INVESTIGATING THE CAUSAL CONTRIBUTION OF INTERPRETIVE BIAS TO ANXIETY VULNERABILITY

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ABSTRACT

It has frequently been reported that individuals with elevated anxiety vulnerability impose threat-congruent interpretations upon emotionally ambiguous stimuli. A common hypothesis is that such threat-congruent interpretations contribute causally to the intensity and frequency of the anxiety elevations experienced by vulnerable individuals. However, no direct evidence has been provided to support this hypothesis. Empirically evaluating this theoretical position was the goal of the series of empirical studies described in this thesis. The approach employed here involved first, systematically and specifically manipulating interpretive bias, and second, assessing the consequences of such manipulations for anxiety vulnerability as assessed by individual differences in the intensity of emotional reaction to a subsequent stressor. This research was conducted in two phases. The studies in Phase 1 were designed to permit the development of training tasks, capable of inducing group differences in interpretive bias. The employed approach to such interpretive training involved the modification of priming tasks previously used to assess interpretive bias. In each trial of such priming tasks, homograph primes with both threatening and non-threatening meanings are first presented, followed by targets which, on different trials, are related to their threatening or to their non-threatening meanings. Participants are required to respond to identify each target, using the prime as a cue. In order to create interpretive training tasks capable of manipulating interpretive bias, contingencies were introduced into such priming task methodologies, such that the targets were related to differentially valenced prime meanings for different groups of participants. For the threat training group, the targets presented during training were always related to the threatening meanings of the

homograph primes, making it advantageous for these participants to interpret the primes in a threat-congruent fashion, with the intention of inducing a threat-congruent interpretive bias. For the non-threat training group, the targets in training were always related to the non-threatening meanings of the ambiguous primes, making it advantageous to interpret the primes in a non-threat-congruent fashion, with the intention of thus encouraging a non-threat-congruent interpretive bias. The success of these training procedures in modifying interpretive bias was then assessed in subsequent, non-contingent versions of these priming procedures. It was important to ensure that the training procedures indeed induced group differences in interpretive bias, rather than simply modifying the capacity to respond to valenced targets. Therefore, control conditions were included in these interpretive assessments in which homograph primes were not presented, thus removing the requirement for differential interpretation. In the final study in Phase 1, an interpretive training version of this priming task was indeed demonstrated to be capable of influencing interpretive bias, with alternative, target specific explanations not supported. Specifically, relative to individuals encouraged to access the threatening meanings of the homograph primes during training, those encouraged to access the non-threatening meanings subsequently displayed speeding to identify targets related to the non-threatening, relative to the threatening meanings of novel primes presented in assessment. Further, the groups did not show this difference when the primes were unambiguous, thus suggesting that the differential interpretations imposed by the groups upon the primes was crucial to the observed group difference in performance. Having confirmed that the training approach developed in Phase 1 was indeed capable of systematically manipulating interpretive bias, the studies in Phase 2 were then designed to assess the consequences of such

manipulations for anxiety vulnerability, assessed in terms of subsequent anxious reaction to a stressor. In four different studies within Phase 2, two variants of the priming methodology developed in Phase 1 were used to induce group differences in interpretive bias, and two different non-contingent versions of this methodology were used to assess the influence of this training upon interpretation. Following this training and subsequent interpretive assessment, anxiety vulnerability was then assessed in terms of anxious reaction to a series of video clips depicting real life emergency situations. Individuals induced to interpret ambiguity in a non-threat congruent fashion, relative to those induced to interpret ambiguity in a threat-congruent fashion, showed an ameliorated tendency to respond to the stressor with elevation in state anxiety. The findings of this second series of studies thus provide strong support for the hypothesis that interpretive bias can make a causal contribution to anxiety vulnerability.

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CHAPTER 1

INTRODUCTION

In this Chapter, anxiety vulnerability is first defined, with reference to the distinction between state and trait anxiety. Next, cognitive models of anxiety are briefly reviewed, with a focus on the large class of models which make predictions about a systematic relation between anxiety vulnerability and the interpretation of ambiguity. The experimental evidence for the hypothesis that anxious individuals are characterised by a threat-congruent bias in their interpretation of ambiguity is then considered. Next, three sources of evidence potentially relevant to the question as to the causal role of interpretive bias in anxiety vulnerability are reviewed, and the limitations of each approach considered. A small number of studies which have been designed to more directly assess the causal status of interpretive bias in anxiety vulnerability are then examined in detail, with consideration of the key limitations of each. The program of empirical research reported in this thesis, which sought to overcome these limitations, is then outlined.

The Concept Of Anxiety Vulnerability

In conceptualising anxiety vulnerability, a distinction is commonly made between state anxiety and trait anxiety (Cattell & Scheier, 1961; Endler, 1983, 1997; Endler, Edwards, Vitelli, & Parker, 1989; Spielberger, 1966, 1972, 1985; Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983). State anxiety reflects the intensity of anxious symptoms experienced at a particular instant in time. Anxious symptoms in this context may be typified by the subjective experience of tension, apprehension or worry,

and by physiological arousal. In contrast, trait anxiety reflects the degree to which an individual tends to experience elevations in state anxiety in response to a given stressor. Typically, trait anxiety is measured on scales which ask participants to rate the frequency with which they experience the symptoms of state anxiety (e.g., Spielberger et al., 1983), with the assumption that such assessments reveal vulnerability to elevation in such symptoms.

Trait anxiety has been suggested as a core dimension of personality by various theorists, often on the basis of quite different bodies of empirical research (e.g., Cattell & Scheier, 1961; Gray, 1982; Jorm, 1989). It is closely related to H. J. Eysenck's (e.g., 1967) notion of neuroticism; indeed, many theorists have stressed the similarities of these two concepts (e.g., M. W. Eysenck, 1992b; Watson & Clark, 1984; L. A. Clark, Watson, & Mineka, 1994). For example, M. W. Eysenck (1992b) suggests that each may reflect a similar underlying proneness to anxiety symptoms. There is then, convergent validity for the trait anxiety construct.

Further validation comes from familial aggregation studies of trait anxiety, which suggest a moderate degree of genetic transmission of this characteristic. For example, Tambs' (1991) findings suggested an upper limit of 43% heritability for the tendency to experience anxiety symptoms. Twin studies, and combined twin/family studies of heritability in adults and adolescents have resulted in similar conclusions (e.g., Jardine, Martin, & Henderson, 1984; Legrand, McGue, & Iacono, 1999; Topolski et al., 1997). For example, Legrand et al. estimated the heritability of trait anxiety endowment at 45%. In sum, these genetic studies accede to the validity of an underlying anxiety vulnerability construct, which is characterised by a moderate degree of additive genetic heritability.

Clinical anxiety states, such as those defined by the Diagnostic and Statistical Manual of Mental Disorders (DSM-IV) (APA 1994), may represent individuals on the extreme of the trait anxiety continuum, not withstanding the idiosyncrasies of each individual syndrome. Consistent with this, elevated levels of trait anxiety have often been reported to characterise individuals diagnosed with various anxiety disorders, such as panic disorder (Borden & Turner, 1989), generalised anxiety disorder (GAD) (Rapee, 1991), post-traumatic stress disorder (PTSD) (Shalev, Freedman, Peri, Brandes, & Sahar, 1997; Tampke & Irwin, 1999) and obsessive-compulsive disorder (OCD) (Scarrabelotti, Duck, & Dickerson, 1995).

Cognitive Models Of Anxiety Vulnerability

What may account for variations in trait anxiety? In recent decades, a number of theorists have provided models of anxiety vulnerability that implicate particular biases in cognitive functioning. Different models have set out to explain variations in vulnerability in different populations. Some models have concentrated upon explaining the maintenance of specific anxiety disorders (Clark, 1986; Clark & Wells, 1995; Rapee & Heimberg, 1997). Others have been developed in order to explain clinical anxiety in general (e.g., Beck & Clark, 1988, 1997; Beck, Emery, & Greenberg, 1985). Still other models have endeavoured to provide a broader theoretical framework which encompasses non-clinical manifestation of heightened anxiety vulnerability, together with clinical phenomena (e.g., Eysenck, 1992a, 1997; Mathews & Mackintosh, 1998; Power & Dalgleish, 1997; Williams, Watts, MacLeod, & Mathews, 1988, 1997). Finally, it is interesting to note that some theorists investigating the developmental trajectory of clinical anxiety across childhood have also turned to explanations in terms

of biased cognitive processes (e.g., Daleiden & Vasey, 1997). Despite such variation in focus, one proposal common to many such models, and indeed, to each of the models listed here, is the notion that the biased interpretation of ambiguity may causally mediate an individual's anxiety vulnerability. In short, these models suggest that more vulnerable individuals may be more likely to interpret ambiguity in a threat-congruent fashion, and that such interpretations may causally mediate vulnerability. Some examples of such models will now be considered in more detail, to illustrate the types of mechanisms proposed to underlie this hypothesised threat-congruent interpretive bias.

Perhaps the best known cognitive account of clinical anxiety comes from the writings of Beck and colleagues (e.g., Beck & Clark, 1988, 1997; Beck et al., 1985). From Beck's perspective, anxiety vulnerable individuals are endowed with what he terms "danger schemata" – overarching informational structures which bias cognition towards the processing of threatening information. Such schemata are supposedly formed through early experience but remain latent until later activated, perhaps by a stressful situation. The activation of such schemata then powerfully and pervasively biases further processing in a threat-congruent fashion, exacerbating the anxiety response to stress. Schemata are suggested to bias a broad range of processes, including attentional allocation, encoding, and the interpretation of ambiguity. Thus, when danger schemata are activated, they will operate to increase the degree to which ambiguous stimuli are interpreted in a threat-congruent fashion.

Various cognitive formulations of specific anxiety disorders have similarly implicated biased interpretations in the causes of anxious symptoms. For example, in D. M. Clark's (1986) influential model of panic disorder, biased interpretation is proposed to be the central cognitive mediator of panic. Clark suggests that panic attacks

result from the misconstrual of innocuous but ambiguous external and internal stimuli as threatening. Subsequent anxious arousal arising from such initially biased interpretations may itself then be misinterpreted as threatening, perhaps as a sign of an impending physical emergency (such as a heart attack), or of mental illness. Such catastrophic misinterpretations, it is suggested, lead to further increases in anxiety which lead in turn to further arousal and further misinterpretation, with this vicious cycle culminating in a panic attack. D. M. Clark & Wells (1995) propose a similar though somewhat elaborated model for social phobia, in which an interpretive bias favouring negative resolutions of ambiguity is also suggested to play a causal role. Similarly, Rapee & Heimberg (1997)'s model of social phobia also implicates biased interpretations as playing a causal role in this disorder.

A number of theorists have sought to provide a unified cognitive explanation of anxiety vulnerability across normal and clinical domains (e.g., M. W. Eysenck, 1992, 1997; Mathews & Mackintosh, 1998; Power & Dalgleish, 1997; Williams et al., 1988, 1997). Frequently, a point of origin for these theorists has been the observation that anxiety vulnerable individuals display biases on tasks assessing selective aspects of attention and interpretation (e.g., Williams et al., 1988). Although some models tend to concentrate on the mechanisms underlying attentional processes (e.g., M. W. Eysenck, 1992; Williams et al., 1988, 1997), virtually all maintain that a threat-congruent interpretive bias plays a role in determining anxiety vulnerability.

One elegant proposal to this effect has been put forward by Mathews and Mackintosh (1998). These theorists employ aspects of Williams et al.'s (1988, 1997) model of attentional bias, extending them to provide a parsimonious explanation of interpretive bias also. Like Williams et al., Mathews and Mackintosh suggest that

anxiety vulnerability might be characterised by bias in the early allocation of cognitive resources, when alternative processing options compete for these resources. A threat evaluation system, which contains stored representations of stimuli signalling danger, is suggested to enhance the activation of inputs which find matches in the system. This input activation is presumed to occur, however, only when these stimuli exceed a minimal threshold of threat intensity. Elevation in state anxiety is suggested to lower the threshold of activation for the system.

Variations in anxiety vulnerability are suggested to be reflected within the threat evaluation system in at least one of two ways. Firstly, by a sustained lowering of the threshold required for the activation of threat input. Secondly, by a greater breadth of threat representations contained in the threat evaluation system. Evidence suggests that competing representations of ambiguous stimuli, reflecting alternative possible interpretations of these stimuli, are often initially activated, but that only one representation remains activated to the point where it reaches consciousness (e.g., Gernsbacher, Varner, & Faust, 1990). For anxiety vulnerable individuals, the threat-congruent interpretations of such stimuli will be more likely to receive activation from the threat evaluation system. Thus, a threat-congruent interpretation will be more likely to obtain processing resources, and so become the meaning that is consciously apprehended. This threat-congruent interpretation will produce an elevation in state anxiety, and this elevation in state anxiety may in turn exacerbate the degree of interpretive bias, by further reducing the output threshold of the threat evaluation system. Thus, as in other models, this will lead to a cycle of threat-congruent interpretations increasing anxiety, and increasing anxiety leading to increases in threat-

congruent interpretations, with the tendency to experience this cycle being pronounced in high trait anxious individuals.

In summary, a number of cognitive models propose that a threat-congruent interpretive bias may causally mediate anxiety vulnerability. As a first step toward evaluating this proposal, it is important to consider the evidence that high levels of anxiety vulnerability are reliably associated with the tendency to impose threat-congruent interpretations on ambiguity. As will be seen, the evidence for such an association is compelling.

Evidence For The Association Between Interpretive Bias And Anxiety Vulnerability

A number of studies have been designed to directly test whether a threat-congruent interpretive bias is associated with anxiety vulnerability. These studies have differed substantially in terms of the manner in which the interpretation of ambiguity is assessed. These different methods of testing may suffer, to differing degrees, from four methodological problems:

1. *The limitations of introspective access*: Some methodologies assess participants' beliefs regarding the interpretations that they would make, rather than assessing those interpretations directly, and such beliefs may not necessarily be accurate.

2. *Experimental demand effects*: Some methodologies have been criticised for imposing strong demand effects, such that participants are likely to recognise that a

particular type of interpretation is expected of them, which may influence their performance.

3. *The capacity to distinguish response bias and interpretive bias*: Some methodologies may be detecting a negative response bias, rather than a threat-congruent interpretive bias, such that more vulnerable individuals tend to respond with more threatening response options, even though their actual interpretations may be unbiased.

4. *The capacity to distinguish interpretive from subsequent memorial processes*: Some studies have been criticised for employing tests which may be affected by biased memorial processes, rather than by biased interpretations.

In the review that follows, methodologies which are successively less susceptible to each of these criticisms are described.

Self Report Measures Of Interpretive Bias

Reporting anticipated interpretations of hypothetical scenarios.

One common approach to assessing interpretive bias has been to present participants with descriptions of hypothetical ambiguous situations, and to ask them to indicate the interpretation that they would likely impose if placed in such situations. In one of the first studies to use this methodology, Butler and Mathews (1983) presented clinically anxious patients and non-clinical controls with a series of ambiguous scenarios which permitted both threatening and non-threatening interpretations (e.g., “Suppose you wake with a start in the middle of the night thinking you heard a noise but all is quiet. What do you suppose woke you up?”). Participants first provided an unconstrained response to each scenario, and were then asked to select from a series

of possible alternative explanations. Some of the presented options were threatening and some were non-threatening. Anxious participants, relative to controls, more often reported that they would impose threat-congruent interpretations on the situations, and ranked a greater number of threatening explanations as more likely to occur to them. Thus, these findings are consistent with the hypothesis that these anxious individuals would be more likely than the controls to interpret such ambiguous information as threatening.

Using a similar methodology, McNally and Foa (1987) also found that patients suffering from panic disorder with agoraphobia were more likely than controls to report that they would impose threat-congruent interpretations on emotionally ambiguous scenarios, and more likely to select the threatening disambiguations as being those which they would probably draw under the suggested circumstances. Using similar tests, comparable findings were obtained for panic disordered individuals, relative to non-anxious control participants (D. M. Clark et al., 1997; Harvey, Richards, Dziadoz, & Swindell, 1993; Kamieniecki, Wade, & Tsourtos,1997; J. C. Richards, Austin, & Alvarenga, 2001). Clark et al. (1997) also observed this bias for participants with a principal diagnosis of GAD. Finally, social-phobics, relative to controls, have been found to endorse threat-congruent interpretations of socially-relevant hypothetical scenarios as being the interpretations which they would be most likely to make (Amir, Foa, & Coles, 1998; Stopa & Clark, 2000).

Despite the fact that the effects obtained in the studies reviewed above are consistent with the operation of a threat-congruent interpretive bias in anxiety, a serious methodological problem limits confidence in the conclusion that such a bias necessarily underpins the findings. In such experiments, participants are typically asked to indicate

how they *believe* they would interpret the hypothetical ambiguous situations. An influential review by Nisbett and Wilson (1977), concerning the degree to which people can access higher order inferential processes such as interpretations and attributions, raises major difficulties for this approach. Nisbett and Wilson cite a plethora of studies to support their argument that individuals commonly report inaccurately on higher order inferential processes, despite their subjective impression that these processes are consciously accessible. These theorists demonstrate that individuals are often unable to report accurately on the nature of their inferences and the factors that influence them. In explaining these observations, Nisbett and Wilson take the view that individuals simply employ some personal and often inaccurate theories to report on the nature of their mental processes. It could be then, that the methodologies reviewed above, rather than demonstrating the interpretive biases of anxious participants, instead tell us more about the personal theories brought to bear by such individuals when instructed to speculate about their likely interpretations. That is, participants must make judgments about the likely responses that they believe would be made in the presence of ambiguous information, but these judgments may not be accurate. Fortunately, other methodologies less susceptible to this methodological limitation have also yielded findings consistent with the operation of a threat-congruent interpretive bias in individuals with heightened vulnerability to anxiety.

Reporting actual interpretations of ambiguous information.

The obvious way of addressing the weakness of an approach in which participants indicate only how they *believe* they would interpret hypothetical ambiguous stimuli, is to directly present such ambiguous stimuli, and require participants to indicate the interpretations which they actually *do* impose on this ambiguity. One such

methodology has involved exposing (usually socially) anxious individuals to a social situation, such as an interaction with an experimental confederate, then presenting a recording of this interaction and requiring participants to rate their social performance. The recording of the interaction, which involves complex social cues, may permit multiple, differentially valenced, interpretations to be imposed. Thus, the ambiguous information is actually presented to participants, who directly report their interpretations.

In a study of this type, J. V. Clark and Arkowitz (1975) involved high socially-anxious and low socially anxious men in social interactions with female confederates. Subsequently, participants rated their interactions from audio-tape recordings. These recordings were also rated by other judges blind to group selection. While low socially anxious participants tended to interpret their interactions as more socially skilled than did the independent judges, this pattern was reversed for the high socially anxious-group. Burgio, Glass, and Merluzzie (1981) obtained a similar pattern of findings in a replication study. When appraising videotapes of such interactions, high socially anxious participants reported negative interpretations of their social performance, while independent judges did not differentiate high and low socially anxious groups on the basis of their performance.

Additionally, in a pair of studies, Derakshan and Eysenck (1997) required high and low trait anxious participants to complete impromptu speeches either before a video camera alone (Experiment 1) or before a video camera and audience (Experiment 2). In each case, when subsequently appraising these video recordings, high trait anxious participants, relative to low trait anxious participants, interpreted their public speaking performance negatively, though this was not the view of independent judges.

Similar findings have also been obtained for individuals receiving clinical diagnosis of social phobia in comparison to control groups, suggesting that though social phobic individuals may be deficient in their level of social performance, they also rate their own performance disproportionately more negatively than do comparison groups (Rapee & Lim, 1992; Stopa & Clark, 1993).

Across such studies, therefore, the outcomes are again consistent with the presence of a threat-congruent interpretive bias in anxiety vulnerable individuals. However, while these studies may overcome the problems associated with reporting beliefs regarding *future* interpretations of *hypothetical* situations, they may be readily criticised for their obvious demand characteristics. In each of these studies, it is possible that the more socially anxious participants provided a greater number of threatening interpretations on the basis of their beliefs about what the experimenter expected. It is likely that these individuals recognised that their social anxiety was the concern of the experiment in each case. It therefore becomes possible that they saw it as appropriate to rate their own performance as less skilled and/or more anxious, thus appearing to interpret their own behaviour negatively.

To avoid the possible influence of demand effects, some experimenters have made both the purpose of the task and the measure of interpretive bias less salient to participants. These requirements are most readily met in studies which do not instruct participants to report their interpretations of ambiguous information, but instead indirectly infer such interpretations from various parameters of task performance.

Performance-Based Measures Of Interpretive Bias

At least four approaches to the assessment of interpretive bias have gone some considerable way towards overcoming the problem of experimental demand. Each of these approaches obtains a measure of performance which in some way relates to the initial interpretation of an ambiguous stimulus, without explicitly highlighting the need to make this interpretation.

The homophone spelling approach.

One well-replicated approach which plausibly reduces experimental demand was first employed to compare high and low trait anxious participants by M. W. Eysenck, MacLeod, and Mathews (1987). These researchers presented participants auditorily with homophones. Each homophone had a threatening meaning with one spelling and a non-threatening meaning with a different spelling (e.g. dye/die). These homophones were mixed among a greater number of unambiguous words, to obscure the interest of the experimenters in interpretation. Participants were not informed that the words could have more than one meaning. The task was represented as a spelling test, and participants were required simply to write each word down. The key variable of interest was the provided spelling of each homophone. High trait anxious participants, relative to the low trait anxious group, provided a disproportionate number of threat spellings, consistent with the expectation that they would interpret these words in a threat-congruent way. This finding has been replicated by other researchers (Byrne & Eysenck, 1993; Dalgleish, 1994; Mogg et al., 1994, Study 2; Richards, Reynolds, & French, 1993; Russo, Patterson, Roberson, Stevenson, & Upward, 1996). Additionally, Mathews, Richards, and Eysenck (1989) used this task to empirically contrast a clinically anxious group (GAD) with a control, non-anxious

group. Consistent with findings from high trait anxious participants, the clinically anxious participants, relative to non-anxious controls, provided a disproportionate number of threat spellings.

