the information in the scenario description: 1 = highly unlikely, 7 = very likely.
3. In box 2, provide a rating (from 1 to 7) of how positive you would feel if this answer did happen: 1 = very negative, 7 = very positive.

Scenario 1: The new job

It's your second week on the job. Your boss stops by your desk in the early afternoon and asks you to come to his office later that day. Why does your boss want to see you?

Answer	**Likelihood**	**Positivity**

Scenario 2: The call from a friend

A friend calls and leaves you a voicemail saying “Give me a call, I need to speak to you. It’s important” What does he/she want to talk to you about?

Answer	**Likelihood**	**Positivity**

Scenario 3: The party

You walk into a party and people turn to look at you, why?

Answer	**Likelihood**	**Positivity**

Scenario 4: The street

You're walking down the street, and you see one of your friends coming the other way with a group of people. You wave, but your friend doesn't respond. Why?

Answer	**Likelihood**	**Positivity**

Scenario 5: The shopping centre

You see two of your closest friends at the shopping centre together. They didn't tell you they were going. Why?

Answer	**Likelihood**	**Positivity**

Scenario 6: The news

A member of your family says that they have some important news that they need to discuss with you. What could it be?

Answer	**Likelihood**	**Positivity**

Part two

In this task, you will read six further scenarios. Each scenario is unfinished and ends with a question, and underneath it are ten spaces, each with two boxes beside. Your task is to:

4. Generate as many answers to the question as you can in the spaces.
5. In box 1, provide a rating (from 1 to 7) of how likely this answer is to happen, given only the information in the scenario description: 1 = highly unlikely, 7 = very likely.
6. In box 2, provide a rating (from 1 to 7) of how positive you would feel if this answer did happen: 1 = very negative, 7 = very positive.

Scenario 1: The conversation

You are talking to someone you have just met and they appear to be distracted. Why might this be?

Answer	**Likelihood**	**Positivity**

Scenario 2: The meal

You are eating in a restaurant and notice people on the table next to you looking over a lot. Why might this be?

Answer	**Likelihood**	**Positivity**

Scenario 3: The friend

You notice that a close friend has been behaving unusually. Why might this be?

Answer	**Likelihood**	**Positivity**

Scenario 4: The workplace

You hear that someone has been asking after you at work. What might they want?

Answer	**Likelihood**	**Positivity**

Scenario 5: The presentation

You've just delivered an important presentation and afterwards no one in your audience asks a question. Why did you not receive any questions?

Answer	**Likelihood**	**Positivity**

Scenario 6: The social network

You post what you think is an important status on your social networking profile, and it attracts no comments from your friends. Why might this be?

Answer	**Likelihood**	**Positivity**