The sentence-stem completion approach.

Performance measures have also been used to indirectly infer the interpretations imposed on more complex stimuli, such as ambiguous sentence stems. For example, Stoler and McNally (1991) provided agoraphobic and control participants with stems which could be interpreted in a way that was relevant or irrelevant to threatening physiological symptoms. For example, 'After trying to avoid being in this state, I…', could be interpreted as referring to a geographical state, or to a physiological state of panic. Thus, a threat-related completion for this example could be, "… I am angry that I am having a panic attack", and a non-threat related completion could be "…looked for a job back home. I didn't particularly want to move back to Ohio either though" (Stoler & McNally, 1991, pp.541-542). Participants were not informed that the sentence stems permitted alternative resolutions differing in valence. Consistent with expectations, agoraphobic participants provided more sentence completions consistent with a threat interpretation than did control participants.

Collectively, it may be that these findings, and those from the homophone task, are less susceptible to the demand effect explanation, and to the problems of introspective access, than the other classes of study reviewed earlier. And again, these findings are consistent with the suggestion that anxiety vulnerability is associated with a threat-congruent interpretive bias. However, some theorists (e.g., M. W. Eysenck 1992a), have suggested that methodologies which require participants to select between

differentially valenced response options are always subject to response-bias explanations.

Specifically, it could be the case that anxiety vulnerable individuals are characterised by a bias to provide or endorse threat-relevant response options, regardless of the actual interpretation imposed. Thus, for example, participants may have often apprehended both meanings of the homophones in each relevant study, but the more vulnerable individuals may have chosen to respond with the threat spellings more often than the non-anxious participants. One means of overcoming this difficulty was suggested by Mathews et al. (1989), who assessed galvanic skin conductance responses to the presented homophones, reasoning that genuine threat-congruent interpretations would precipitate greater fluctuations on this measure. While trends consistent with an anxiety-linked interpretive bias emerged, the findings were not significant, thus preventing any strong conclusions. However, as will be discussed, another form of task, the false recognition memory task, has been modified to more rigorously exclude the influence of response bias upon task performance.

The false recognition memory task.

One means of distinguishing interpretive bias from response bias has been provided by a methodology in which interpretations are assessed from patterns of recognition memory (e.g., M. W. Eysenck, Mogg, May, Richards, & Mathews, 1991). In M. W. Eysenck et al.'s (1991) studies, participants were auditorily presented with a series of ambiguous sentences, each of which could be interpreted in a threat-congruent or a non-threat congruent fashion. For example, 'Your boss calls you to their office to discuss the quality of your recent work' (M. W. Eysenck et al., 1991, p. 148). In a subsequent recognition test, participants were presented with threat and non-threat

disambiguations of such sentences. Thus, for the above example, the threat disambiguation was: ‘Your boss calls you to their office to say your work is not up to standard’; and the non-threat disambiguation was: ‘Your boss calls you to their office to congratulate you on your work’ (M. W. Eysenck et al., 1991, p. 148). Eysenck et al. found that generalised anxiety disorder patients, relative to a non-clinical control group, displayed a greater degree of false recognition memory for threat, relative to non-threat disambiguations, suggesting that they had more frequently interpreted these sentences in a threatening fashion.

In a follow-up study (M. W. Eysenck et al., 1991, Experiment 2), these researchers then assessed the degree to which their results could be attributed to response bias. Specifically, the recognition memory test now included threat and non-threat distractor sentences which did not correspond to any possible interpretation of each original ambiguous sentence. Thus, with reference to the above example, the threat distractor was: ‘Your boss calls you to their office to ask you why you are not getting on with your colleagues’; and the non-threat distractor was: ‘Your boss calls you to their office to offer you a pay rise’ (M. W. Eysenck et al., 1991, p.148). Signal detection analysis revealed that the clinically anxious participants were more likely to endorse the threat, than non-threat disambiguations, relative to the non-anxious control group, but did not display a similar elevated tendency to endorse the threat, relative to non-threat, distractors. Thus, a response bias explanation was not supported by these findings.

However, the impact of these findings may be compromised by a different criticism levelled against this methodology. Specifically, because the participants’ interpretations are not assessed at the time of stimulus presentation, the observed effects may reflect anxiety-linked biases in memory retrieval, rather than in interpretation

(e.g., MacLeod & Cohen, 1993). That is, all participants may initially impose an equal number of threatening and non-threatening interpretations on the ambiguous stimuli, but anxious individuals may be disproportionately capable of recognising their threatening interpretations in the subsequent memory test. However, studies less susceptible to this criticism, as well as to the other criticisms already highlighted, have continued to reveal findings consistent with the hypothesis that anxiety vulnerability is indeed characterised by a threat-congruent interpretive bias.

Priming tasks.

The class of methodology that perhaps best overcomes these various problems includes a number of interpretive assessment tasks known broadly as priming tasks. These tasks present ambiguous items (e.g., words or a sentences) as prime stimuli. Interpretive bias is inferred by observing the degree to which the presentation of each ambiguous prime facilitates (i.e., primes) the processing of a subsequently presented target, which on different trials may be related to the threatening or to the non-threatening meaning of the prime. This relative degree of facilitation for threat related and non-threat related targets is typically contrasted with appropriate control conditions, in which the influence of such differential interpretations upon performance is prevented, for example, by presenting the related targets in the absence of the primes. Thus, the contaminating influence of response bias and other target-specific processing explanations can be controlled for. Further, because interpretation is inferred from a response index recorded immediately following the presentation of the ambiguous stimulus, explanations in terms of post-interpretive memorial biases are also largely eliminated.

Using this approach, the hypothesis that anxiety vulnerability is characterised by a threat-congruent interpretive bias has been assessed using ambiguous words, or more extensive pieces of ambiguous text as primes, and using both words and sentences as the targets through which the interpretation is inferred.

In one elegant study employing the priming procedure, Richards and French (1992) assessed the interpretive bias of high and low trait anxious participants by presenting them with a series of homograph primes, each permitting a threat-congruent and a non-threat-congruent interpretation (from here on, referred to as threat/non-threat homographs), such as “stroke” and “sentence”. Each homograph prime was followed a short time later by a target word which was an associate of the prime’s threatening or non-threatening meaning. The participant made a lexical decision response to indicate whether or not this target associate was a word (yes/no). Results were consistent with the operation of an anxiety-linked interpretive bias. For prime-target SOAs at or exceeding 750 ms, high, relative to low trait anxious participants, displayed shorter decision latencies for threat-related targets, relative to those which were non-threat-related, suggesting that they were biased to impose threat-congruent interpretations upon these primes. Explanations alluding to the processing of valenced stimuli, irrespective of interpretive bias, were excluded by the inclusion of a baseline condition in which related homographs did not appear. These findings are thus fully consistent with the operation of a threat-congruent interpretive bias in anxiety vulnerability.

Similar findings were obtained in a study cited by MacLeod (1990) in which participants named word targets related to the threatening or non-threatening meanings of ambiguous sentence-primes. High trait anxious participants, relative to the low trait

group, obtained reduced naming latencies for threat related, relative to non-threat related targets, relative to an appropriate baseline, thus suggesting a threat-congruent interpretive bias. Using similar stimuli, comparable findings were obtained by Calvo and Castillo (2001, Experiment 1); and by Calvo, Eysenck, and Estevez (1994), for high and low test anxious individuals.

Hirsch and Mathews (1997) investigated interpretive bias in socially anxious individuals, using descriptions of interview situations as primes. These descriptions were incomplete at various points at which potentially threatening or non-threatening outcomes could be anticipated, depending upon interpretation of the interview description. At each of these ambiguous points, a single target word was presented, which could be related to the threatening or non-threatening interpretation of the prime information. In the first study, participants were required to judge whether or not the target was consistent with the preceding prime description. Participants who reported being vulnerable to experiencing anxiety about interviews, displayed shorter judgement latencies for targets related to the threatening, rather than to the non-threatening meanings of these primes. They also showed faster lexical decisions for threat-related, relative to non-threat related targets in a subsequent study (Hirsch & Mathews, 1997, Experiment 2).

In their final study (Hirsch & Mathews, 1997, Experiment 3), these researchers examined explanations of their effects relating to the processing of valenced target information, independent of the influence of differential interpretations of the primes. This was done by presenting the valenced targets in isolation from their preceding primes. Consistent with expectations, in the absence of an emotionally ambiguous prime, the anxiety-linked difference in target processing disappeared. Thus, these

findings are best understood as implicating a threat-congruent interpretive bias associated with anxiety vulnerability. Subsequently, these researchers (Hirsch & Mathews, 2000) extended their approach to a socially phobic group, and found that these socially phobic individuals, relative to non-anxious controls, were also characterised by a threat-congruent interpretive bias, as assessed using the same priming procedure.

As well as the primes, the complexity of the target information provided in these tasks has also varied. Interpretive bias has been assessed using tasks in which the interpretation is inferred from the participant's reading time for an entire target sentence, as opposed to their latency in responding to a single target word. In one such study, MacLeod and Cohen (1993) presented participants with a priming sentence which permitted both a threatening and a non-threatening interpretation. Each sentence was followed by a second, target sentence, which was related to the threatening or non-threatening meaning of the priming sentence. For example, "Working behind bars gave Sammy a new view of life", could be followed by, "The prisoners' problems greatly affected his attitude" (threatening continuation), or, "The patron's problems greatly affected his attitude" (non-threatening continuation) (MacLeod & Cohen, 1993, p. 247).

For targets following unconstrained ambiguous sentences, the high trait anxious group, relative to low trait anxious group, displayed shorter comprehension latencies for threat, compared to non-threat continuation sentences, in comparison to appropriate baseline conditions which constrained the capacity for differential interpretations to be produced. Thus, these participants were more likely to impose the threat-congruent interpretation on each initial priming sentence. Subsequently, Calvo, Eysenck, and

Castillo (1997), and Calvo and Castillo (1997) reported similar results for high and low test anxious participants, suggesting that the high text anxious participants were more likely than low test anxious participants to interpret ambiguous textual scenarios in a threat-congruent fashion.

To conclude, priming studies, which overcome many of the shortcomings of other methodologies used to assess interpretations, nonetheless indicate that anxiety vulnerability is indeed associated with a threat-congruent interpretive bias.

The Causal Basis Of The Relationship Between Anxiety Vulnerability And Interpretive Bias

Studies employing methodologies increasingly less susceptible to the methodological problems raised earlier have consistently evidenced that anxiety vulnerability is indeed associated with a tendency to interpret ambiguity in a threat-congruent fashion. Failure to consistently observe this association would disconfirm the central causal hypothesis under consideration, that interpretive bias causally mediates anxiety vulnerability. Nevertheless, the existence of this association alone can not serve to confirm this hypothesis. For example, though some inconsistent findings have been obtained (e.g., Lawson & MacLeod, 1999), some recent evidence suggests that depression may also be characterised by a negative interpretive bias (e.g., Lawson, MacLeod, & Hammond, 2002). Perhaps then, the causal role of interpretive bias is not specific to anxiety vulnerability, influencing vulnerability to depression also.

Of greater importance, the observation of this association does not necessarily require the inference that emotional vulnerability is causally mediated by interpretive bias. For example, it is possible instead, that the more frequent and intense experience

of anxiety elevations by vulnerable individuals predisposes these individuals to develop a threat-congruent interpretative bias. Indeed, Bower's (1981) influential framework for explaining the relationship between mood and cognition implies this alternative. Of course, the possibility that this opposite direction of causation operates (i.e., that elevated anxiety vulnerability leads to the development of threat-congruent interpretive bias) does not necessarily preclude the possibility that interpretive bias also causally mediates anxiety vulnerability. Nonetheless, the plausibility of the alternative account certainly highlights the fact that the reported evidence for an association between interpretive bias and anxiety vulnerability does not implicate one particular causal pathway.

In addition, it remains possible that interpretive bias and anxiety vulnerability are not causally linked, and instead are each causally mediated by an unknown third factor. Their association might therefore be explained by the similar influence of this third factor upon interpretive bias and upon anxiety vulnerability.

Evidence Bearing Upon Minimal Predictions Provided By The Causal Hypothesis

We turn now to other sources of evidence which may reflect upon the validity of the specific causal hypothesis under consideration - the hypothesis that interpretive bias causally mediates anxiety vulnerability. Apart from the associative evidence already considered, there are at least three classes of evidence which may have some bearing upon the causal hypothesis. Firstly, if interpretive bias causally mediates vulnerability, then this bias should be detected, developmentally, wherever the earliest indications of anxious symptomatology are evident. Secondly, if interpretive bias necessarily causally mediates anxious symptomatology, then the successful treatment of such symptoms

should be accompanied by a concomitant reduction in interpretive bias. Thirdly, if interpretive bias causally mediates anxiety vulnerability, then variations in this bias should predict subsequent variations in vulnerability.

In considering these classes of evidence, two distinct issues may be considered. One is whether or not the causal hypothesis survives disconfirmation in light of the evidence obtained. The failure to obtain the findings predicted by the causal hypothesis, within each class of evidence, would place the veracity of the causal hypothesis in doubt. A second issue is whether or not the findings emerging from a particular class of evidence provide *direct* support for the causal pathway under consideration. That is, it may be important to consider alternative causal explanations in each case, such as the possible operation of the reverse causal pathway, with anxiety vulnerability causally mediating the development of interpretive bias. These issues will be considered with respect to each class of evidence.

Developmental Expression Of Anxiety Vulnerability And Interpretive Bias

If anxiety vulnerability represents a direct consequence of interpretive bias, then a threat-congruent interpretive bias should be evident even in the earliest childhood expression of anxiety vulnerability. Conversely, if this bias emerges only as a consequence of repeated anxiety episodes, then a threat-congruent interpretive bias might not be observed in anxious children. A small number of studies have attempted to examine the association between interpretive bias and anxiety vulnerability in anxious children. As described below, these studies have typically mirrored the findings obtained in the larger number of adult-focused studies.

Barrett, Rapee, Dadds, and Ryan (1996) conducted a large-scale study of interpretive bias in children with anxiety disorders, using a modified version of Butler et al.' s (1983) ambiguous scenarios task. Participants were children aged 7 to 14 years, who were diagnosed with any of a variety of childhood anxiety disorders (overanxious disorder, separation anxiety disorder, simple phobia or social phobia), or alternatively had another psychiatric diagnosis (the control group). An interviewer provided the children with a number of ambiguous scenarios adapted to be relevant to this age range. Participants were asked to explain each scenario freely, then were presented with a number of possible interpretations which they were asked to rank in terms of subjective probability. Results from each measure were consistent with the operation of a threat-congruent interpretive bias amongst the anxious group, with anxious children supplying and selecting more threat interpretations than was the case for the control group. Chorpita, Albano, and Barlow (1996) obtained a similar pattern of results when they presented ambiguous scenarios to anxiety disordered children, in the age range of 9 to 13 years. Subsequently, these effects were also replicated for a group of children aged 6 to 14 years, by Shortt, Barrett, Dadds, and Fox (2001).

A similar methodology was employed by Leitenberg, Yost, and Carroll-Wilson (1986) with a non-clinical sample of children who differed in levels of anxiety regarding social evaluation. In their study, high and low evaluation anxious children (fourth, sixth and eighth grades) were presented with a number of scenarios, and were asked to rate the similarity of presented interpretations of each scenario to their own likely interpretation of that same situation. High evaluation anxious children, relative to low evaluation anxious children, produced profiles of ratings suggestive of a threat-congruent interpretive bias.

Other methodologies have been used to investigate the interpretations imposed by anxious children upon single ambiguous words. Taghavi, Moradi, Neshat-Doost, Yule, and Dalgleish (2000) presented children and adolescents (aged 8-17 years) who were clinically anxious with threat/non-threat homographs similar to those employed by Richards and French (1992). Participants were instructed to use each homograph within a self-generated sentence, and these sentences were then examined to determine which meaning the participants imposed on the homographs. As predicted, the anxious group, relative to controls, produced a greater number of sentences consistent with the homographs' threatening meanings, showing that they were more likely to impose threat-congruent interpretations upon them. Similar findings were obtained for children without a clinical diagnosis who demonstrated elevated levels of trait anxiety. Hadwin, Frost, French, and Richards (1997) employed a modification of M. W. Eysenck et al.'s (1987) homophone paradigm to test such a sample of 7 and 9 year old children. Each child was read a series of threat/non-threat homophones, and each homophone was accompanied by a pair of pictures depicting its alternative referents. Children were instructed to select the picture in each pair that was best described by the word. The picture selected by each child was used to infer the interpretation that they had imposed on the homophone. Individual differences in trait anxiety across the sample were found to be positively associated with the number of threatening pictures selected, again consistent with the operation of threat-congruent interpretive bias in the more anxious children.

Of course, as has already been discussed at length in descriptions of adult studies, these methodologies may be susceptible to a number of difficulties. For example, the studies asking participants to provide their likely interpretations of

hypothetical scenarios (e.g., Barrett et al., 1996) may be assessing second-order beliefs about interpretations, as opposed to those interpretations themselves, as well as being susceptible to the potential impact of experimental demand. However, though not conclusive, the findings from this handful of studies are certainly consistent with the prediction generated by the causal hypothesis, that interpretive bias should characterise anxiety vulnerability at its earliest observed instances in development. Thus, these findings certainly do not discredit the hypothesis that interpretive bias may causally mediate anxiety vulnerability.

On the other hand, even if subsequent studies addressing methodological shortcomings in these studies served to sustain this expectation, it would not be necessary to conclude that interpretive bias causally mediates anxiety vulnerability. For instance, the possibility can not be excluded that interpretive bias simply emerges very rapidly as a consequence of the emergence of clinical anxiety in children. Thus, these findings, even in their strongest form, do not allow us to conclude that the bias is the precipitator of the anxiety vulnerability.

Covariation Of Interpretive Bias With Treatment-Induced Changes In Anxious Symptoms

Predictions from the causal hypothesis may also be examined by assessing the changes in interpretive bias that occur when clinically anxious patients receive treatment. If a threat-congruent interpretive bias causally maintains a particular anxiety disorder, then effective treatment of this disorder should result from a treatment which reduces threat-congruent interpretive bias to some degree. Conversely, the presence of the same degree of threat-congruent interpretive bias in recovered anxious patients

would not support this causal hypothesis. A number of studies provide data which may be used to test this prediction. These studies have assessed interpretive bias in clinically anxious individuals prior to and following treatment.

One approach has been cross-sectional, and has involved comparing a recovered anxious group with a currently anxious and control group, to determine differences in interpretive bias across these three groups. McNally and Foa's (1987) study examining interpretations of hypothetical scenarios in agoraphobics also included a group of recovered agoraphobics. This recovered group provided fewer threat-congruent interpretations, and rated threat-congruent interpretations as less probable, than did the group awaiting treatment. Additionally, this treated group did not differ from the controls in these respects.

Similar findings were obtained in two other studies which used methods less susceptible to demand-effect explanations. Mathews et al. (1989) included a recovered generalised anxiety disorder group in their homophone study. Unlike the symptomatic GAD group, these recovered participants provided a proportion of threat, relative to non-threat spellings, that did not differ significantly from that provided by the control group. M.W. Eysenck et al.'s (1991, Experiment 1) study, which employed the recognition memory measure of interpretive bias, also included a recovered GAD group. This recovered anxious group did not differ from the control group, but differed significantly from the anxious group, showing a reduced tendency to falsely recognise threatening, relative to non-threatening disambiguations of ambiguous sentences. Thus, in each case, evidence for a threat-congruent interpretive bias was not detected amongst those who also evidenced remediation of anxiety dysfunction.

Findings complementary to those above were obtained in Stoler and McNally's (1991) study examining completions of ambiguous sentence stems. This study also included a control group of individuals who had already received treatment for agoraphobia. These participants still displayed an interpretive bias which differed from that shown by the non-anxious control group, which at first glance may appear to be contrary to the causal hypothesis. However, while this group no longer met the full diagnostic criteria for agoraphobia, they still scored significantly higher than the non-anxious controls on measures of anxiety and fear of bodily sensations. Thus, in this case, the failure to effectively ameliorate threat-congruent interpretive bias was accompanied by the sustained expression of anxiety vulnerability.

Other studies providing data relevant to this issue have assessed interpretive bias in the same group of participants prior to and following treatment. Westling and Öst (1995) compared panic disordered patients with controls, prior to and following psychological treatment. All participants completed a questionnaire which involved the ranking of possible interpretations of emotionally ambiguous events. Prior to treatment, the panic-disordered group differed from the controls by demonstrating a preference for threat-congruent interpretations of the ambiguous stimuli. Following successful treatment, the panic disordered patients did not differ from the controls in this way.

D. M. Clark et al. (1997, Experiment 3) also obtained results consistent with the causal hypothesis. In this study, panic patients were assigned to a treatment group or to a 3-month wait-list control group. Comparisons with the control group demonstrated that the treatment group displayed a reduction in the ranking of threat interpretations of ambiguous events, which was associated with a reduction in anxious symptoms.

The findings from these studies are consistent with the prediction, generated by the causal hypothesis, that interpretive bias should be modified by treatments which successfully reduce symptoms of anxiety. However, these findings do not unambiguously sustain the hypothesis that threat-congruent interpretive bias causally mediates anxiety vulnerability. An alternative explanation for these findings could be that interpretive bias declines with other symptoms of the anxiety disorder, either because the anxiety symptoms causally influence the bias, or because both the bias and the anxiety symptoms are independently caused by some third factor, which is influenced by the treatment manipulation.

Prediction Of Variation In Anxiety Vulnerability From Prior Expression Of Interpretive Bias

In order to more directly assess the hypothesis that interpretive bias causally mediates anxiety vulnerability, Pury (2002) used the approach of assessing interpretive bias prior to the point at which participants encountered a stressor. If interpretive bias causally mediates elevated state anxiety in response to such a stressor, it should be the case that the interpretive bias displayed at this earlier point in time predicts the anxiety elevation subsequently displayed in response to the later stressor. In Pury's study, unselected university students completed Mathews et al.'s (1989) homophone task approximately 30 days prior to their examinations. It was predicted that if interpretive bias causally mediates anxiety vulnerability, then the degree to which these individuals displayed an interpretive bias towards threat during an initial testing session would be predictive of subsequent elevations in anxiety symptoms elicited by impending examinations. Consistent with expectations, the proportion of threat spellings provided

at the earlier testing time was indeed predictive of the degree to which negative affect (both anxious and depressive symptoms) subsequently became elevated closer to the examinations. This predictive capacity was found to remain even when variance attributed to an initial trait anxiety questionnaire measure was accounted for.

Despite reservations (expressed earlier) regarding this measure of interpretive bias, these findings do not undermine the hypothesis that interpretive bias may causally mediate anxiety vulnerability, though they suggest that the causal contribution of this bias may be broader, influencing vulnerability to elevation of both state anxiety and depression in response to stress. Nevertheless, once again these findings are insufficient to draw the firm conclusion that interpretive bias causally mediates anxiety vulnerability. It could be the case, for example, that anxiety vulnerability and interpretive bias are each causally determined by some third, unknown, individual difference factor, which remains stable across the testing period.

Limitations Of Studies Indirectly Examining The Causal Role Of Interpretive Bias In Anxiety Vulnerability

As has been indicated, as with studies which investigate the association between interpretive bias and anxiety vulnerability, the three classes of evidence described above each depend in some way upon the demonstration of covariation. Thus, findings from the developmental literature, and treatment recovery studies, are equally amenable to the possibility that anxiety vulnerability causes the development of interpretive bias, instead of the reverse being the case. Furthermore, none of these approaches can rule out the possibility that interpretive bias and anxiety vulnerability are each determined by a third, underlying, causal factor.

Therefore, it would be premature to conclude that interpretive bias can causally mediate anxiety vulnerability, although it would certainly be true to say that none of the findings conveyed thus far preclude this possibility. The strongest evidence for the causal role of interpretive bias in anxiety vulnerability would come from studies within which interpretive bias is directly and specifically manipulated in order to determine the impact of this manipulation upon anxiety vulnerability. Equally important, such manipulation should occur in isolation from any confounding factors (such as variations in mood state). If it were to be found that such manipulation indeed served to produce consequent variation in anxiety vulnerability, then this would provide strong evidence for the causally mediating role of interpretive bias.

Attempts To Directly Assess The Causal Hypothesis Through Experimental Manipulation Of Interpretive Bias

It has been suggested that the causal hypothesis may be best assessed by directly manipulating interpretive bias and examining consequent changes in anxiety vulnerability. Only a small number of studies consistent with this rationale have been reported in the literature (Grey & Mathews, 2000; Mathews & Mackintosh, 2000). These studies have sought to develop methodologies capable of manipulating interpretive bias by modifying procedures originally designed to assess interpretive bias. Such interpretive training procedures have been created through the introduction of certain contingencies between the ambiguous stimuli and valenced targets presented to participants. The intention behind the introduction of such contingencies is to make one particular interpretive bias (threat or non-threat-congruent) advantageous for participants in a particular condition, by ensuring that this particular style of

interpretation is consistently beneficial to the processing of the targets, thereby encouraging the development of this interpretive bias. Thus, these training procedures are designed with the intention of inducing group differences in interpretive bias.

Tasks Developed To Experimentally Manipulate Interpretive Bias

Across a series of experiments, Grey and Mathews (2000) developed variants of Richards and French's (1992) priming methodology, into which training contingencies were introduced to manipulate interpretive bias. In their first study, Grey and Mathews (2000) employed a fragment-completion variant of the priming task. Each critical trial involved the initial presentation of a threat/non-threat homograph prime. This was followed, 750 msec later, by a target word, an associate of either the threatening or the non-threatening meaning of the prime. Letters were deleted from each target associate, producing a word-fragment. Each participant was instructed to use each prime as a clue when solving the word-fragment, and to indicate with a button-press as soon as they were able to identify the fragment. Their response latency to indicate knowledge of the solution to the fragment was recorded. They then demonstrated the accuracy of their solution by pressing the appropriate key to supply the fragment's first missing letter.

The experiment incorporated an interpretive training phase, followed by an interpretive assessment phase in which the success of the interpretive training was appraised. A contingency was introduced during the interpretive training phase, with participants assigned at random to either a non-threat or to a threat training group, and each group received a different training contingency. For the threat training group, the solution to the word-fragment target was always related to the prime's threatening meaning. For this group, therefore, it was advantageous to always access the threatening

meaning of each homograph prime to help with completion of the subsequent word-fragment. The goal of introducing this condition was thus to induce, across a number of trials, a bias favouring the threat-congruent interpretations of ambiguous stimuli in these participants. Conversely, for the non-threat training group, the solution to the word-fragment was always related to the non-threatening meaning of the preceding threat/non-threat homograph prime, making it advantageous to selectively access the non-threatening meaning in each case. Thus, across repeated trials, it was expected that this other group would acquire a bias favouring non-threat-congruent interpretations of ambiguous stimuli.

An interpretive assessment phase followed, designed to determine the success of the interpretive training procedure. In these assessment trials, threat or non-threat word-fragments followed homograph primes with equal frequency. Following the training, participants from the non-threat training group, relative to the threat training group, displayed facilitation in completing non-threat related, relative to threat related fragments in the assessment trials. Grey and Mathews (2000) took this pattern of findings as evidence that the groups had indeed acquired differential tendencies to impose particular valences of interpretation upon novel ambiguous stimuli, depending upon the received training condition.

A subsequent study reported in the same paper employed different training and assessment methodologies, but obtained a pattern of results that invited the same conclusion (Grey & Mathews 2000, Experiment 3). In this later experiment, the training task involved making relatedness judgments, and the interpretive assessment employed the same lexical decision task as used by Richards and French (1992). On training trials, participants were required to indicate whether or not semantic

relationships existed between homographs and target words presented with an SOA of 750 msec. The training contingencies were such that for different groups of participants, related targets were only associated with one valence of meaning of the homographs, with the goal again of inducing a group difference in interpretive bias. Thus, for some participants assigned to the threat training group, these targets were always related to the threatening meanings of the homographs, and for the non-threat training group, targets were always related to the non-threatening meanings of the homographs. In the subsequent lexical decision task used in interpretive assessment, participants from the non-threat training group, relative to the threat training group, were found to display shorter decision latencies for targets related to the non-threatening, rather than to the threatening meanings of new homograph primes. Once again, this finding is consistent with the hypothesis that participants assigned to the different training groups acquired differing interpretive biases in response to the training.

As noted by Grey and Mathews (2000) themselves, one obstacle to concluding that their findings necessarily reflect induced interpretive biases comes from the absence of an appropriate baseline condition during the interpretive assessment phase in each study. Richards and French's (1992) task included a control condition within which valenced targets were *not* preceded by related homograph primes, so that target processing effects due to the selective *interpretation* of homograph primes could be distinguished from any acquired tendency to simply process targets related to differentially valenced prime meanings more rapidly. Grey and Mathews did not include such a control condition in their own interpretive assessment phase. Thus, to clarify whether their observed training effects were really due to the acquisition of differentially biased interpretive style, it would be necessary to extend their assessment

procedure by adding trials in which target words are not preceded by ambiguous primes. Despite this limitation, it does seem plausible that Grey and Mathews' training procedures may indeed provide a means of systematically manipulating interpretive bias.

Mathews and Mackintosh (2000) further expanded the range of such interpretive training methodologies. For example, participants were asked to read extended vignettes describing hypothetical scenarios. As with previous studies employing similar methodologies (e.g., Hirsch & Mathews, 1997) certain emotionally relevant aspects of each scenario were left ambiguous in each description. Participants were then presented with a sentence which ended in a word-fragment whose completion could be related to only one meaning of the scenario. Participants were required to complete this word-fragment. The completion task therefore required that one of the two meanings of the vignette was accessed. Participants once again were assigned to threat and non-threat training groups, and each group completed sentences within which the words to be completed were exclusively consistent with only the threatening or with only the non-threatening interpretations of the preceding vignettes. Of course, the intention of introducing this contingency was to create a group difference in interpretive bias.

Following this interpretive training phase, participants were then presented with a series of novel ambiguous scenarios. Participants' interpretations of these new scenarios were assessed using a false recognition methodology similar to that employed by Eysenck et al. (1991), described earlier in this chapter. Findings were consistent with the expectation that training would induce the intended group difference in interpretive bias. The two groups falsely recognised differing ratios of threatening and non-threatening disambiguations, with their apparent interpretations favouring one valence

of meaning in a manner congruent with training group allocation. Despite the restriction imposed upon these findings by assessing interpretive bias using a task that could be contaminated by biases in memory, these findings are again consistent with the possibility that a group difference in interpretive bias was induced.

Assessment Of The Emotional Consequences Of Interpretive Bias Manipulations

As has already been suggested, in order to examine the hypothesis that interpretive bias can causally mediate anxiety vulnerability, it is necessary not only to modify interpretive bias but also to subsequently assess the consequences of such manipulations for anxiety vulnerability. Mathews and Mackintosh (2000) did examine the impact of their training methodology upon mood state, but they never directly examined the impact of this training on anxiety vulnerability. Mood state ratings were obtained prior to the training, and then again following the interpretive assessment. When the groups were required to actively generate emotional meanings of the ambiguous training stimuli, analyses of the anxiety ratings revealed a reduced elevation of state anxiety in the non-threat training group, relative to the threat training group. One possible interpretation of these findings is that the groups, having acquired differential interpretive biases during training, then imposed such biased interpretations upon the unconstrained vignettes presented during the assessment. Consistent with the causal hypothesis, the differential interpretation of these vignettes by the different training groups may have led to their differential elevations in state anxiety. However, it is also possible that exposure to differing training stimuli directly influenced mood state, rather than modifying emotional vulnerability, especially given that the training stimuli provided to each training group probably differed systematically in emotional

valence. What are required then, are methodologies which can directly appraise the impact of training on anxiety *vulnerability*, independent of changes in mood state per se.

Such methodologies would be comprised of three consecutive phases. The initial two phases, as in previous work, would involve, first, training designed to induce a group difference in interpretive bias, and second, the subsequent assessment of the success of this training in producing group differences in interpretive bias. The third crucial component, given the success of the interpretive training, would be to expose these groups to some form of anxiety-inducing stressor. The causal hypothesis under consideration predicts that these groups, having been induced to interpret emotional ambiguity differently, should then display a consequent difference in anxiety vulnerability, as revealed by the magnitude of state anxiety elevation in reaction to the stressor. Thus, the introduction of this subsequent anxiety induction procedure would permit direct assessment of the hypothesis that interpretive bias can causally mediate anxiety vulnerability.

Overview Of Current Research Program

Limitations of Previous Work And Principal Issues Requiring Empirical Investigation

The present research program was designed to evaluate the hypothesis that an interpretive bias can causally mediate anxiety vulnerability. As has been argued, to date, no strong empirical assessment of this hypothesis has been provided in the literature. Grey and Mathews (2000), and Mathews and Mackintosh (2000) each described methodologies which might plausibly manipulate interpretive bias. However, Mathews and Mackintosh employed a measure of interpretive bias which has been criticised for conflating interpretive bias applied at the time of stimulus presentation, and subsequent

bias in memory. Grey and Mathews' (2000) assessment of interpretive bias was less susceptible to this criticism, but did not include the control conditions necessary to distinguish an induced group difference in interpretive bias from a group difference simply in the processing of target associates related to differentially valenced prime meanings. Of these two series of studies, only Mathews and Mackintosh (2000) investigated the emotional consequences of their training, and while their findings are consistent with the causal hypothesis, they do not distinguish training-linked differences in anxiety vulnerability from the direct impact of training on state anxiety per se. Thus, the status of the causal hypothesis, that interpretive bias can causally mediate anxiety vulnerability, remains indeterminate.

Overview Of Methodologies Employed To Investigate The Causal Hypothesis

The present research program was designed to provide a strong test of the causal hypothesis, and this research was comprised of two successive phases. The goal of Phase 1 was to develop and validate training tasks capable of modifying interpretive bias, using the framework provided by Grey and Mathews (2000). It was important here to include the conditions necessary to determine whether such methodologies can indeed manipulate interpretive bias, rather than influencing the processing of valenced target stimuli in ways that do not concern biased interpretations. The goal of Phase 2 was to examine the hypothesis that interpretive bias can causally mediate anxiety vulnerability, by assessing the impact of interpretive bias manipulations upon subsequent vulnerability to experience elevations in state anxiety, in response to a standard experimental stressor.

In Phase 1, Studies 1-4 were thus directed towards the goal of developing an effective interpretive training approach using priming methodologies, thus extending the work of Grey and Mathews (2000). These studies set out to distinguish the hypothesis that such training can indeed modify interpretive bias, from alternative, interpretation-irrelevant explanations, by including the appropriate control conditions in the interpretive assessment phase which followed training.

Phase 2 (Studies 5-8) then sought to examine the impact of manipulating interpretive bias for anxiety vulnerability. Thus, following the training of group differences in interpretive bias, and subsequent to assessing indications of the success of this induction, the groups were then exposed to an anxiety-inducing stressor. The causal hypothesis provided the prediction that the training groups would differ in the degree to which they showed elevations in state anxiety in response to the subsequent stressor, with these differences congruent with the valence (threat or non-threat) of induced group differences in interpretive bias.

CHAPTER 2

DEVELOPMENT AND VALIDATION OF STIMULUS MATERIALS FOR INTERPRETIVE TRAINING AND ASSESSMENT TASKS

This chapter describes the creation of the stimulus array employed in the interpretive training and assessment tasks carried out in this research program. Two studies are described, each of which sought to obtain crucial empirical data required for the development and/or validation of this stimulus array.

Stimulus Considerations Relevant To The Development Of Tasks

As described in Chapter 1, the major goal of Phase 1 was to create methodologies for the manipulation of interpretive bias using a single-word priming approach (e.g., Grey & Mathews, 2000), and to ensure that such training can indeed induce group differences in interpretive bias. Given the use of this single-word priming approach, a number of homographs sustaining differentially threatening and non-threatening interpretations were thus required to serve as primes within the interpretive training tasks, and in subsequent assessments of the success of this training. It was important to ensure that each of the selected homograph primes could individually sustain two meanings differing in valence. It was also important to ensure that, on average, the threatening meanings of the homographs were approximately equivalent in dominance to the non-threatening meanings, that is, in their tendency to be accessed in everyday language. This equivalence in dominance was expected to be important in particular during interpretive assessment where it would be necessary to ensure that

biased tendencies to more frequently access either the threatening or the non-threatening meanings of the homographs could each be detected with sensitivity.

These training and assessment tasks also required word associates related to the threatening and non-threatening meanings of the homograph primes, by which the training contingencies could be instantiated, and variations in the presence of induced interpretive bias subsequently assessed. As conveyed in the introduction, training and assessment methodologies based on a priming approach are premised upon the assumption that access to particular (threatening and/or non-threatening) meanings of the homograph primes will facilitate (i.e., will prime) access to the representations of their related associates. It was important to ensure, furthermore, not only that the homograph primes facilitated access to representations of their related associates (or derivative targets), but also that this facilitation occurred to a similar degree for threat and non-threat related associates. This was understood to be essential to the effectiveness of training contingencies, involving the presentation of target associates of one valence or the other; and for the sensitivity of assessments, comparing the capacity to respond to targets of one valence, relative to the other, when preceded by homograph primes.

Prior to the execution of the major stimulus development studies, a large pool of candidate homographs and related associates was first generated. Study 1 then gathered ratings data which guided the selection of homographs from this pool, with these homographs chosen on the basis of their permitting alternative meanings which differed in valence, but which, on average, were equally dominant. Study 2 then sought to establish the suitability of the stimulus set comprising these selected homographs and related associates. These stimuli were employed in a priming task in which the degree

of facilitation provided by each homograph for the processing of chosen associates, could be assessed.

Generation Of Initial Stimulus Pool

The initial stimulus pool included 250 homographs, each of which was considered to permit two meanings which it was supposed could plausibly be considered threatening and non-threatening, respectively. These homographs came from various published lists of homographs (e.g., Azuma, 1996; Cramer, 1970; French & Richards, 1992; Gorfein, Viviani, & Leddo, 1982; Kausler & Kollasch, 1970; Nelson, McEvoy, Walling, & Wheeler, 1980). For each of the chosen homographs, four associates related to its threatening meaning and four associates related to its non-threatening meaning were also generated. As with the homographs, most of these associates came from relevant lists of word-association norms (Cramer, 1970; French & Richards, 1992; Kausler & Kollasch, 1970; Nelson et al., 1980), with some additional associates generated by the experimenter and colleagues in the UWA Cognition and Emotion Laboratory. Thus, the overall stimulus pool comprised 250 "*nonets*"[1], where each *nonet* consisted of a threat/non-threat homograph, four associates of its threatening meaning, and four associates of its non-threatening meaning.

Study 1

In Study 1, valence and dominance ratings were obtained for the homographs in the stimulus pool, with the goal of selecting the 96 from the 250 (i.e., approximately the most desirable 40%) which best fit the criteria outlined above. As per these criteria, the

[1] This term, referring to this collection of nine items (homograph and 8 associates), will be used to facilitate reference to the stimuli throughout the thesis.

intention was to select those 96 homographs whose alternative meanings were distinct in terms of the degree of threat which they posed, but across which, on average, the threat and non-threat meanings were not perceived as differentially dominant. It was necessary therefore, to obtain ratings of the threat intensity and perceived dominance of the alternative meanings of each homograph. In this study, each participant was randomly assigned to complete one of two possible rating tasks, designed to obtain either threat-related or dominance-related rating information.

To obtain threat ratings, this study employed a variant of the procedure developed by French and Richards (1992). These researchers presented participants with each individual homograph, accompanied by a disambiguating definition which referred either to its threatening or to its non-threatening meaning. Participants in French and Richards' study were required to indicate, on a 7 point scale, the degree of threat posed by the meaning of each homograph identified by the accompanying definition. Following this approach, in the present study, half of the participants completed a questionnaire in which they rated the degree to which each homograph was considered threatening when disambiguated by a definition designed to suggest its purportedly threatening or non-threatening meaning.

To acquire information about the perceived dominance of different homograph meanings, this study employed a variant of Azuma's (1996) approach. This researcher presented participants with a series of homographs, each accompanied by its alternative definitions. Participants were required to indicate, on a provided scale, how often each homograph was used in a fashion consistent with each alternative definition. Thus, in the present study, half of the participants completed a computer task in which each homograph was presented together with alternative definitions referring to its two

meanings. Participants rated, comparatively, their perception of the relative dominance of the meaning implied by each definition.

A selection protocol was then applied to the obtained ratings data in order to select the 96 most appropriate homographs (i.e., those which suggested two meanings associated with different degrees of threat intensity, with these alternatively valenced meanings perceived on average to be equally dominant). Some initial exclusionary criteria were first imposed, to reject those homographs for which the pair of meanings did not differ in threat rating, or for which dominance ratings were excessively polarised towards one meaning or the other. From the remaining portion of the rated pool, the selection procedure was designed to identify those 96 candidate homographs for which the threat ratings obtained for threat definitions were maximised, and were maximally different from the threat ratings obtained for corresponding non-threat definitions. In addition, the selection procedure incorporated the requirement that the meanings, on average, should be relatively equivalent in terms of rated dominance.

Method

Participants

Participants were 12 male and 12 female first-year psychology students, recruited for the study through sign-up sheets posted in first-year laboratories. These students participated in the study in exchange for course credit. Of these participants, 6 males and 6 females were randomly assigned to the dominance rating task, while the other participants completed the threat rating questionnaire. The ages of participants assigned to each group did not differ significantly (17.5 years, SD = 0.9, for threat rating task; 18.4 years, SD = 2.0 for the dominance rating task), $F(1, 22) = 2.04$, *ns*.

Materials

Homograph pool and associated definitions.

For each of the 250 candidate homographs, two short definitions were generated. One definition in each pair referred to its purportedly threatening meaning, and one to its purportedly non-threatening meaning. These definitions were adapted versions of those provided in the Little Oxford English Dictionary (Coulson, 1969), and each definition ranged from one to seven words in length.

Computer hardware.

Stimulus presentation and response recording in the dominance rating task was executed using an Acorn Archimedes Microcomputer with a 15-inch colour monitor. Responses for this task were relayed to the computer via a 3-button mouse connected to the serial port.

Dominance Rating Task

The dominance rating task was executed using an Acorn BASIC program. On each trial, one of the candidate homographs was presented in yellow font, horizontally centred, 4 cm from the top of the screen. One second later, two alternative definitions appeared on the screen. One was presented 4 cm below the homograph. The other was presented 9 cm below the homograph. A percentage score, with a starting point of 50, and followed by the text, '% of the time', was presented 1 cm directly below each definition. The participant's task was to indicate the percentage of time, as a proportion of its overall usage, in which they considered the homograph to be used in a sense consistent with each definition. The participant adjusted the percentage score corresponding to each definition by moving the mouse upwards or downwards.

Movement in either direction increased the score corresponding to one definition, and reduced the score corresponding to the other definition, with the two scores always summing to 100%. The participant was instructed that when the scores were at the point considered to best reflect the usage of the word, they should press the left mouse key. When this key press response was detected, the scores provided by the participant for each sentence were recorded by the computer, and the next trial initiated.

Each participant completed 250 trials. Each of the different 250 candidate homographs and its two corresponding definitions appeared in one of these trials. Across trials, the positioning of definitions of different valences upon the screen was balanced, such that each valence of definition was presented an equal number of times in the upper and lower screen positions, across both male and female participants. The order of presentation of the 250 homographs and associated definitions was randomised for each participant.

Threat Rating Questionnaire

The threat rating questionnaire consisted of 500 items. Each item comprised a homograph together with one of its corresponding definitions. Alongside every homograph was a line of integers from 0 to 6. The participant was instructed to rate, on this scale, the degree of threat posed by each presented word, when understood in terms of the provided definition. The 7 point scale was headed, ‘HOW THREATENING?’. Opposing ends of the scale were marked ‘not at all threatening’ (0) and ‘extremely threatening’ (6), with ‘slightly threatening’ and ‘moderately threatening’ marked at intermediate points (2) and (4), respectively.

Within this questionnaire, each homograph was presented twice, accompanied in each case by a different corresponding definition. A unique copy of the threat rating questionnaire was generated for each participant, with items arranged in a different unconstrained random order each time.

Procedure

Participants were tested individually. Each participant completed either 250 trials of the dominance rating task or the 500 items of the threat rating task, preceded by the verbal conveyance of the appropriate set of instructions (see Appendices I and II). No time limit was imposed for either rating task. Participants completing the dominance rating task were provided with a short break following the completion of 125 of the task's 250 trials. Participants completing the threat rating questionnaire could, of course, take breaks as required, given that the timing of this task was entirely under each participant's own control.

Protocol For Homograph Selection

As indicated, the goal of the homograph selection process was to select 96 homographs possessing distinct threatening and non-threatening meanings that on average were comparable in dominance. Prior to selecting 96 homographs to fulfil these goals, two exclusionary criteria were first imposed upon the entire pool of candidates. Firstly, any candidate homograph for which the supposedly threatening meaning did not obtain a higher threat rating than the supposedly non-threatening meaning was excluded. Secondly, any candidate homograph for which either meaning obtained a

dominance rating of 100% from any of the participants, or for which at least three raters provided a dominance rating of less than 25% for one definition, was also excluded. Following such exclusion, a simple algorithm was applied to the ratings to guide the selection process. This algorithm was comprised of 2 major procedures, with a simple decision rule at the end of each procedure determining the next procedure to be followed. The algorithm was as follows:

1. a. Surviving homographs were rank-ordered by the mean threat rating obtained for the threat definition of each individual homograph, and the 96 obtaining the highest threat ratings were provisionally selected.

b. The homograph that obtained ranking 96 in step (1a) was identified, and the mean threat rating obtained for its threat definition selected as an exclusionary criterion for use in step (1c).

c. Any homograph amongst the provisionally selected 96 for which its non-threat definition obtained a greater mean threat rating than this criterion established in step (1b) was excluded, and step (1) was repeated from step (1a) onwards.

d. When there were no homographs remaining in the interim set of 96 that required exclusion on the grounds indicated in step (1c), then processing progressed to step (2).

2. a. Across the 96 provisionally selected candidate homographs, an overall mean dominance rating and associated standard deviation were calculated separately, for dominance ratings obtained across threat definitions, and for dominance ratings obtained across non-threat definitions, of the surviving candidates.

b. If the discrepancy between these means exceeded one standard deviation, then the homograph which contributed most to this imbalance (i.e., the one for which the difference in mean dominance ratings for its different definitions was most weighted towards the overall imbalance) was then excluded from the remaining pool, and the processing repeated from step (1a) onwards.

c. When the discrepancy between these mean dominance ratings, calculated in step (2a), no longer exceeded one standard deviation, the processing was terminated, and the interim set of 96 candidates remaining at that stage became the final homograph selection.

Results

In Appendix III, the final selection of 96 *nonets* which comprised the stimulus array is provided. The overall mean threat and dominance ratings, calculated across the scores obtained for the relevant 96 threat/non-threat homographs, are provided in Table 2.1. One of the goals of this study was to select the 96 homographs for which the threatening and non-threatening meanings corresponded to distinctly different levels of threat intensity. Unsurprisingly given the selection process, the threat ratings obtained for the threat and non-threat definitions of the selected 96 homographs were indeed significantly different, $F(1, 95) = 558.85$, $p<.001$, in the appropriate direction. In addition, the mean threat ratings obtained for the differentially valenced meanings of

each individual homograph did not overlap across the selection, with a minimum mean threat rating of 1.3 obtained for the threat meaning, and a maximum mean of 1.2 obtained for the non-threat meaning of any homograph in the selected 96.

Table 2.1

Mean Threat-Ratings And Dominance Ratings For Homographs Selected In Study 1

	Valence of Provided Definition	
Characteristic	Non-threat	Threat
Threat rating	0.4	2.6
	(0.3)	(0.9)
Dominance rating	52.0	48.0
	(15.6)	(15.6)

Note. Standard deviations are provided in brackets.

The other goal of this study was to select homographs for which the perceived dominance of these differentially valenced meanings did not on average differ. As can be seen from Table 2.1, the overall mean dominance ratings obtained for each definition were separated by a discrepancy of less than 5%, and this difference was not significant, $F(1, 95) = 1.63$, *ns*. Thus, on average, the alternative meanings of these homographs did not differ in perceived dominance.

Discussion

In this study, the procedures applied to the homograph rating data were successful in yielding a set of 96 homographs which fulfilled the criteria crucial for their

use within interpretive training and assessment tasks subsequently developed in this thesis. As demonstrated in analyses of obtained threat ratings, the selected homographs each provided two different meanings with distinctly different threat valence. Further, as revealed by dominance ratings, these differentially valenced homograph meanings were not perceived to differ significantly in terms of their likelihood of use in everyday speech. Thus, one major aim within this stimulus development phase, the creation of an appropriate set of homographs, was met by this first study. It thus remained for the second major goal to be fulfilled, concerning the validation of the associates related to the alternative meanings of these homographs.

Study 2

This study sought to provide empirical validation of the assumption that the chosen associates, selected on the basis of being judged meaningfully related to one of the two homograph meanings, were indeed semantically primed by preceding their presentation with the presentation of the corresponding homograph. To reiterate, it was a requirement that accessing the meanings of these homographs should facilitate the processing of the supposedly related associates. Thus, it was reasoned that when the stimuli were employed in a priming task, the response to a target associate should, on average, be facilitated by the prior presentation of the corresponding homograph. Furthermore, such facilitation should be observed to occur to a degree that does not significantly differ in terms of whether the target is threat or non-threat associate of that homograph prime, if indeed these two meanings of the homograph prime are comparable in dominance.

Employing a fragment-completion priming methodology (see Chapter 1), this present study assessed the degree to which the chosen homographs facilitated the completion of word-fragments derived from the 8 associated stimulus words (i.e., associates) purportedly related to each homograph. Across trials, each such target word-fragment could either be primed or unprimed by a preceding homograph, thus permitting assessment of the degree to which target identification was facilitated through priming by the homograph. Thus, in primed trials, a threat/non-threat homograph appeared first, and was replaced 2 seconds later by a word-fragment. With equal probability, this fragment was derived from one of the four associates related to the homograph's threatening meaning, or from one of the four associates related to its non-threatening meaning. Thus, in primed trials, the participants were instructed to use the homograph as a cue when attempting to solve the word-fragment. In the unprimed half of the trials, a neutral baseline condition (e.g., Neely, 1977) was employed, in which the target fragments instead were preceded by a row of Xs ('XXXXX'). It was anticipated that any facilitation provided by the homograph would thus be reflected in enhanced probability of completion of the fragments in the primed condition, relative to this baseline unprimed condition.

Participants were permitted a maximum of six seconds to solve each target word-fragment. This 6 second time limit represented a significant reduction in response interval, as opposed to, for example, the 32 seconds used by Grey and Mathews (2000) in their variant of the fragment-completion task. Under this time constraint some detection of variation in the successful completion*,* as opposed to speed of completion, of fragments by participants, could reasonably be anticipated. Thus, the dependent measure was the percentage of word-fragments that could accurately be solved within

this permitted time. Under these experimental conditions, if these homographs do facilitate the processing of the supposedly related associates, then participants should demonstrate a greater percentage of completions for word-fragments in the primed condition than in the unprimed condition. Further, if the threat and non-threat meanings of the homographs are similarly dominant, then such priming should not differ for threat, relative to non-threat related associates.

Method

Overview

In this study, on equal numbers of trials, the word-fragments were primed or unprimed by preceding homographs, permitting the assessment of the influence of such primes in facilitating responses to the targets. Additionally, for the trials of each prime condition, the valence of the target associate was varied, such that a threat associate was used to create the target fragment in half of the trials, and a non-threat associate in the other half. Thus, the degree of priming provided for targets of each valence was assessed. The dependent variable was the completion score for word-fragments, expressed as the percentage of completions provided for target word-fragments in each condition.

Participants

Participants were 64 first-year psychology students (32 males and 32 females) who participated in the experiment in exchange for course credit. The average age of the participants was 18.8 years (SD = 3.6).

Materials

Experimental hardware.

An Acorn Archimedes 5000 computer and an Acorn AKF 50 colour monitor were employed to present stimuli and record responses. A standard Acorn compatible 3-button mouse, linked to the computer via the serial port, was installed for the registration of participant responses. A standard Acorn-compatible keyboard relayed relevant responses during the fragment-completion priming task.

Lexical stimuli.

As has been described, the stimulus array consisted of 96 *nonets*, where each *nonet* contained a threat/non-threat homograph, four associates of its threatening meaning, and four associates of its non-threatening meaning. From each of these associates, two word-fragments were generated by deleting, on average, 25% of the letters from any relevant word.

Fragment-Completion Priming Task

A computer program in BASIC was developed to control the presentation of the stimuli and the recording of responses in this task. Each trial began with the appearance of three white fixation crosses in the centre of the screen for 1000 ms. The crosses were approximately 5mm wide, separated by spaces of approximately 1mm, and participants were instructed to fixate their gaze upon this central screen location. Following the termination of this fixation display, the screen was blanked for 500 ms. The prime display, a string of yellow characters, was then presented in this same central screen location. On 50% of trials, the prime display was a homograph (primed condition). On the other 50% of trials, this prime display consisted of a set of 5 yellow ‘X’s (unprimed

condition). After 2000 ms, the prime display was blanked, and the target, a white word-fragment, appeared in the same central screen location. On primed trials, this word-fragment was always derived from an associate of the presented homograph prime. Participants were instructed to solve each word-fragment as quickly and accurately as possible. It was emphasised that on trials in which an initial yellow word prime was presented, the solution to the word-fragment would always be related in meaning to this prime, and participants were instructed to use the prime to assist with the fragment's completion. Participants were instructed to press the left mouse button as soon as they were able to solve the word-fragment. On each trial, a maximum of 6 seconds was permitted in which a response could be considered valid. If a button-press was detected, then the participants' task was to select from the keyboard the first missing letter of the presented word-fragment. Participant accuracy in making this selection was recorded, thus providing an indication of capacity to complete the word-fragment. Additionally, the software recorded the latency between the presentation of the word-fragment and the detection of a button press response. If no response was detected within the permitted 6 second response interval, then the trial was scored as incorrect. At the termination of each trial, the screen was then blanked, with the next trial initiated 1000 ms later.

Each participant completed two blocks of trials in this task, with each of the 96 *nonets* providing an associate in each block. On half of these trials in each block, this associate was related to the threatening meaning of the homograph (threat associate condition) and in the other half, to its non-threatening meaning (non-threat associate condition). In each block of trials, half were primed and half were unprimed.
For each participant, the allocation of the threat/non-threat homographs and corresponding associates to prime status and target associate conditions was switched

across the two blocks of trials. Thus, those *nonets* which were assigned to the unprimed condition in the first block were assigned to the primed condition in the second block and vice versa. In addition, those *nonets* assigned to the threat associate condition in the first block were assigned to the non-threat associate condition in the second block, and vice versa.

It was important to ensure that each of the 4 threat associates, and each of the 4 non-threat associates from each different *nonet*, was presented to participants. Therefore, although each participant received only two of the 8 associates (1 threat and 1 non-threat associate) from a particular *nonet*, across participants, each of the 8 associates from each *nonet* was employed. In addition, the allocation of these stimuli to different conditions was rotated such that across every 8 participants, each different associate from the array had provided a word-fragment presented once under primed, and once under unprimed conditions. The counterbalancing achieved by this rotation was handled separately for male and female participants.

Procedure

Participants were tested individually. They undertook the task in a 1.5 m by 1.5 m sound-attenuated testing chamber. They were first seated approximately 50 cm from the computer monitor, then read a verbatim set of instructions on how to perform the fragment-completion priming task (see Appendix IV). After receiving the instructions, each participant completed 10 practice trials in the presence of the experimenter. These practice trials employed primes and associated stimuli not presented during the main priming task, provided in Appendix V. Following the completion of these trials and the correction of any misunderstandings, participants then

completed the two blocks of trials of the fragment-completion priming task as described above. The experimental session then ended.

Results

Completion Scores

The mean percentage of target word-fragments correctly completed under each condition (i.e., completion score) was calculated. These scores, along with corresponding standard deviations, are presented in Table 2.2.

Table 2.2

Mean Completion Scores (%) Obtained In Study 2

	Primed	Unprimed
Non-threat associate	83.1	77.7
	(8.1)	(8.5)
Threat associate	78.8	74.9
	(9.2)	(9.6)

Note. Standard deviations are provided in brackets.

In order to assess the central predictions regarding the threat/non-threat homographs and associates, these completion scores were subjected to a two-way, within-participants ANOVA, in which the factors were target associate (non-threat associate vs. threat associate) and prime status (primed vs. unprimed). This analysis revealed a main effect of target associate, $F(1, 63) = 22.89$, $p<.001$, with participants obtaining a greater percentage of successful completions for word-fragments related to

the non-threat meanings of the homographs, than of word-fragments related to the threat meanings of the homographs (80.4% vs. 76.8%), suggesting that these targets may have been easier to complete, irrespective of prime status.

More central to this study was the presence of a significant main effect of prime status, with participants displaying superior performance for primed, relative to unprimed targets, $F(1, 63) = 39.31$, $p<.001$ (81.0% vs. 76.3%, respectively). This finding is consistent with the expectation that the interpretations imposed upon homograph primes facilitated access to representations of the related associates, thus enhancing the completion of the word-fragment targets. Importantly, this main effect was not modified by the target associate factor, $F(1, 63) = 2.16$, *ns*, thus revealing no evidence that the degree of facilitation provided by these primes differed depending on whether the target was related to the threatening or non-threatening meaning of the homograph prime. Therefore, these results were consistent with the requirement that the meanings of these homographs should not significantly differ in dominance, thus being accessed to comparable degrees, and also facilitating, to degrees that did not significantly differ, the completions of related associates.

Completion Times

Completion times for responding to word-fragment targets derived from the associates, under each prime status condition, are provided in Table 2.3.

Table 2.3

Mean Completion Times (ms) Obtained In Study 2

	Primed	Unprimed
Non-threat associate	2510	2751
	(755)	(807)
Threat associate	2842	3050
	(900)	(941)

Note. Standard deviations are provided in brackets.

Significant findings were not expected to emerge from analyses of completion time data, given that the restricted response interval would likely reduce relevant variation on this measure. However, analyses were performed nonetheless, primarily to eliminate speed-accuracy trade-off explanations for effects that emerged from completion score data[2]. The analyses performed for completion score data were thus paralleled here. Completion times for these target fragments were subjected to a two-way within-participants analysis of variance, in which the factors were target associate (threat associate vs. non-threat associate) and prime status (primed vs. unprimed). This analysis revealed a main effect of target associate, $F(1, 63) = 56.52$, $p<.001$, with participants responding faster to non-threat associates than to threat associates (2630 ms vs. 2946 ms, respectively). However, rather than reflecting a speed-accuracy trade-off, this unexpected main effect was consistent with findings from analyses of completion score data, in suggesting a lower degree of difficulty in

[2] Visual inspection revealed that the distribution of completion times was positively skewed. In order to correct for the impact of this skewness upon the outcome of analyses, these data were therefore subjected to log transformation. Analyses of corrected data revealed equivalent patterns of findings in all cases.

completing these non-threat fragments. More centrally, there was a significant main effect of prime status, $F(1, 63) = 12.89$, $p<.05$, with participants displaying shorter completion times for primed trials relative to unprimed trials (2676 ms vs. 2900 ms, respectively). This effect was also consistent with analyses from completion score data, and bolsters the conclusion that these threat/non-threat homograph primes indeed facilitate access to the lexical representations of related targets of each valence. The interaction of prime status by target associate was not significant, $F(1, 63) = .119$, *ns*.

Discussion

This study aimed to empirically validate the selection of the associates grouped with the threat/non-threat homographs by investigating whether the processing of target stimuli derived from these associates was primed by the homographs. Analysis of target completion scores and completion times revealed that, relative to a baseline unprimed condition, more targets were completed, and these were completed faster, when preceded by threat/non-threat homographs. Thus, the processing of these associates may indeed be facilitated by their semantic relationship to the threat/non-threat homographs. Across participants, on average, this priming effect did not differ for threat relative to non-threat associates, consistent with the requirement that that each homograph suggest alternative meanings of comparable accessibility, with comparable degrees of relation to associates of each valence.

Thus, these findings were consistent with the requirement that access to the representations of these threat and non-threat related associates be semantically facilitated by corresponding, related homographs, and that such facilitation should occur to comparable degrees for associates related to each valence of homograph meaning.

These findings thereby fulfilled the second major goal of this stimulus development work, and as such, it was thus deemed appropriate that these associates be employed, along with the homographs, in the interpretive training and assessment tasks of this thesis.

CHAPTER 3

DEVELOPMENT AND VALIDATION OF INTERPRETIVE TRAINING AND ASSESSMENT TASKS USING THE FRAGMENT-COMPLETION METHODOLOGY

The set of stimuli constructed in Studies 1 and 2 included 96 homographs selected to permit threat-congruent and non-threat-congruent interpretations which on average were shown to be similarly probable. These homographs were also shown to facilitate the processing of word-fragments derived from the related associates included within this stimulus set, using a fragment-completion priming task. This chapter describes the employment of these stimuli within an interpretive training task, created by introducing training contingencies into this same fragment-completion task.

Study 3

The main aim of the present study was to design and validate an interpretive training task. This task was created by the introduction of training contingencies into the fragment-completion priming task, following the general approach described in the introduction (eg., Grey & Mathews, 2000), and using the stimuli developed in Chapter 2. The central question was whether such training would, as hypothesised, prove successful in inducing a group difference in interpretive bias, or whether it would simply induce a group difference in the capacity to process valenced targets, without actually modifying interpretive bias at all. In Chapter 2, the chosen threat/non-threat homographs appeared to facilitate the processing of the selected associates within a fragment-completion task similar to the one used by Grey and Mathews (2000). In this

present study, following the Grey and Mathews' approach, training contingencies were introduced into this task. Thus, for one group of participants, the word-fragments received in training were always derived from associates related to the threatening meanings of corresponding threat/non-threat homograph primes. For this group it was expected that the introduction of this training contingency would induce a tendency to interpret ambiguous homographs in a threat-congruent fashion. For the other group of participants, the word-fragments were always derived from associates related to the non-threatening meanings of corresponding threat/non-threat homograph primes. Thus, for this group, it was expected that the training contingency would induce a tendency to interpret ambiguous homographs in a non-threat congruent fashion.

The success of this training procedure, in modifying interpretive bias, was investigated using assessment trials involving the same fragment-completion task but across which the training contingency did not apply. As in the previous study, the primary dependent measure was capacity to provide a completion for each relevant word-fragment within a 6 six second time-limit.

The approach to assessment in this study differed in three important respects from the approach previously adopted in Grey and Mathews' (2000, Experiment 1) study. Firstly, in the present study, a pre-training interpretive assessment phase was introduced in order to exclude the possibility of pre-existing group differences in interpretive bias. Secondly, the valence of word-fragment targets was treated as a within-participants, rather than as a between-groups factor. Thus, in the present study, during the interpretive assessment phases, all participants received word-fragments related to the threatening, as well as to the non-threatening meanings of the homograph primes. Thirdly, and most importantly, this methodology included the unprimed

condition, introduced in Study 2 within the tasks, whereas Grey and Mathews included no such condition. Within this study, these unprimed trials served a critical purpose. In the interpretive assessment phases, they provided a baseline condition under which any induced group difference in interpretive bias would not influence performance. Thus, a group difference in the processing of target associates related to differentially valenced prime meanings, rather than in interpretation of the homograph primes themselves, would be equally evident under both primed and unprimed conditions. In contrast, if training indeed induced a group difference in interpretive bias, then this would be revealed by a relative enhancement in the processing of non-threat fragment completions, relative to threat completions, for the non-threat training group, relative to the threat training group, *only* in the primed condition, and not in the unprimed condition.

Method

Overview

This study included a pre-training interpretive assessment phase, an interpretive training phase, and a post-training interpretive assessment phase. During the interpretive training phase, participants were assigned to the threat training group or to the non-threat training group, which determined the valence of the word-fragments received. An interpretive assessment phase was included prior to the interpretive training phase to exclude the possibility of pre-existing group differences in processing. An interpretive assessment phase was also included following the interpretive training, to assess the influence of the training procedure. In each interpretive assessment phase, participants completed equal numbers of trials in which the word-fragment target was primed or

unprimed by a corresponding homograph (primed vs. unprimed conditions), and equal numbers of trials in which the target was related to the threatening or non-threatening meaning of its corresponding homograph (threat associate vs. non-threat associate conditions). Thus, the existence of a group difference in interpretation of the primes, in each case, could be distinguished from a group difference in the capacity simply to complete the word-fragments. The principal dependent measure was the percentage of word-fragments within each relevant assessment condition completed by participants.

Participants

Participants were 32 male and 32 female first-year undergraduate students at the University of Western Australia (UWA).There were 16 male and 16 female participants within each training group. The participants were unselected, apart from the constraint that the training groups be gender-balanced. Additionally, these training groups did not significantly differ in mean age (18.7 years, SD= 1.9 for the non-threat training group; 18.6 years, SD = 2.0, for the threat training group), $F(1, 62) = .04$, *ns*.

Materials

Lexical stimuli.

The 96 *nonets* comprising the array of threat/non-threat homographs and associates, as well as the word-fragments derived from each associate, were employed in the present study (see Chapter 2 for details of these stimuli). The practice stimuli were also the same as those employed in the previous experiment.

Experimental hardware.

The hardware specifications in this fragment-completion task were the same as for Study 2 (see Chapter 2).

Experimental Task

The main experimental task was delivered across three phases in the experiment: a pre-training interpretive assessment phase; an interpretive training phase; and a post-training interpretive assessment phase. In each phase, the details of prime and target presentation and task requirements were identical to those employed in Study 2. To reiterate, on each trial, following the presentation of the fixation stimulus, a yellow prime display then appeared, replaced 2000 ms later by a white word-fragment target. As in the previous task, in 50% of trials (primed trials), the prime display was a homograph, related in meaning to the subsequent target, and in the remaining 50% of trials (unprimed trials), this display consisted in a row of 'X's ('XXXXX'). Each participant's task was to solve the word-fragment, using the prime as a cue when present. The participant pressed the left mouse button when s/he was able to identify the word-fragment, then indicated its solution by selecting the first missing letter of the word-fragment from the keyboard.

There were 6 blocks of trials in total and 48 *nonets* from the array of threat/non-threat homographs and associates were employed in each block. The first and the final blocks comprised, respectively, the pre-training and post-training interpretive assessment phases. These two blocks were separated by four blocks of trials comprising the interpretive training phase. Half of the trials in each block were primed, and half were unprimed. Relevant differences across the three phases are described below.

Interpretive training phase: Fragment-completion interpretive training task.

In the interpretive training phase, a contingency was introduced, such that for those participants assigned to the threat training group, the word-fragments presented in this phase were always related to the threatening meanings of the homographs in the *nonets* from which they were drawn, with the opposite being the case for the non-threat training group. Thus, for each group, on primed trials, it would be advantageous to impose one valence of interpretation upon presented threat/non-threat homographs, to gain benefit in solving the subsequently presented fragments. It was intended that, across trials, the two training contingencies would come to induce threat or non-threat-congruent interpretive biases, respectively.

For any participant, a particular selection of 48 *nonets* from the array of threat/non-threat homographs and associates was assigned to the interpretive training phase. In each of the four blocks of training trials, for each relevant *nonet*, a different, appropriately valenced word-fragment target, derived from one of the four appropriately valenced associates, was presented. The order of presentation of these stimuli within each block was randomised.

Across participants, the allocation of stimuli to training was rotated, such that within each training group, all appropriately valenced fragments from the array of threat/non-threat homographs and associates were presented equally often in each training block, and equally often under primed and unprimed conditions.

Pre & post-training interpretive assessment phases: Fragment-completion interpretive assessment task.

The training contingency introduced into the interpretive training phase was not present within the interpretive assessment phases, and so the word-fragments derived from associates of each valence appeared with equal frequency. These targets equally often either appeared primed by corresponding threat/non-threat homographs, or appeared in the unprimed condition.

For each participant, each interpretive assessment phase employed the 48 *nonets* from the array of threat/non-threat homograph and associates not presented during training. Across interpretive assessment phases, any *nonet* which provided a threat associate in the first phase provided a non-threat associate in the second. Also, any *nonet* for which an associate appeared in the primed condition in the first phase, then provided an associate which appeared in the unprimed condition in the second phase. The order of presentation of stimuli within each block of assessment trials was randomised.

Across participants, the allocation of threat/non-threat homographs and targets to the various assessment conditions was rotated. Across all participants, each of the 4 associates of each valence from each *nonet* had been used to create a target word-fragment in each assessment, an equal number of times in the primed condition and in the unprimed condition.

Finally, the allocation of the stimuli to either the training phase or the interpretive assessment phases was also rotated within each training group, such that all of the *nonets* were assigned equally often to the training phase or to the different interpretive assessment conditions.

Procedure

All participants were tested individually, in a testing chamber which contained the computer equipment. Participants were provided with verbatim instructions and 10 practice trials as in Study 2 (see Appendices IV and V). Each participant then completed the pre-training interpretive assessment phase, the interpretive training phase, and the post-training interpretive assessment phase. Half way through the training, each participant was allowed a self-timed break. Following the completion of the final assessment trials, the participant was then debriefed regarding the aims of the experiment, and the experimental session concluded.

Results

Completion Scores

For each participant, mean completion scores were calculated for each of the assessment conditions. Mean scores for each condition and associated standard deviations are provided in Table 3.1.

Analyses of pre-training completion scores.

Prior to assessing the central empirical hypothesis regarding the influence of the training upon interpretive bias, it was first important to ensure that the groups did not differ in interpretive bias from the outset. Thus, analyses of scores obtained in pre-training interpretive assessment phase trials were carried out first. Completion score data for threat and non-threat associates, on primed and unprimed trials, were compared in 2-way mixed design ANOVAs, involving training group (threat training group vs. non-threat training group) and target associate (threat associate vs. non-threat associate) as factors. Most importantly, these analyses revealed no evidence of a training group

difference in the capacity to complete threat, relative to non-threat word fragments, on primed trials, $F(1, 62) = 0.22$, *ns*, or on unprimed trials, $F(1, 62) = 0.05$, *ns*, thus providing no evidence that the groups differed in interpretive bias, or simply in the capacity to complete valenced targets, prior to the training. In fact, the only significant effect reflected an enhanced capacity to complete targets derived from non-threat, relative to threat associates, on primed trials (82.3% vs. 74.7%, for non-threat and threat associates, respectively), $F(1, 62) = 11.40$, $p<.01$. This suggests that, prior to training, participants were, on average, more likely to interpret the homographs in a non-threat-congruent fashion. However, as this effect was not significantly modified by training group, it is of no further relevance to the purposes of this study. No other effects were significant (all Fs<1).

Analyses of post-training completion scores.

The central hypothesis under empirical scrutiny was that the introduction of the training contingency would lead to a congruent group difference in interpretive bias, as distinguished from the alternative possibility that the training might simply induce a group difference in the capacity to process valenced targets. Thus, it was anticipated that the threat training group, relative to the non-threat training group, would display enhanced completion of fragments derived threat associates, relative to fragments derived from non-threat associates, on primed, but not on unprimed trials. Completion scores from the primed and unprimed trials, for threat and non-threat associates, were thus contrasted as for pre-training interpretive assessment trials, using 2-way mixed design ANOVAs. Most importantly, the training groups did not differ significantly in completion of fragments derived from threat, relative to non-threat associates, on

Table 3.1

Mean Completion Scores (%) Obtained In Study 3

	Pre-training		Post-training	
	Non-threat associate	Threat Associate	Non-threat associate	Threat associate
		Unprimed		
Non-threat training group	74.2 (13.8)	69.5 (15.1)	75.0 (13.9)	76.6 (17.4)
		Primed		
	81.0 (12.9)	74.5 (16.5)	77.1 (14.8)	77.6 (15.9)
		Unprimed		
Threat training group	75.8 (14.4)	70.0 (19.6)	71.1 (17.2)	77.6 (15.0)
		Primed		
	83.6 (10.5)	75.0 (14.6)	76.3 (14.8)	76.8 (14.6)

Note. Standard deviations are provided in brackets.

unprimed trials, $F(1, 62) = 1.06$, *ns*, but also did not differ in their capacity to complete fragments derived from threat, relative to non-threat associates on primed trials, $F(1, 62) = 0$, *ns*. Thus, while there was no evidence for the alternative possibility, that the training might simply have induced a group difference in the capacity to complete targets derived from associates of each valence, there also was no evidence that this

present training procedure served to induce a group difference in interpretive bias. No other significant effects were revealed (all other *F*s<1, except for the non-significant main effect of the target associate factor in primed trials, $F(1, 62) = 2.82$, *ns*).

Completion Times

Completion times for each interpretive assessment phase, for word-fragments in each condition, are provided in Table 3.2. As with Study 2, completion scores were considered the primary metric of performance in this study. The completion time analyses were thus of supplementary interest, and because the completion score analyses did not reveal the anticipated training congruent effects, these current analyses served primarily to rule out possible inconsistencies with these null conclusions in completion time data. To parallel completion score analyses, completion time analyses of the data in the pre-training and post-training interpretive assessment phases are provided separately[3].

[3] As for previous reaction times, in order to correct for the influence of positive skewness upon the outcomes of completion time analyses, these data were subjected to log transformation. Analyses of transformed data revealed the equivalent pattern of findings in all cases.

Table 3.2

Mean Completion Times (ms) Obtained In Study 3

	Pre-training		Post-training	
	Non-threat associate	Threat associate	Non-threat associate	Threat associate
		Unprimed		
Non-threat training group	2094 (1185)	2389 (1378)	1942 (1165)	2105 (1504)
		Primed		
	1721 (802)	2118 (1344)	1919 (1084)	1937 (1267)
		Unprimed		
Threat training group	2112 (1195)	2494 (1614)	2275 (1407)	1988 (1296)
		Primed		
	1606 (602)	1982 (1153)	1762 (1071)	1919 (1244)

Note. Standard deviations are provided in brackets.

Analyses of pre-training completion times.

As for completion score data, completion time data from the pre-training interpretive assessment phase were compared using 2-way mixed-design ANOVAs, again involving training group (non-threat training group vs. threat training group) and target associate (threat associate vs. non-threat associate). Most centrally once again,

these analyses revealed that the training groups did not differ in the completion of threat, relative to non-threat associates, for unprimed trials $F(1, 62) = 0.05$, *ns*, or for primed trials, $F(1, 62) = 0$, *ns*. In fact, only one significant effect emerged in these analyses, a main effect of target associate for word-fragments primed by threat/non-threat homographs, $F(1, 62) = 6.18$, $p<.05$, with participants, on average, faster to complete non-threat than threat associates, following homograph primes (1663 ms vs. 2050 ms, for non-threat associates and threat associates, respectively). Though not predicted, this finding is consistent with results from completion score analyses, in suggesting an average tendency to impose non-threat-congruent interpretations upon these emotionally ambiguous primes prior to training. However, once again, because this main effect was not modified by training group, it has no further bearing upon the central experimental hypotheses under investigation. No other effects were significant (All *Fs*<1 except for the non-significant main effect of target associate for unprimed trials, $F(1, 62) = 2.95$, *ns*).

Analyses of post-training completion times.

The completion times for each condition from the post-training interpretive assessment phase were next analysed for the same factors. In these scores, as for completion scores, once again, the training groups displayed no significant difference in time taken to complete threat, relative to non-threat associates, on unprimed trials, $F(1, 62) = 1.65$, *ns,* or on primed trials, $F(1, 62) = 0.32$, *ns.* No other significant effects were revealed (all other *Fs*<1). Thus, consistent with completion score data, these completion time analyses provided no indication of a group difference in interpretive bias following the training.

Discussion

In this study, a variant of the fragment-completion task was designed to induce differential interpretive biases amongst two groups of participants. However, the data suggest that this training procedure did not induce such a group difference in interpretive bias effectively. Contrary to predictions, analyses of word-fragment completions did not reveal any post-training group difference in relative ability to complete word-fragments consistent with the threatening meanings, and those consistent with the non-threatening meanings, of preceding homograph primes.

The failure to induce a group difference in interpretive bias in this present study raises the need to analyse the characteristics of this training methodology which may have compromised its efficacy. These characteristics may be considered by comparing the present methodology to the training approach employed by Grey and Mathews (2000), and to priming tasks carried out in previous studies. There are at least three potential explanations for the present failure to obtain the predicted training effects: (i) poorly perceived relationship between primes and targets; (ii) over-familiarity with requirements of the assessment task; and (iii) influence of other aspects of prime and target presentation upon usefulness of primes. These explanations may not be mutually exclusive. Nevertheless, each is considered in turn in the discussion that follows. Additionally, in each case, some consideration is given to possible means for overcoming such potential problems.

(i) *Poorly Perceived Relationship Between Primes And Targets*

The apprehension, by participants, of the capacity for the homographs to facilitate the processing of related targets, is likely to be a requirement for effective

training. It is possible that at least one feature of the present methodology may have compromised the degree to which the helpful relationship between the homographs and targets was perceived by participants. The present study diverged from the methodology of Grey and Mathews (2000) by incorporating a large number of unprimed trials, whereas Grey and Mathews included none. This feature was instantiated within interpretive assessment phases in order to exclude an alternative hypothesis regarding training effects, but was maintained within interpretive training phases simply for consistency. It is possible that the inclusion of these unprimed trials, in particular during training, may have reduced the extent to which participants made use of primes to help solve the fragments.

This suggestion can be supported by surveying the outcomes of various priming studies in the broader priming literature, in which researchers have varied, as a proportion of the total number of trials, the number of trials in which a target is preceded by a related prime (e.g., de Groot, 1984; den Heyer, 1985; Seidenberg, Waters, Sanders, & Langer, 1984**;** Stolz & Neely, 1995). At prime-target SOAs exceeding 300 msec, the proportion of such related targets is robustly and directly associated with the degree to which priming takes place.

As noted, half of the trials in the present study were unprimed, meaning that a related prime preceded targets on only half of the trials. The proportion of related trials may have been further reduced by the introduction of the training contingency, in interaction with this unprimed condition. Suppose, for example, that an individual without an apparent interpretive bias was assigned to the threat training group. This individual might, at least initially, have tended to interpret the threat/non-threat homographs in a threat-congruent fashion in only 50% of the trials in which these

homographs were presented. Thus, given the use of unprimed trials also, the targets would have been perceived to be related to the preceding homographs on only 25% of trials. It is possible that when perceived likelihood of prime-target relatedness is excessively low, this will reduce the tendency to employ the primes to help solve the fragments. This would reduce the likelihood of training effects and would compromise the capacity to detect them.

This, if correct, would mean that priming effects would reduce across the session, as homographs are found to be unhelpful in solving targets, and so become ignored. Consistent with this suggestion, priming effects were detected prior to training, but not in the post-training interpretive assessment phase. Specifically, during the pre-training interpretive assessment phase, participants displayed significant priming effects (78.5% vs. 72.4%, for primed and unprimed conditions, respectively), $F(1, 62) = 14.04$, $p<.001$. In contrast, during the post-training interpretive assessment phase, there was no evidence of significant priming (77.0% vs. 75.1%, for primed and unprimed conditions, respectively), $F(1, 62) = 1.39$, *ns*. These analyses are certainly consistent then, with a reduction in priming across the course of the study.

Two methodological adjustments could help to remedy this possible problem. Most obviously, the removal of the unprimed condition from the training trials would seem appropriate, especially given its redundant role within the interpretive training phase. Furthermore, within the interpretive assessment phase, instead of the unprimed condition, it might be possible to introduce an alternative baseline condition which does retain a high degree of prime-target relatedness.

(ii) *Over-Familiarity With Requirements Of The Assessment Task*

The absence of the priming effects, post-training, noted in (i), may also be amenable to another related explanation: It is possible that across the course of training, participants became adept at completing word-fragments, such that, following training, the primes were no longer able to provide a detectable degree of facilitation. As a consequence, the subsequent interpretive assessment phase might then have failed to detect a successfully induced group difference in interpretive bias.

There are a number of ways in which participants could have become skilled at fragment-completion. For example, learning to complete word-fragments could occur by memorisation of sub-lexical components of stimuli, such as particular syllables which occur across words (e.g., Light, La Voie, & Kennison, 1996; Murrell & Morton, 1974; but see also Dorffman, 1999), thus facilitating fragment completion insofar as it may operate on this sub-lexical basis. Similarly, participants might have honed a strategy whereby fragment completions were suggested on the basis of letter-orders which are phonologically plausible. Such possibilities would be consistent with evidence suggesting the operation of a sub-lexical pathway in the production of word spellings (e.g., Folk, Rapp, & Goldrick, 2002; Goodman & Caramazza, 1986). The reason why participants might have become overly skilled in fragment-completion in this present task, and not in Study 2, could simply be the larger number of trials required to incorporate interpretive training, as well as assessment, into the task framework.

An appropriate solution to this potential problem would be to introduce a novel task to assess interpretive bias, different enough from the task employed in training to

preclude such task specific learning effects. For example, Grey and Mathew's (2000) relatedness-judgment task (see Chapter 1) could be employed.

(iii) *Influence Of Other Aspects Of Prime And Target Presentation Upon Usefulness Of Primes*

The present priming task was adapted directly from the task validated in Study 2, which was itself based upon the conventions of a large body of previous studies (e.g. Neely, 1977). However, it differed from Grey and Mathews' (2001, Experiment 1) study, with regard to some specifics of prime and target presentation. It could be then, that these differences meant that this variant of the task was less effective than Grey and Mathew's procedure for the induction of group differences in interpretive bias, or for the sensitive detection of such induced bias. There are two particular differences between these present training and assessment tasks and the variants employed by Grey and Mathews which may be relevant. These concern temporal and spatial aspects of stimulus presentation.

Firstly, the prime-target SOA in the present study was 2000 ms, as opposed to the 750 ms employed in Grey and Mathews' (2000) studies. This may be relevant, given the typical time-course of activation for the alternative meanings of homographs (e.g., Simpson & Burgess, 1985; Simpson & Krueger, 1991). Relevant studies typically suggest that both meanings are activated, but that the subordinate meaning tends to be activated more slowly than the dominant meaning. One possible implication of these observations is that both meanings of these homographs may already have been accessed by the time at which targets were received in this present study, thus reducing the degree of facilitation provided by, say, initially having accessed one meaning more

rapidly because of the training manipulation. This possibility could thus potentially limit the effectiveness of the training as well as compromising the sensitivity of the assessment task variant.

A second potentially relevant characteristic of this study is that the target replaced the prime, rather than appearing below it, as was the case in Grey and Mathews' (2000) task. This aspect of trial presentation could have made it more difficult for participants to use the primes in solving the fragments by introducing the additional requirement that the participants hold the primes in memory while simultaneously processing the targets, and could thereby have discouraged their use.

Of course, the obvious approach to eliminating such possible problems would be to adopt the parameters employed by Grey and Mathews (2000), by using a 750 ms interval, rather than 2000 ms, and by having the target appear below the prime, rather than replacing it.

Conclusion

In summary, in Study 3, a modified fragment-completion task failed to induce a detectable group difference in interpretive bias. A number of explanations for this failure have been suggested, relating specifically to a potentially low level of perceived relatedness between primes and targets, to participants becoming overly skilled in fragment-completion, or to the parameters concerning prime and target presentation. Possible methodological refinements to address each of these issues have been provided, and these were in fact instantiated in the next attempt to induce group difference in interpretive bias, described in Chapter 4.

CHAPTER 4

MODIFICATION OF THE FRAGMENT-COMPLETION TRAINING TASK AND VALIDATION USING A RELATEDNESS-JUDGEMENT ASSESSMENT TASK

In Chapter 3, three explanations were offered for the failure of the developed interpretive training task to induce a group difference in interpretive bias. Each explanation provided suggestion for modification of the interpretive training and/or assessment task. As discussed below, each of these modifications was incorporated into the tasks used in Study 4. This study thus represented a second attempt to meet the Phase 1 goal of developing a task capable of inducing a group difference in interpretive bias.

Task Modifications Introduced In Study 4

There were three major modifications made to the interpretive training and/or assessment methodologies in this study: (i) modification of the unprimed condition and filler stimuli; (ii) modification of the interpretive assessment task; and (iii) modification of spatial and temporal parameters of prime and target onset. Each of these was made in response to the problems suggested to be associated with the design of the previous training and assessment procedures, and each is discussed in turn, below.

(i) *Modification Of Unprimed Condition And Filler Stimuli*

Modifications were made in light of the possibility that a low degree of perceived relatedness between primes and targets might have reduced the effectiveness

of the training and/or the sensitivity of the assessment. In particular, the low number of primed trials was identified as of potential relevance to this issue. Therefore, a major modification was made to the methodology employed in the present study: The unprimed trials were removed from interpretive training and assessment phases, thus effectively doubling the probability that targets would be preceded by primes with some degree of perceived relatedness.

Of course, the unprimed trials were originally included with an explicit functional purpose. Specifically, the purpose of these trials was to provide a baseline measure of the ease of processing valenced targets, where, because no ambiguous primes are presented, interpretive biases cannot be applied. So a functional substitute for this aspect of the design was required. However, it was important that this substitute be likely to enhance, rather than to reduce, the degree of perceived prime-target relatedness.

The adopted solution was to include unambiguous primes and associated targets within both the interpretive training and interpretive assessment phases. During training, these primes could be expected to increase the proportion of trials for which a prime would be perceived as related to the target, thereby enhancing the probability of successful interpretive training. That is, homograph primes would be seen to be related to targets only when interpreted in a way consistent with the introduced training contingency, whereas these new unambiguous primes would always be seen to be related to the targets. In the present interpretive training task, each training group therefore received a number of trials in which the primes were unambiguous, but of a valence congruent with the received training condition. During the interpretive assessment phases, an equal number of threatening and non-threatening unambiguous

primes, paired with related targets, were included together with the ambiguous primes and related targets. As with training, the inclusion of these stimuli in assessment was expected to enhance the perceived relatedness of primes and targets, thereby maximising the possibility that the primes would be used to help solve the target fragments. Thus, this would enhance the prospect of detecting an induced group difference in interpretive bias.

In discussing the interpretive training phase of the Study 3 (see Chapter 3), it was estimated that, with the unprimed trials taken into consideration, unbiased participants could be expected to perceive the primes to be related to targets on only 25% of trials. This is because the primes were only presented on the primed 50% of trials, and might only have been interpreted in a fashion consistent with the associated targets in 50% of these primed trials. In the interpretive training used in Study 4, the threat/non-threat homographs now appeared on 80% of trials and the unambiguous primes on 20%. Thus, even if the ambiguous primes were interpreted in a manner inconsistent with the provided associates on 50% of relevant trials, participants would still perceive a relationship between primes and targets on 60% of the total number of training trials. This modification thus more than doubled the minimum anticipated degree of perceived relatedness between primes and targets.

More important for the interpretive assessment phases, these targets filled the role previously provided by the unprimed trials. Thus, these unambiguous prime trials permitted the assessment of the degree to which training induced a group difference in the ease of processing valenced associates of primes, under conditions in which there was no requirement for differential interpretation of the primes themselves.

(ii) *Modification Of The Interpretive Assessment Task*

A second, related explanation for the failure of Study 3 to induce a detectable group difference in processing is that participants may have become overly practised at the completion of the fragment-completion task, such that the homograph primes no longer provided any assistance. Thus, induced group differences in interpretive bias might not have been detected during the post-training interpretive assessment phase. Another major modification of the methodology therefore involved employing a different task, not involving fragment-completion, during the interpretive assessment phases. This was intended to eliminate the influence of such task-specific practice effects. The employed interpretive assessment task was a variant of the relatedness-judgement task (Grey & Mathews, 2000, Experiment 3), described in Chapter 1.

(iii) *Modification Of Spatial And Temporal Parameters Of Prime And Target Onset*

The discussion in Chapter 3 drew attention to features of prime and target presentation which differed between the methodology of Study 3 and that of Grey and Mathews (2000, Experiment 1). In the present Study 3, the prime was replaced by the target at the end of a 2000 ms SOA. In Grey and Mathews' version of the task, the prime appeared below and in addition to the target, which itself remained onscreen after an SOA of 750 ms. In Chapter 3, it was speculated that the longer SOA employed in the present Study 3 could have compromised the inducement and/or the detection of an induced interpretive bias. With regard to training, this problem may have arisen because the degree of facilitation provided by the act of initially accessing the appropriate meaning of each homograph prime was lessened at a longer SOA, by which point each meaning could have been consciously accessed. Similarly, the longer SOA could have

reduced, during the interpretive assessment phases, the capacity to distinguish between variations in facilitation from different prime meanings. Study 4 therefore used the prime and target presentation conditions and SOA employed by Grey and Mathews.

Study 4

The main aim of this study was to examine the efficacy of the modified fragment-completion interpretive training task, as assessed using a different, relatedness-judgment interpretive assessment task. The modified fragment-completion interpretive training task once again involved a contingency designed to induce a group difference in interpretive bias. For the threat training group, primes presented during training were followed by word-fragments derived from associates of their threatening meanings. For the non-threat training group, the associates from which the targets were derived were always related to the non-threatening meanings of the primes.

Once again, the training was preceded and followed by interpretive assessment phases. However, these phases now involved a task quite different from that employed in Study 3, instead providing a relatedness-judgment measure of interpretive bias. In this modified interpretive assessment task, following the presentation of the prime, the participant was presented with a target display consisting of two words. One of these words, the target associate, was an associate of the prime. The other was a foil, an unrelated word of the same valence. The participant was required to indicate with a button press which of the two words was related to the prime. The latency prior to their making this selection was recorded. There were four central experimental conditions instantiated in these relatedness-judgment interpretive assessments. On half of the assessment trials, the primes were threat/non-threat homographs, which, across trials,

were followed with equal frequency by associates related either to their threatening or to their non-threatening meanings. On the other half of the trials, the primes were unambiguous, and were threatening or non-threatening with equal frequency. The target display that followed each unambiguous prime always included an associate related to the valenced prime's meaning. The unambiguous trials thus permitted an assessment of the ease of selecting associates of each possible valence when the requirement to interpret an emotionally ambiguous prime was removed. The relatedness-judgment task was designed in a manner intended to permit participants to perform with a consistently high degree of accuracy. The participant's response latency to select the related associate was therefore considered to be the principal measure of performance.

Method

Overview

This study once again consisted of three phases: A pre-training interpretive assessment phase, an interpretive training phase, and a post-training interpretive assessment phase. Once again, participants were assigned to threat training and non-threat training groups, with the grouping determining the valence of the received training.

During the interpretive assessment phases, participants completed equal numbers of trials in which the prime was a threat/non-threat homograph or an unambiguous, valenced word. The homograph primes were followed equally often, in different trials, by target associates related to their threatening or non-threatening meanings (threat associate and non-threat associate conditions). Similarly, the unambiguous primes of each valence were always followed by related target associates.

Thus, the examination of selection latencies for trials across these different conditions enabled the assessment of group differences in interpretive bias, as opposed to group differences in capacity to select associates of differentially valenced prime meanings in the absence of any ambiguity. Furthermore, these differences could be assessed both prior to and following the interpretive training phase.

Participants

Participants in this study were 48 first-year psychology students who participated in exchange for course credit. There were 24 participants in each training group, with 14 female and 10 male participants in the threat training group, and 16 female and 8 male participants in the non-threat training group. The mean age of participants was 17.8 years (SD = 1.2) for those assigned to the non-threat training group, and 18.8 years (SD = 4.1) for those assigned to the threat training group. These mean scores did not significantly differ, $F(1, 46) = 1.64$, *ns*.

Materials

Lexical stimuli.

The lexical stimuli employed in this study represented an extension of those employed in Study 3. The earlier set of threat/non-threat homographs and associates was retained, consisting in the same 96 *nonets*, as well as word-fragments corresponding to associates in these *nonets*. Unambiguous valenced primes and associates were now also required, and the source of these stimuli was determined by the different requirements of the interpretive training and interpretive assessment phases within which they were used. The most important requirement for the interpretive

assessment phases was that the unambiguous and ambiguous prime trials should be equivalent in all respects other than the ambiguity of the primes themselves. Therefore, the unambiguous prime trials in this phase involved unambiguous valenced stimuli cannibalised from the associates in the original nonets. Thus, a threat and non-threat associate from each of the 96 *nonets* were assigned to serve as the threat and non-threat unambiguous primes within assessment trials and a different 2 associates from each *nonet* were assigned to serve as the threat and non-threat target associates.

Within training, it was important to provide the maximum possible number of training trials involving different associates of the appropriate valence, paired with corresponding homograph primes. This limited the availability of stimuli from the original *nonets* which could be employed as additional unambiguous primes and associates in training. Therefore, an additional 32 unambiguous threatening primes and 32 unambiguous non-threatening primes, each paired with a related associated, were generated for use in the interpretive training phase (see Appendices VI and VII).

Computer hardware.

Stimuli were presented and responses recorded using an Acorn Archimedes Microcomputer with a 15-inch colour monitor. A keyboard relayed responses during the fragment-completion interpretive training task. A three-button mouse, connected through the serial mouse port, relayed responses during the relatedness-judgment interpretive assessment tasks.

Experimental Tasks

Interpretive training phase: Fragment-completion interpretive training task.

The interpretive training phase employed a modified version of the fragment-completion task used in the previous two studies. Each trial commenced with the central presentation of the 3 white fixation crosses for 1000 ms, following which the screen was blanked for 500 ms. The yellow prime display then appeared, 13 cm from the side of the screen and 0.5cm above the fixation point. The target word-fragment then appeared 750 ms later, 13 cm from the side of the screen and 0.5cm below the fixation point. As before, the participant was instructed that the solution to the word-fragment was related in meaning to the prime, and that the prime should be used to help solve this word-fragment as quickly and accurately as possible. The participant was required to press the spacebar as soon as he or she was able to solve the word-fragment. If the spacebar was pressed within the 6 sec time limit, then, as with previous variants of this task, the participant was then required to provide the first missing letter of the word-fragment. The accuracy of the participant's response was recorded.

There were thus two important features of task appearance by which this variant diverged from the task employed in Studies 1 and 2. Firstly, the prime now remained on screen following the presentation of the target display, instead of being blanked prior to the appearance of the target fragment. The second important difference was that the SOA separating these events was reduced from 2000 ms to 750 ms. This training variant also differed from the previous training variant by eliminating the large number of unprimed trials. Thus, the prime was always related in meaning to the target.

In 80% of the trials, the prime was a threat/non-threat homograph and in the remaining 20% it was an unambiguous (threatening or non-threatening) word, whose

valence was congruent with training group assignment. As in the previous study, the training contingency was such that, for the threat training group, the word-fragment targets which followed each kind of prime were always threat-related, and for the non-threat training group, always non-threat related. Thus, for both training groups, employing a particular (threat or non-threat-congruent) interpretive bias meant that primes would provide assistance in completing the related word-fragments for the 80% of trials which involved the homograph primes.

Therefore, compared to Study 3, the stimulus presentation conditions used for training in this study were likely to sustain a tendency to employ the primes to assist the processing of targets, since, with the appropriate interpretive bias, it would be advantageous to employ the primes on 100% of trials (including unambiguous prime trials), as opposed the primed 50% of trials in the previous study. Even for an individual who was unbiased in their interpretations of the homographs, the inclusion of unambiguous primes meant that the primes could still be expected to provide facilitation on 60% of trials, as opposed to 25% in Study 3. For this individual, the adoption of a particular interpretive bias, consistent with training group assignment, would enhance the degree to which such primes could potentially provide facilitation, from 60%, up to as much as 100% of the time.

The interpretive training phase consisted of four blocks of 40 trials. Each block was comprised of 32 trials involving *nonets* from the array of threat/non-threat homographs and associates, as well as 8 trials involving unambiguous primes and associated stimuli from the additional array of stimuli created for this purpose. Within these blocks, the trials were then sorted into groups of 10, with eight threat/non-threat

homograph primes, and two unambiguous primes appearing within each 10, with order randomised beyond these constraints.

For each participant, a particular set of 32 *nonets* was assigned to the interpretive training phase, and each homograph from this *nonet* appeared in each of the four different training blocks paired with a different, related associate of the appropriate valence. Within each training group, the allocation of stimuli to the interpretive training phase and to different training blocks was rotated such that, across all participants within the training group, all homographs and associates of the appropriate valence were presented an equal number of times within each block of the interpretive training phase. Similarly, across participants, the unambiguous primes of the appropriate valence were allocated equally often to the different interpretive training blocks.

Interpretive assessment phases: Relatedness-judgment interpretive assessment task.

The presentation characteristics of this task essentially followed those of the fragment-completion interpretive training task until the appearance of the target display. Thus, each trial began with the appearance of the 3 white fixation crosses in the central screen location for 1 second. The screen was then blanked for 500 ms, and the prime display then appeared in yellow font, centrally, 0.5 cm above the central screen location. The target display then appeared centrally, 750 ms later, in capitalised white font, 0.5 cm below the central screen location. In this relatedness-judgment task, the target display consisted of two words, with inner edges 1.5 cm to both the left and right of the central screen location. On each trial, one of the words was an associate of the prime and the other was an irrelevant foil of the same valence[4]. For any given trial, there was a

[4] This foil was an associate from a *nonet* assigned to the other assessment phase and to the opposite target associate condition.

50% chance that the related associate would appear on a particular (left or right) side of the screen. Participants were instructed to indicate which word in the target display was related to the prime as quickly and accurately as possible by pressing the mouse button (left or right) corresponding to the relative position of their selection. The computer recorded the latency between the appearance of the target display and the detection of the participant's mouse button response. Accuracy of this response was also recorded. As in the training task, a maximum of 6 sec was allowed for this response. The next trial was then initiated one second later.

In this interpretive assessment task, as in Study 3, in half of the assessment trials the prime was a threat/non-threat homograph. However, in the remaining half of the trials the prime was now an unambiguous valenced word selected from the original *nonets* for this purpose. Across the assessment trials, ambiguous primes were followed with equal probability by threat or non-threat related target associates (threat and non-threat associate conditions). In different unambiguous prime trials, the prime was unambiguously threatening or non-threatening with equal frequency, in each case followed by a related target associate.

For each participant, the pre-training and post-training interpretive assessment phases were each comprised of 32 trials. A different 32 *nonets* not presented during the interpretive training phase were assigned to the trials of each interpretive assessment phase. Thus, across the three phases, all 96 *nonets* were employed. Within each training group, the assignment of the *nonets* to particular interpretive assessment phases was rotated, as was the allocation of *nonets* to the trials involving ambiguous and unambiguous primes, and threat and non-threat associates, within each interpretive assessment phase. Thus, across the participants within each training group, stimuli from

each *nonet* appeared an equal number of times in each type of trial within each interpretive assessment phase.

Additionally, the allocation of the *nonets* to the two interpretive assessment phases and to the interpretive training phase was rotated within each training group, such that particular *nonets* were equally often allocated to the pre-training interpretive assessment phase, the interpretive training phase, and the post-training interpretive assessment phase.

Procedure

As for previous studies, each participant was tested in an individual testing chamber which contained the computer equipment. The participant was provided with verbatim instructions concerning completion of the relatedness-judgment interpretive assessment task and fragment-completion interpretive training task (see Appendices VIII and IX). The experimenter demonstrated the completion of one trial of each task, to illustrate the verbal instructions.

Next, the participant completed 24 practice trials which used an extended version of the set of practice stimuli provided in the previous two studies (see Appendix X). In order to reassert the task order which had already been explained to the participants, both the first and final eight of these practice trials involved the relatedness-judgment task, while the middle eight employed the fragment-completion task. The experimenter remained present during the practice to ensure comprehension of task requirements. The experimenter then left the testing chamber, and the participant completed the 32 trials of the relatedness-judgment interpretive assessment task (pre-training interpretive assessment phase); the 160 trials of the fragment-completion

interpretive training task (interpretive training phase); and the 32 trials of second relatedness-judgment interpretive assessment task (post-training interpretive assessment phase). Between phases, the participant was allowed a self-regulated break during which an on-screen message reminded him or her that the task was about to change (i.e., from relatedness-judgment to fragment-completion, or vice versa). Following the second completion of the relatedness-judgment interpretive assessment task, the participant was fully debriefed and the session was terminated.

Results

Selection Times

Mean selection times on correct trials, demonstrated by each training group under each of the critical assessment conditions, are provided, together with corresponding standard deviations[5], in Table 4.1.

Analyses of pre-training selection times.

The pre-training interpretive assessment phase was analysed first, in order to test for the existence of any pre-existing group differences in interpretive bias prior to the training. Thus, 2-way, mixed design ANOVAs were used to analyse these pre-training selection time data. These analyses incorporated the between-groups factor, training group (threat training group vs. non-threat training group), and the within-participants factor, target associate (threat associate vs. non-threat associate).

[5] Visual data inspection revealed that the distribution of selection times was positively skewed. In order to correct for the influence of this skewness upon the outcomes of analyses, these data were therefore subjected to log transformation. Analyses of corrected data revealed the equivalent pattern of findings in all cases to analyses to untransformed data.

Table 4.1.

Mean Selection Times (ms) Obtained In Study 4

	Pre-training		Post-training	
	Non-threat associate	Threat associate	Non-threat associate	Threat associate
	Ambiguous prime			
Non-threat training group	1598 (490)	1740 (455)	1476 (381)	1761 (507)
	Unambiguous prime			
	1356 (374)	1434 (447)	1273 (334)	1384 (387)
	Ambiguous prime			
Threat training group	1599 (507)	1806 (691)	1508 (475)	1588 (506)
	Unambiguous prime			
	1426 (487)	1424 (502)	1312 (408)	1463 (609)

Note. Standard deviations are provided in brackets.

Most crucially, these analyses revealed that the training group by target association interaction did not approach significance, for trials involving unambiguous primes, $F(1, 46) = 1.20$, *ns*, or ambiguous primes, $F(1, 46) = 0.23$, *ns*. The analyses thus provided no evidence that the groups differed, prior to the training, in the capacity to process associates of differentially valenced prime meanings, or in the interpretive bias

that they imposed upon the ambiguous primes. Indeed, the only significant effect revealed by this analysis was a main effect of target associate, $F(1, 46) = 6.79$, $p<.05$, with participants, on average, faster to select target associates related to the non-threatening, relative to threatening, meanings of homographs primes (1599 ms vs. 1773 ms, respectively). The presence of this significant effect for selection times for targets following ambiguous primes, together with the non-significance of this effect for targets following unambiguous primes, $F(1, 46) = 0.29$, *ns*, suggests that prior to the training, on average, the participants interpreted the homographs in a non-threat-congruent, rather than threat-congruent, fashion. However, as this effect was not significantly modified by the training group factor, it is possible to conclude that the groups did not detectably *differ* in interpretive bias prior to the delivery of the interpretive training. No other effects were revealed (all other *Fs* <2).

Analyses of post-training selection times.

Having obtained no evidence that the groups differed in interpretive bias prior to training, the selection time data from the post-training interpretive assessment phase could then be analysed. Once again, these data were entered into 2-way, mixed design ANOVAs, in which the factors were training group (threat training group vs. non-threat training group) and target associate (threat associate vs. non-threat associate). For responses for targets following unambiguous primes, the only significant effect was target associate, $F(1, 46) = 9.56$, $p<.05$. This reflected a relative advantage to more quickly select non-threat relative to the threat related associates of the unambiguous primes (1292 ms vs. 1423 ms, respectively), irrespective of training group. The 2-way training group by target associate interaction was not significant for these unambiguous

primes, however, $F(1, 46) = 0.24$, *ns*, and neither was the main effect of training group, $F(1, 46) = 0.23$, *ns*. Thus, there was no indication whatsoever that the groups differed in the capacity to select associates of prime meanings of different valences, where no differential prime interpretation was possible.

Two significant effects were revealed in analyses of selection times for targets following the homograph primes. Firstly, as for the unambiguous primes, there was an overall speeding for non-threat related, relative to threat related, target associates (1491 ms vs. 1674 ms), $F(1, 46) = 17.91$, $p<.001$. However, this main effect was now modified in a significant training group by target associate interaction, $F(1, 46) = 5.62$, $p<.05$. The nature of this interaction is depicted in Figure 4.1.

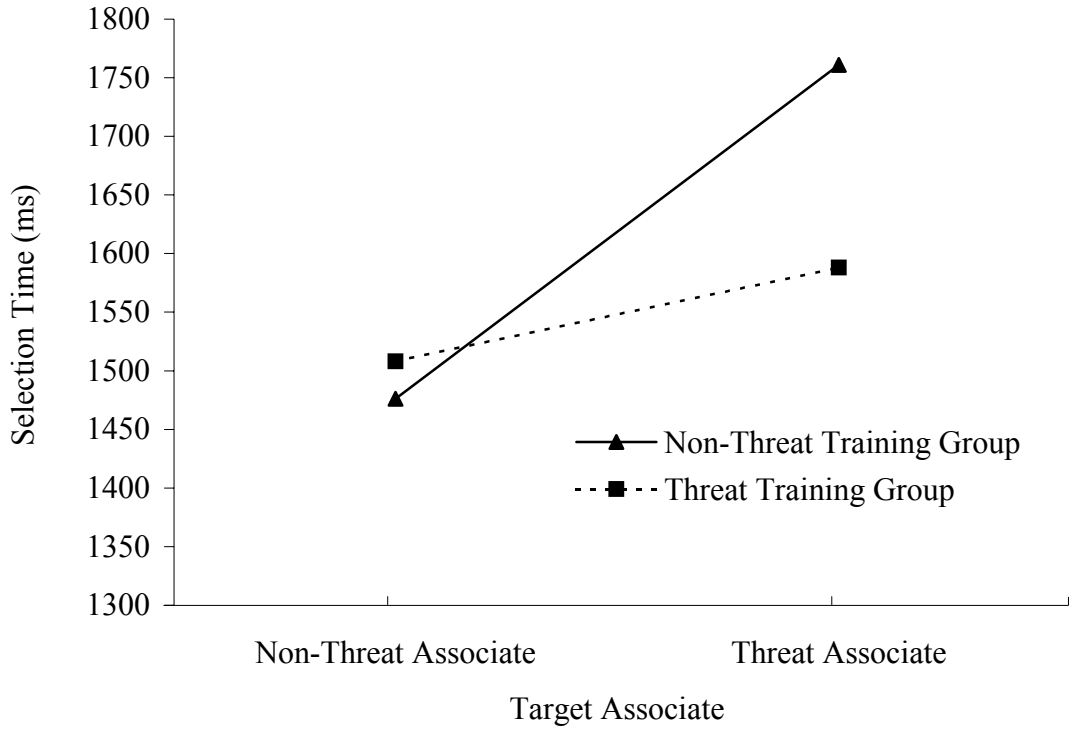

Figure 4.1

Mean selection times for target associates following ambiguous primes in the post-training interpretive assessment phase of Study 4.

As can be seen from the figure, following training, the groups displayed patterns of selection time consistent with an induced group difference in interpretive bias, congruent with their training group assignment. Thus, the non-threat training group, relative to the threat training group, displayed relatively longer latencies to select threat, relative to non-threat associates, but only when, as depicted in the figure, these were associates of potential interpretations of ambiguous primes, as opposed to unambiguous primes. All other effects were non-significant (Fs<1).

Given the obtained evidence of this training successful induction of a group difference in interpretive bias, a follow-up analysis was then performed in order to determine the extent to which the effect observed for ambiguous primes following training differed from responses shown in the unambiguous baseline condition, as well as from responses obtained at pre-training interpretive assessment. Thus, all data were entered into a 4-way, mixed-design, ANOVA that included assessment time (pre-training vs post-training) and prime ambiguity (ambiguous prime vs unambiguous prime) as factors. There was a main effect of prime ambiguity, $F(1, 46) = 57.42$, $p<.001$, and a main effect of target associate, $F(1, 46) = 23.2$, $p<.001$. However, most centrally, this analysis indeed revealed a four-way interaction of prime ambiguity and assessment time with the training group and target associate factors, $F(1, 46) = 4.86$, $p<.05$. All other effects were non-significant (all Fs<2 except for the prime ambiguity by target associate interaction, $F(1, 46) = 3.23$, *ns*).

Selection Accuracies

Participant selection accuracies during the interpretive assessment phases, for each relevant condition, are provided in Table 4.2. Given the strong evidence for the induction of a group difference in interpretive bias from the selection time analyses, these selection accuracy data were subjected to equivalent analyses to exclude speed-accuracy trade-off explanations for those findings.

Analyses of pre-training selection accuracies.

As for the selection time data, pre-training selection accuracy data were entered into 2-way mixed design ANOVAs involving the same factors. Here also, the training group by target associate interaction was not significant for unambiguous primes, $F(1, 46) = 0.65$, *ns*, or for ambiguous primes, $F(1, 46) = 0.25$, *ns*, thereby providing no indication that, prior to training, these groups differed in their capacity to select associates of differentially valenced prime meanings, or in interpretive bias. Indeed, the only significant effect revealed was a significant main effect of target associate in the analysis of ambiguous prime data, $F(1, 46) = 4.43$, $p<.05$. This main effect reflected greater accuracy in selecting targets associated with the non-threatening, than with the threatening meanings of the homographs (89.5% vs. 84.1%). Together with the non-significance of this same effect in the unambiguous prime data, $F(1, 46) = 0.31$, *ns,* this finding suggests that participants, on average, were more likely to interpret the homographs in a non-threat-congruent fashion prior to training. Thus, this single significant effect was consistent with the outcome of the corresponding analysis of response latencies and therefore does not support differential speed-accuracy trade-off explanations. No other effects were significant (all other Fs<1).

Table 4.2

Mean Selection Accuracies (%) Obtained In Study 4

	Pre-training		Post-training	
	Non-threat Associate	Threat associate	Non-threat associate	Threat associate
	Ambiguous prime			
Non-threat training group	90.6 (9.2)	83.8 (15.8)	89.1 (11.2)	89.1 (9.3)
	Unambiguous prime			
	93.2 (6.4)	89.1 (12.9)	93.7 (10.4)	92.7 (13.2)
	Ambiguous prime			
Threat training group	88.5 (9.7)	84.4 (17.0)	84.9 (15.2)	88.0 (14.0)
	Unambiguous prime			
	93.2 (12.2)	92.7 (12.7)	91.1 (11.9)	88.5 (11.6)

Note. Standard deviations are provided in brackets.

Analyses of post-training selection accuracies.

Selection accuracies for post-training data were also subjected to the same 2-way mixed design ANOVAs. These analyses revealed no significant effects (all *Fs* ≤1). Most importantly, the target associate by training group interaction was not significant for unambiguous primes, $F(1, 46) = 0.19$, *ns*, or for ambiguous primes, $F(1, 46) = 0.48$, *ns*. Thus, there was no indication of any performance difference on these

scores contrary to that displayed for the main selection time analyses. These analyses thus provide no support for a speed-accuracy trade-off explanation of the selection time analyses.

Discussion

The aim of this study was to overcome methodological limitations potentially associated with Study 3, in a further attempt to develop a task capable of inducing a group difference in interpretive bias. It was demonstrated that the two training groups did not differ significantly in terms of their interpretive bias prior to training, as revealed by their relative speeds in selecting associates related to threatening and non-threatening meanings of homograph primes in the pre-training interpretive assessment phase. Analyses of accuracies from this phase were also consistent with this conclusion. However, after completing variants of the fragment-completion task designed to encourage a bias either towards or away from the threatening interpretations of ambiguity, these groups then came to display selection times consistent with a group difference in interpretive bias, and again, analyses of accuracies did not undermine this interpretation of these findings. Furthermore, a follow-up analysis which directly contrasted the observed group difference for targets following ambiguous primes to the unambiguous prime baseline, as well as responses obtained prior to training, confirmed that this group difference could indeed be distinguished from these baseline conditions. Thus, these findings are altogether consistent with the conclusion that the training procedure successfully induced a group difference in interpretive bias.

By achieving such a training effect, this interpretive training task achieved the primary goal of this first phase of the research. However, the discrepancy between the

findings of this methodology and that employed in Study 3 remains to be explained. Of course, the kinds of explanation which might be provided could be derived from the important modifications introduced in this study, as contrasted with Study 3: the substitution of trials involving unambiguous primes for the previously employed unprimed trials; the introduction of a novel interpretive assessment task; and the modification of the temporal and spatial parameters of prime and target presentation. Explaining the variation in outcomes across these studies, in light of such potentially relevant modifications, may be important for research which subsequently attempts to replicate and enhance these training effects. In lieu of this, a subsidiary goal of Phase 2 was to use appropriate methods to assess the importance of the different modifications for the success of this interpretive training.

Whatever the explanation for this discrepancy, the findings of this present study strongly support the claim, made by Grey & Mathews (2000), that interpretive bias can indeed be manipulated using an approach based upon the introduction of training contingencies into a priming methodology. Further, an alternative explanation for such findings, that the training might simply modify the capacity to process target associates of differentially valenced meanings, was excluded by these present findings. The major goal of Phase 1, to create a procedure that could achieve, and permit assessment of such interpretive training, was thus met. To appraise the hypothesis that interpretive bias can causally mediate anxiety vulnerability, it remained for the studies described in Phase 2 to incorporate this training methodology into designs capable of assessing the emotional consequences of such interpretive bias manipulations.

CHAPTER 5

OVERVIEW OF METHODOLOGIES EMPLOYED IN PHASE 2

Overview Of Experimental Designs And Associated Hypotheses

Phase 1 saw the development of a training methodology which was demonstrated in its capacity to induce a group difference in interpretive bias. An alternative account of the influence of this training, that it simply induces a group difference in the ability to process targets related to differentially valenced prime meanings, was not supported. It was therefore possible to proceed to Phase 2 of this research program, which employed the developed training approach to investigate the causal role of interpretive bias in anxiety vulnerability. The experimental designs in the studies carried out within Phase 2 included three important components. Firstly, in each study, participants were exposed to variants of the training methodology developed in Phase 1, with training contingencies introduced to encourage threat and non-threat-congruent interpretive biases for different groups of participants. Secondly, also as in Phase 1, the success of this induction was determined using interpretive assessment tasks, involving priming methodologies in which the training contingencies did not apply. Thirdly, the hypothesis that interpretive bias can causally mediate anxiety vulnerability was then assessed, by exposing these groups to a stressor, and assessing the degree to which the participants displayed variations in state anxiety elevation in reaction to this stressor. On the basis of the hypothesis that interpretive bias can causally mediate anxiety vulnerability, it was predicted that where group differences in interpretive bias were successfully induced, the group induced to interpret ambiguity in

a non-threat-congruent fashion would then display a reduced degree of state anxiety elevation in reaction to the stressor, in comparison to the group induced to interpret ambiguity in a threat-congruent fashion.

As noted in the introduction, interpretive bias might causally mediate anxiety vulnerability specifically, or might causally mediate vulnerability to elevations in depression, as well as anxiety. Therefore, the methodologies in Phase 2 included measures of depressive mood state (i.e., *state* depression), as well as of state anxiety, both prior to and following the stressor. Thus, training-linked variations in anxiety vulnerability were assessed, as was the specificity of this hypothesized causal mediation.

Video Stressor

As indicated above, in the studies described in Phase 2, following the interpretive training and interpretive assessment tasks, a negative emotional reaction was evoked in the participants by exposing them to a stressor. Various types of stressor have been employed to evoke negative emotional reactions. These have included the completion of difficult cognitive tasks, such as mental arithmetic (e.g., Jain, Burg, Souter, & Zaret, 1995), the Stroop task (e.g., Carr et al., 2003; Lattimore, 2001) or difficult anagrams (Mogg, Mathews, Bird, & MacGregor-Morris, 1990); the discussion of personally-relevant stressful events (e.g., Harrigan, Lucic, & Rosenthal, 1991; Ironson et al., 1992); and the threat of pain (e.g. Grillon, Ameli, Woods, Merikangas, & Davis, 1990). However, one of the most reliable techniques for inducing a negative emotional response, has involved exposing participants to a film or video depicting threatening situations or stimuli (e.g., Brewin & Saunders, 2001; Green, Rogers, &

Elliman, 1995; Green, Rogers, & Hedderley, 1996; Gross & Levenson, 1995; Kinzie et al., 1998; Lattimore, 2001; Riemann & McNally, 1995; Speisman, Lazarus, Mordkoff & Davison, 1964; Tuschen-Caffier & Vogele, 1999).

For example, Green et al. (1995) presented participants with a 10-minute video sequence describing the medical consequences of a nuclear explosion in a populated area. The participants completed a series of visual analogue mood scales (VAMS) prior to and following the video. Each mood scale was labelled at opposite ends with relevant extremes of a different emotional dimension (e.g., anxious - relaxed; happy - depressed). Whenever the scales were provided, participants selected the point along each scale which corresponded to their current mood state. Analyses of ratings on these scales indicated a significant increase in state anxiety and state depression, from prior to following the video presentation, indicating that a negative emotional reaction was evoked by the video. Presenting a similar video stressor to participants, Green et al. (1996) found that participants reported elevated levels of negative mood state 5 minutes after the termination of the video. Similar findings were also obtained by Riemann and McNally (1995), who presented participants with either a 20-minute segment from a horror film, or a video sample of similar length from an emotionally neutral documentary. All participants completed anxiety VAMS prior to and following the film. In contrast to those presented with the emotionally neutral film, those presented with the horror film displayed elevated state anxiety ratings from pre- to post-film presentation.

There are a number of advantages in employing aversive film or video stimuli as stressors in experimental situations. Edited video sequences can be used to evoke detectable anxiety responses within relatively short time periods. In addition, a range of

ecologically valid feared stimuli and situations, each of which may be capable of evoking an emotional response, can be incorporated into a single video presentation, thus spanning a range of possible concerns of participants. For example, employing similar analogue measurements of mood state to those described by Green et al. (1995), Gross and Levenson (1995) found that clips averaging 151 seconds in length could elicit strong emotional responses, including elevations in state anxiety and depression. The video segments found by these experimenters to evoke the most powerful emotional responses included depictions of police abusing protesters (146 seconds long), the amputation of an arm (63 seconds long); and a basement chase from the film, Silence of the Lambs (209 seconds long). Thus, these researchers were able to employ stimuli involving a range of possible threats to successfully evoke strong negative emotional responses within relative short time periods.

Another advantage of video stressors, as Gross and Levenson (1995) note, is that they are easy to administer. Furthermore, their effective use requires no deception, and so is less reliant upon the skill of the experimenter, which could plausibly vary (for example on the basis of experimenter experience).

The present series of studies therefore employed a brief video stressor to evoke a negative emotional response. Mood state ratings made prior to and following this stressor (see below) permitted the assessment of the degree to which the participants from each training group responded with elevations in state anxiety and depression in response to the video stressor.

Assessment Of Emotional Response To The Video Stressor And Interpretive Training

In order to assess variations in emotional reactivity, the studies in Phase 2 employed visual analogue mood scales (VAMS), similar to those employed in previous studies examining emotional responses to video stressors (e.g., Green et al., 1995; 1996). Thus, participants provided analog ratings of the degree of state anxiety and depression experienced at key points during the experiment.

There is good precedent for the use of VAMS to assess variations in state anxiety and depression (e.g., Aitken, 1969; Cella & Perry, 1986; Tseng, MacLeod, & Wright, 1997). As has already been discussed, and most pertinent to the present study, researchers such as Green et al. (1995) have employed these scales to successfully detect the influence of a stressful video upon state anxiety. As well as being sensitive to momentary emotional fluctuations, VAMS are also quick to administer and easy to complete. These scales have proven to be effective in assessing diurnal or day-to-day fluctuations in mood state (Cella & Perry, 1986; Steiner et al., 1999) and mood state variations that occur in response to other acute stressors, such as CO_2 challenge (van Beek et al., 2003) or public speaking (Del-Ben et al., 2001).

VAMS have demonstrated high levels of test-retest reliability. For example, Kreindler, Levitt, Wooldridge, and Lumsden (2003) provided participants with a number of VAMS on two occasions, separated by an interval of 10 minutes during which participants remained in a quiet room. This study revealed high intra-class correlation coefficients for VAMS-rated items (greater than .7 for 14 out of 18 items).

VAMS also show moderate to high concurrent validity when compared to more extensive mood scales. In a sample of relatives of burns victims, Cella and Perry (1986)

found a reasonable correlation, r = .58, between a depression VAMS and Beck Depression Inventory score (Beck, Ward, Mendelsohn, Mock, & Erlbaugh, 1961), and between an anxiety VAMS and state scores from the State Trait Anxiety Inventory (Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983), r = .52. Similarly, Maruff et al. (1994) described analyses of ratings provided by an HIV positive sample, which revealed a high correlation between anxiety VAMS scores and STAI –S scores, r=.86, and between depression VAMS scores and scores on the Center for Epidemiological Studies (CES)-Depression scale (Radloff, 1977), r =.78.

In the present study, VAMS were presented at four points: prior to and following the interpretive training/assessment phases, and prior to and following the presentation of the video stressor. Thus, this design permitted assessment of the direct impact of the training on both state anxiety and depression. Separately, and most importantly, the impact that this training exerted on emotional vulnerability could also be assessed, as indicated by anxious and depressive reactivity to the subsequent stressor. Thus, the hypothesis that interpretive bias can causally mediate anxiety vulnerability was directly tested within Phase 2 studies, as was the specificity of the influence of this hypothesised causal mediator.

Variations In Training And Assessment Methodology Employed

It was considered desirable to instantiate this general experimental design in more than one study, with some degree of variation in the tasks involved in each case, to ensure some degree of generalisability for the conclusions drawn. Furthermore, it was considered not undesirable that the effectiveness of such training procedures in inducing group differences in interpretive bias might vary across these experiments. In the

context of this series of studies, such variability could shed further light on the hypothesis under investigation. Specifically, the failure to induce group differences in interpretive bias should be accompanied by a parallel failure to induce group differences in anxiety vulnerability, unless some aspect of the training manipulation, unrelated to its influence upon interpretive bias, underpins its capacity to modify anxiety vulnerability.

This requirement for the causal hypothesis to be assessed using different variants of the same general experimental approach permitted a subsidiary issue also to be addressed in this second phase. Specifically, the employment of different tasks allowed for the empirical appraisal of different accounts of the inconsistent outcomes observed across Studies 3 and 4, in terms of their success in inducing group differences in interpretive bias. This was considered an issue which might be of some relevance to future researchers attempting to replicate or enhance interpretive training effects.

It is possible to distinguish at least three accounts of the observation that the training task used in Study 4 succeeded in inducing a group difference in interpretive bias while that used in Study 3 did not. These accounts can be distinguished in terms of the task features associated with each training and assessment task. Firstly, according to the *general task features* account, the success of the training in Study 4, as opposed to Study 3, resulted from the introduction of changes to key task parameters, including the reduction of the SOA and the substitution of the condition involving unambiguous primes for the unprimed condition. Secondly, according to the *training-assessment distinctiveness* account, the introduction of a different task to appraise interpretation in the interpretive assessment phase from the task used to train interpretive bias was most important. This account suggests that this alternative assessment task permitted the more sensitive assessment of group differences in interpretive bias, by excluding the

influence of task-specific aspects of learning (e.g., skill in fragment-completion, irrespective of interpretation of the prime). Thirdly, the *assessment sensitivity* account suggests that it was specifically the use of the relatedness-judgment task to assess interpretive bias, rather than the fragment-completion task, that led to the success of Study 4.

Table 5.1 summarises a series of four studies which would distinguish these accounts, with each possible study involving a different combination of interpretive training and interpretive assessment tasks.

Table 5.1.

Arrangements Of Interpretive Training And Assessment Tasks In Phase 2 And Associated Predictions From Each Account

			Accounts		
Study	Interpretive training task	Interpretive assessment task	GTF	T-AD	AS
5	Fragment-completion	Relatedness-judgment	Predicted	Predicted	Predicted
6	Relatedness-judgment	Relatedness-judgment	Predicted	Not predicted	Predicted
7	Relatedness-judgment	Fragment-completion	Predicted	Predicted	Not predicted
8	Fragment-completion	Fragment-completion	Predicted	Not predicted	Not predicted

Note. GTF = *general task features account*; T-AD = *training-assessment distinctiveness account*; AS = *assessment sensitivity account*.

Each accounts provides a different profile of predictions across these studies, in terms of detecting induced group differences in interpretive bias. Given that all of these tasks use the SOA parameters used in Study 4 and none include an unprimed condition, the *general task features* account suggests that each training task should induce detectable group differences in interpretive bias, regardless of whether the relatedness judgment or fragment-completion task is used as the assessment task. Conversely, the *training-assessment distinctiveness* account suggests that training should lead to detectable group differences in interpretive bias only on those two studies in which differing tasks are employed within interpretive training and assessment phases (i.e., relatedness-judgment in training and fragment-completion task in assessment, or vice versa). Finally, the *assessment sensitivity account* suggests that an induced group difference in interpretive bias will be detected only in those two studies in which the relatedness-judgment task is employed to assess interpretive bias.

As indicated in the table, the four studies described in Phase 2 in fact employed these four different arrangements of training and assessment task, in order to provide the basis for the assessment of these different accounts.

However, the main purpose of all four studies in Phase 2 was to test the key prediction generated by the causal hypothesis under investigation. That is, if interpretive bias causally mediates anxiety vulnerability, then when an induced group difference in interpretive bias is detected, this should be accompanied by a parallel group difference in anxiety vulnerability, as assessed by emotional reactivity to the subsequent video stressor. Conversely, whenever a training task fails to induce a detectable group difference in interpretive bias, no such group difference in anxiety vulnerability should be detected.

CHAPTER 6

ASSESSMENT OF THE CAUSAL HYPOTHESIS USING FRAGMENT-COMPLETION TRAINING AND RELATEDNESS-JUDGMENT ASSESSMENT TASKS

The study described in this chapter was the first of four experiments designed to assess the hypothesis that interpretive bias can causally mediate anxiety vulnerability. As indicated in Chapter 5, the approach which was employed here involved attempting to induce a detectable group difference in interpretive bias using a training procedure, then assessing the causal impact of this induction upon anxiety vulnerability, as measured in terms of state anxiety elevation evoked by a subsequent stressor.

In this first of the four studies designed to assess the causal hypothesis, the fragment-completion task was employed as the means of training, and the relatedness-judgment task was then used to assess the success of this training in manipulating interpretive bias. Thus, the study used the training and interpretive assessment tasks already demonstrated to produce a detectable group difference in interpretive bias in Phase 1. Following this, as in all the studies in Phase 2, the participants were exposed to a stressor which took the form of a brief distressing video. Variations in the intensity of emotional reactivity, shown in response to this stressor, were recorded.

Study 5

The goal of this study was to investigate the consequences of the interpretive training, developed in Phase 1, for anxiety vulnerability. Thus, the interpretive training and interpretive assessment phases of this study employed that combination of tasks

already demonstrated in Phase 1 to induce, and subsequently detect, a group difference in interpretive bias. Following this, the consequences of such an induced interpretive bias manipulation for anxiety vulnerability were then investigated. This was done by exposing participants to the video stressor, and assessing the fluctuations in state anxiety for groups who had just experienced training contingencies designed to induce a group difference in interpretive bias.

This study thus incorporated an interpretive training phase, an interpretive assessment phase, and a stress phase. During the interpretive training phase, depending on group allocation, participants were exposed to one of two variants of the fragment-completion interpretive training task. For participants in the threat training group, homograph primes and unambiguous primes were each followed by word-fragments whose completions were associated with the primes' threatening meanings. In contrast, for the participants in the non-threat training group, the target fragments permitted only completions which were associated with the primes' non-threatening meanings. Each contingency was designed to encourage the development of the congruent interpretive bias amongst relevant participants.

In the subsequent interpretive assessment phase, the relatedness-judgment interpretive assessment task, shown in Study 4 to be sensitive to group differences in interpretive bias, was employed to test for training induced group differences in interpretive bias. As with all of the studies in Phase 2, and in contrast to those in Phase 1, interpretive bias was now tested following, but not prior to, the interpretive training phase. Further, this assessment now employed double the number of trials employed in the post-training interpretive assessment of Study 4. The intention here was to maximise the sensitivity of the assessment to a post-training group difference in

interpretive bias, congruent with training condition. The capacity to detect whether such a group difference was successfully induced was a necessary prerequisite for adequate assessment of the causal hypothesis.

In all other respects this relatedness-judgment interpretive assessment task was identical to the variant employed in Study 4, with all participants receiving an equal number of homographs or unambiguous primes. The homograph primes were followed by targets which with equal frequency were related to the primes' threatening meanings or to their non-threatening meanings. Similarly, the unambiguous primes were equally often threatening or non-threatening, followed by associated targets in each case. As in Study 4, the participants' access to meanings of the primes was assessed through their latency to correctly select each associate from its target pair, when this associate was related to either the threatening or the non-threatening meaning of the prime.

Thus, the predictions for the interpretive assessment task essentially were the same as for Study 4. It was predicted that if the training simply induced a group difference in the capacity to identify associates of differentially valenced prime meanings, but did not affect the interpretation of ambiguity, then a group difference in the speed to identify non-threat relative to threat associates in the target pairs would be found, even following the unambiguous primes. Conversely, if, as was anticipated in light of findings from Study 4, the training induced a group difference in interpretive bias, then this group difference in responding would only be shown for targets following homograph primes. On these ambiguous prime trials, the non-threat training group, compared to the threat training group, would display speeded selection of non-threat associates, relative to threat associates, of the preceding homograph primes.

The novel and also pivotal aspect of this current study was the stress phase, which followed the interpretive training and interpretive assessment phases. In this stress phase, participants were exposed to the video stressor, designed to make manifest any extant group differences in anxiety vulnerability resulting from training-linked differences in interpretive bias. This video stressor was comprised of a series of clips depicting real-life rescue operations and emergency situations which were selected for their distressing content. Participants provided anxiety and depression VAMS (visual analogue mood scale) ratings prior to and following this stressor, as well as prior to the interpretive training phase, and immediately following the interpretive assessment phase. The measures directly preceding and following the interpretive training/assessment trials were included to ensure that the differentially valenced stimuli presented to the training groups did not lead directly to a group difference in *mood state*. Thus, these scores could be analysed independently of the scores indicating response to the stressor itself, which is a major strength of this present approach over previous approaches to this issue.

The causal hypothesis itself, that interpretive bias can causally mediate anxiety *vulnerability*, provided predictions for the VAMS scores obtained before and after stressor. It was predicted that if the two training groups were indeed induced to differentially interpret ambiguity, then the non-threat training group, relative to the threat training group, would display a reduced degree of elevation in state anxiety in reaction to the video stressor. Further, if the causal role of interpretive bias is specific to anxiety, then this relative reduction in reactivity amongst the non-threat training group, would be displayed on anxiety VAMS scores, but not on depression VAMS scores. Conversely, if the non-threat training group, relative to the threat training group,

displayed a reduced degree of reactivity on depression, as well as anxiety, VAMS scores when presented with the video stressor, then this would suggest interpretive bias as a causal mediator of negative emotional vulnerability more generally.

Method

Overview

This study consisted of three phases: an interpretive training phase, an interpretive assessment phase, and a stress phase. In the interpretive training phase, participants were assigned to a training group (threat training group or non-threat training group), which determined the valence of the word-fragments received in this training. In the subsequent interpretive assessment phase, each prime could be either ambiguous or unambiguous, and ambiguous primes were followed by target pairs containing associates related to either their threatening or non-threatening meanings (threat and non-threat associate conditions). Half of the unambiguous primes were threatening, and half were non-threatening, with related associates following in each case. The principal dependent variable was the time taken to accurately select related associates from target displays during the interpretive assessment phase.

The second major aspect of the study, designed to assess the influence of the training upon mood state and emotional vulnerability, involved distinguishing the mood variations of each training group. An outline of the procedure, which includes the points at which mood ratings were taken, is given in Figure 6.1. As indicated in the figure, mood ratings were obtained prior to and following the trials designed to train and assess interpretive bias (pre-training and post-interpretive assessment mood assessment points), and just prior to and following the video stressor (pre-stress and post-stress

mood assessment points). Thus, the inclusion of these four mood assessment points permitted the influence of the interpretive training on mood state during training, and upon subsequent emotional reactivity, to each be assessed. The dependent variables here were scores on visual analogue mood scales (VAMS), assessing both state anxiety and state depression.

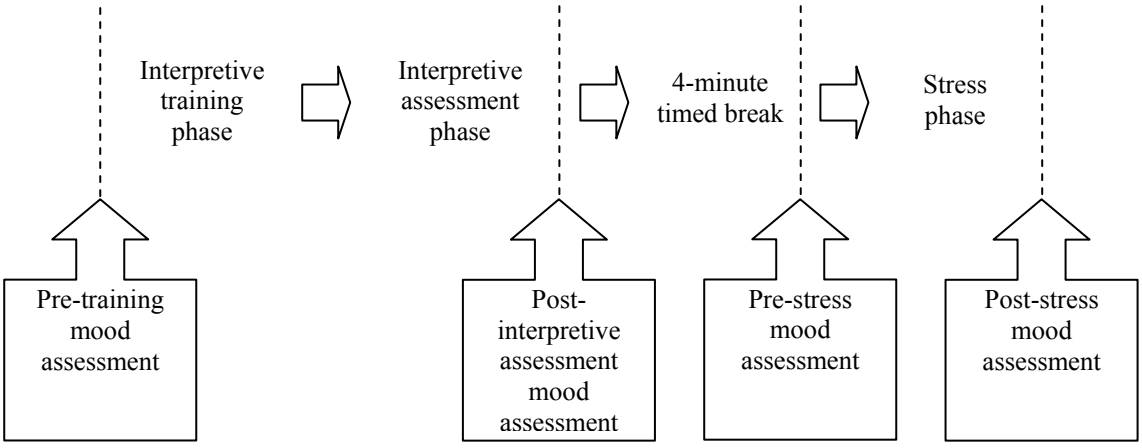

Figure 6.1 Overview of mood assessment points incorporated into design of Study 6.

Participants

Twenty-four male and 24 female undergraduate psychology students participated in the experiment in exchange for course credit. Participants were randomly allocated to non-threat and threat training groups, with the constraint that 12 male and 12 female participants were assigned to each group. The characteristics of each training group are provided in Table 6.1.

The training groups did not significantly differ in terms of trait anxiety, as indicated by scores on the State Trait Anxiety Inventory, trait scale (STAI-T: Spielberger, et al., 1983), $F(1, 46) = 0.17$, *ns*, nor in state anxiety as indicated on the

State Trait Anxiety Inventory, state scale (STAI-S: Spielberger et al., 1983), $F(1, 46) = 0.05$, *ns*, nor in depression, as indicated by the Beck Depression Inventory, second edition (BDI-II: Beck, Steer, & Brown, 1996), $F(1, 46) = 0.41$, *ns*, nor in age, $F(1, 46) = 0.45$, *ns*.

Table 6.1

Mean Characteristics Of Participants In Study 5

Characteristic	Non-threat training group	Threat training group
STAI–T	38.2	37.2
	(6.5)	(9.3)
STAI–S	34.4	35.0
	(9.2)	(10.5)
BDI–II	8.0	7.1
	(4.9)	(5.4)
Age (yrs)	18.0	18.3
	(1.4)	(1.6)

Note. Standard deviations are provided in brackets. *N*=24, in each training group.
STAI–T = State-Trait Anxiety Inventory, trait scale; STAI-S = State-Trait Anxiety Inventory, state scale;
BDI-II = Beck Depression Inventory, second edition.

Materials

State-Trait Anxiety Inventory.

As already indicated, the State-Trait Anxiety Inventory (Revised Form) trait and state scales (STAI-T & STAI-S, respectively : Spielberger et al., 1983), were used to assess initial levels of trait and state anxiety. Each scale contains 20 items relating to symptoms of anxiety. The STAI–T asks participants to rate how they generally feel, by

indicating the frequency with which they experience each of the provided anxiety symptoms. The STAI–S requires participants to rate how they feel at the moment of assessment, by indicating the degree to which they are presently experiencing each described symptom. Scores on these scales range from 20 to 80, with higher scores indicating higher levels of trait anxiety, or state anxiety, respectively.

The two subscales have good internal consistency and test-retest reliability. A large-scale reliability generalization study (Barnes, Harp, & Jung, 2002), provided mean reliability coefficients of .89 for internal consistency, and .88 for test-retest reliability, for the STAI–T. The STAI–S obtained a comparable internal consistency coefficient (.91), and a lower mean test-retest coefficient (.70), consistent with the expectation that state anxiety will vary considerably across time and situations. Spielberger et al. (1983) reported comparable levels of internal consistency for a university student sample, with alpha levels as high as .93 for the STAI–S and .91 for the STAI–T.

Beck Depression Inventory, second edition.

As indicated, initial level of depression was assessed using the Beck Depression Inventory, second edition (BDI-II: Beck et al., 1996). The questionnaire contains 21 items relating to symptoms of clinical depression. For each item, participants are required to endorse one of four corresponding statements which indicate the severity with which a particular symptom has been experienced across a two week period that includes the time of testing. Scores for this measure range from 0 to 63, with higher scores indicating higher degrees of depressive symptomatology.

The BDI-II shows good reliability in student populations; Osman et al. (1997) obtained an alpha level of .90 for a sample of 230 university students. A high level of

test-retest reliability (.96) has also been reported for this scale, in a sample of university students receiving counselling (Sprinkle et al., 2002).

Visual analogue mood scales.

As indicated, visual analogue mood scales (VAMS) were provided to assess state anxiety and state depression at four mood assessment points across the experimental session: Prior to and following the pair of tasks comprising the interpretive training/assessment phases (pre-training and post-interpretive assessment) and prior to and following the video stressor (pre-stress and post-stress). Some information on the validity and reliability of VAMS is provided in Chapter 5.

These VAMS were 18cm in length, and were designed to assess state anxiety and state depression, respectively. At opposite ends, the anxiety mood scale was labelled with 'relaxed' and 'anxious', and the depression mood scale with 'happy' and 'depressed'. Subjects used the computer mouse to adjust the cursor to the point along each scale that corresponded to their current mood-state. The computer then recorded a score for each scale, with a score of 1 representing the minimum and 100 representing the maximum level of state anxiety or depression.

Lexical stimuli.

All details of the stimuli employed in this study, including those employed in practice, and in the interpretive training and interpretive assessment trials, were the same as for Study 4, and are described in Chapter 4.

Computer Hardware.

The computer hardware employed was identical to that used in Study 4, and also described in Chapter 4.

Experimental Tasks

Interpretive training phase : Fragment-completion interpretive training task.

All details of the fragment-completion training task employed in this experiment, including stimulus presentation details, etc., were the same as for when this task was employed in Study 4 (see Chapter 4).

Interpretive assessment phase: Relatedness-judgment interpretive assessment task.

As indicated, the interpretive assessment phase in the present study involved the same relatedness-judgment interpretive assessment task as was used in Study 4. The task requirements and presentation details of the task in this study were also identical to those that characterized the earlier study, with the exception that this assessment now only followed, but did not precede the training, and that the number of these post-training assessment trials was now 64, rather than 32. For each participant, these trials thus involved the 64 *nonets* from the array of threat/non-threat homographs and associates not employed during the interpretive training phase, once again assigned in equal proportions to conditions in which the primes were threat/non-threat homographs or unambiguous valenced primes, and equally often to conditions in which the target associates were related to the threatening or to the non-threatening meanings of preceding primes. In addition, as in Study 4, the four possible trial types produced by crossing of threat associate and non-threat associate conditions with ambiguous prime and unambiguous prime conditions, appeared equally often within each consecutive eight assessment trials, with trial order randomized within each of these eight trials.

Across participants, the allocation of stimuli to the assessment trials mirrored the approach employed in Study 4. Thus, across the 48 participants, the allocation of stimuli

to different interpretive assessment task conditions was rotated, such that each of the 96 *nonets* from the array of threat/non-threat homographs and associates was assigned equally often to threat and non-threat associate conditions, and to conditions in which the prime was a homograph or was unambiguous. In addition, these stimuli appeared equally often within the first and second blocks of assessment trials.

Finally, the allocation of *nonets* to interpretive training and interpretive assessment phases was also rotated across participants, such that each *nonet* was assigned equally often to either the interpretive training phase, or to either of the two blocks of trials in the interpretive assessment phase.

Stress phase: Video stressor.

This video stressor was comprised of a series of four brief video clips, selected from a pool of 9 clips initially compiled and standardized by Campbell (2001). These clips were considered to plausibly provide the grounds for differentially threatening interpretations, potentially induced by training, to be imposed. For example, distressing aspects of the clips (e.g., the extent of a physical injury, the outcome of an attempted rescue operation) were not resolved until the clips' conclusions, thus permitting some variation in interpretation.

It was considered important that these clips be perceived as anxiety-provoking, as well as sustaining a minimal level of engagement. Each of the clips was thus rated by 6 Psychology graduate students, in terms of four characteristics: the degree to which it induced apprehension, was stressful, was coherent and was engaging. Each of these ratings was made using a 7 point scale, with 1 labeled, 'not at all', 4, 'moderately', and 7, 'very much so'. The selection of the final four clips was determined by selecting those clips which obtained the highest ratings in terms of their perceived capacity to be

stressful and induce apprehension, whilst also obtaining mean ratings on the other two scales which reached or exceeded 4 points (i.e., were perceived to be at least moderately engaging and coherent). The ratings obtained for the four clips selected for use in the study are provided in Table 6.2.

Table 6.2

Mean Ratings For Clips Used In The Video Stressor

Characteristic	Mean
Apprehension inducing	5.6 (0.5)
Stressful	5.3 (0.6)
Engaging	5.9 (0.4)
Coherent	6.1 (0.3)

Note. Standard deviations are provided in brackets.

The final edited version of the video stressor presented these clips in a fixed order, interspersed with intervals of five seconds showing a blank screen. In the first clip, an injured man was shown being rescued from rapidly flowing floodwaters. The second depicted the crash and subsequent rescue of a rally driver, while the third portrayed the collapse of a Ferris Wheel and the rescue of a girl from the top of the structure. In the final clip, a fireman battling against an industrial fire was seen being rescued from the blaze after being engulfed by a sudden eruption of flames. The total

running time of the video stressor was 6 minutes and 43 seconds, and it was presented using a Teac Televideo colour television with built-in VCR.

Procedure

Each participant initially completed the STAI-T, the STAI-S and the BDI-II, following which he/she was provided with verbatim instructions concerning the completion of the fragment-completion task, the relatedness-judgment task, and the VAMS (see Appendices VIII, IX, and XI). While supervised by the experimenter, the participant then completed 16 practice trials of the fragment-completion interpretive training task and eight practice trials of the relatedness-judgment interpretive assessment task. These trials employed the practice stimulus array created for this purpose (see Appendix X). The experimenter then departed from the testing chamber, and the participant completed the pre-training VAMS, the 160 trials of the fragment-completion interpretive training task, the 64 trials of the relatedness-judgment interpretive assessment task, and the post-interpretive assessment VAMS ratings. Between the interpretive training and interpretive assessment phases, the participant was provided with instructions on the screen informing him or her that the task requirements would now change (i.e. from fragment-completion to relatedness-judgment). Following the completion of the post-interpretive assessment VAMS, the participant was provided with a timed 4 minute break during which she or he remained in the testing chamber, and was given no other instruction other than to remain seated in the testing chamber until the experimenter's return. This break was included in order to allow for the dissipation of any immediate and direct emotional impact of the training session on mood state. Participants then completed the third, pre-stress VAMS ratings, following

which they were seated before the television and the video stressor was presented. Instructions on the television at the termination of this video stressor directed participants to reveal and complete the final VAMS (post-stress mood assessment point). [6] The participant was then debriefed and the experimental session ended.

Results

Selection Times

Mean times taken to select associates as being related to primes, under each experimental condition, are presented, along with associated standard deviations, in Table 6.3[7].

Table 6.3

Mean Selection Times (ms) Obtained In Study 5

	Ambiguous prime		Unambiguous prime	
	Non-threat associate	Threat associate	Non-threat associate	Threat associate
Non-threat training group	1835 (379)	2034 (533)	1660 (358)	1661 (401)
Threat training group	2145 (541)	2036 (515)	1792 (550)	1786 (524)

Note. Standard deviations are provided in brackets.

[6] These final ratings were made on a paper and pencil version of the VAMS which otherwise was identical in requirements and layout to the computer version. The reason for this was to minimise the disruption caused by returning the participant to the computer and so to accurately assess mood immediately following stressor exposure. Researchers have compared the outcomes of paper-and-pencil and computerised versions of the VAMS, and have not found them to be distinct (Kreindler et al., 2003).

[7] Visual data inspection revealed that the distribution of selection times was positively skewed. In order to correct for the influence of this skewness upon the outcomes of analyses, these data were therefore subjected to log transformation. Analyses of corrected data revealed the equivalent pattern of findings in all cases to analyses of untransformed data.

Selection times for trials in the interpretive assessment phase were subjected to 2-way mixed-design ANOVAs in which the between-groups factor was training group (threat training group vs. non-threat training group) and the within-participants factor was target associate (threat associate vs. non-threat associate). There were no significant effects revealed for the analysis of selection times for unambiguous prime trials (all $Fs \leq 1$). Most importantly, the 2-way interaction involving training group and target associate was not significant, $F(1, 46) = 0.01$, *ns*, thus providing no indication that the training simply modified the capacity for each group to identify threat and non-threat associates of the primes.

The central expectation, that the training would induce a group difference in interpretive bias, could then be assessed through the analysis of times for selection of targets related to the threat/non-threat homographs. The hypothesis that the training would induce such a group difference in interpretive bias provided the prediction of a training group by target associate interaction for these selection times. Indeed, the only significant effect revealed by this analysis was this training group by target associate interaction, $F(1, 46) = 6.46$, $p<.05$ (all other Fs<2).

Depicted in Figure 6.2, the nature of this interaction is fully consistent with the interpretation that the training induced the intended group difference in interpretive bias. Specifically, it reflects relative speeding on the part of the non-threat training group, relative to the threat training group, to select non-threat, as opposed to threat associates, following these threat/non-threat homographs. Thus, overall, these findings suggest that the non-threat training group were biased, relative to the threat training group, to be more likely to impose non-threat, rather than threat interpretations upon these homograph primes, with this leading to the observed pattern of reduced selection times

to identify targets of one valence relative to the other as being the associates of the primes.

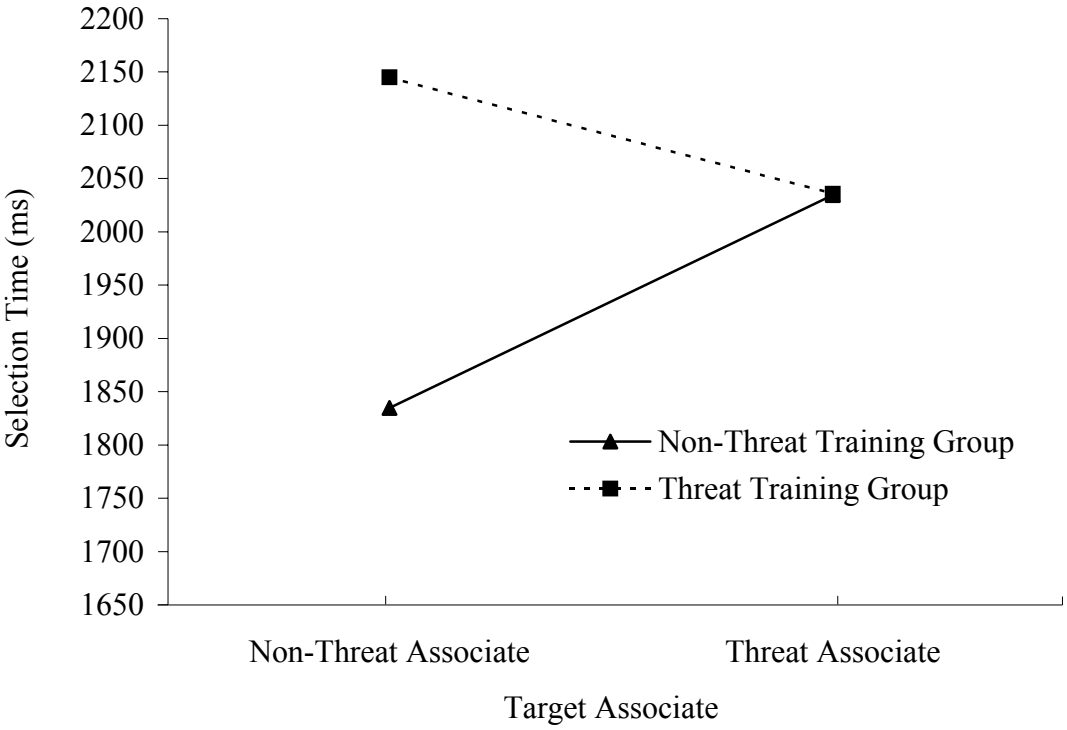

Figure 6.2

Mean selection times for target associates following ambiguous primes in the interpretive assessment phase of Study 5.

A follow-up analysis was then performed in order to determine the degree to which the training effect for ambiguous primes could be distinguished from performance on the baseline unambiguous prime trials. Thus, selection times for each type of prime were entered into one overarching analysis which included prime ambiguity (ambiguous vs unambiguous) as a factor, in addition to training group and target associate. In this analysis, a main effect of prime ambiguity $F(1, 46) = 48.3$, $p<.001$, and a significant target associate by training group interaction, $F(1, 46) = 4.08$, $p<.05$, were subsumed within a significant training group by target associate by prime

ambiguity interaction, $F(1, 46) = 4.42$, $p<.05$. This is consistent with the conclusion that the group difference speeding to select threat associates relative to non-threat associates, was shown for ambiguous primes to a greater degree than for unambiguous primes. No other significant effects were revealed by this analysis, (all other Fs <1).

Selection Accuracies

The selection accuracy scores for the interpretive assessment phase are provided, for each assessment condition and training group, in Table 6.4.

Table 6.4

Mean Selection Accuracies (%) Obtained In Study 5

	Ambiguous prime		Unambiguous prime	
	Non-threat Associate	Threat Associate	Non-threat associate	Threat associate
Non-threat training group	85.3 (11.1)	83.7 (12.4)	92.9 (5.7)	89.2 (10.0)
Threat training group	81.1 (19.9)	83.5 (19.5)	86.9 (19.1)	86.3 (21.1)

Note. Standard deviations provided in parentheses.

Selection accuracies were analyzed for individual conditions in order to exclude speed-accuracy trade-off explanations for findings from the selection time data. The analyses performed on the selection accuracy data thus mirrored those performed on the selection time data (described above). Thus, selection accuracies for assessment trials

involving unambiguous primes, and threat/non-threat homographs, were analyzed using 2-way, mixed-design ANOVAs, in which the factors were training group (threat training group vs. non-threat training group), and target associate (threat associate vs. non-threat associate).

These analyses did not reveal any significant effects. Most crucially, the training group by target associate interaction was non-significant, both for the unambiguous prime trials, $F(1, 46) = 1.34$, *ns*, and for the ambiguous prime trials, $F(1, 46) = 1.35$, *ns*. Thus, there was no indication that the groups differed in their accuracy when selecting associates of each valence, regardless of the ambiguity of the primes, and these findings thus provide no indication that the effects observed on the selection time measure are compromised by a speed-accuracy trade-off. No other effects were significant (other Fs<2 except for the non-significant main effect of target associate for unambiguous primes, $F(1, 46) = 2.65$, *ns*).

Visual Analogue Mood Scale Scores

Mean VAMS scores obtained at each mood assessment point for each training group, along with associated standard deviations, are provided in Table 6.5. Three participants from the non-threat training group and one participant from the threat training group did not complete the final VAMS rating. The reason for their failure to complete these ratings was that the on-screen instructions directing participants to do so appeared a few seconds after the termination of the video, and these participants mistakenly believed prematurely that the experimental session had ended. These three participants have been excluded from Table 6.4, and from analyses of these scores.

Table 6.5

Visual Analogue Mood Scale Scores Obtained In Study 5

	Mood assessment points			
	Pre-training	Post-interpretive assessment	Pre-stress	Post-stress
		Anxiety scale		
Non-threat training group	29.7 (22.4)	33.6 (22.5)	42.9 (21.2)	42.2 (23.5)
Threat Training group	26.5 (21.4)	30.4 (21.8)	31.9 (19.7)	49.5 (25.0)
		Depression scale		
Non-threat training group	35.1 (21.2)	30.3 (18.1)	38.4 (20.9)	35.6 (18.8)
Threat Training group	30.6 (21.6)	26.5 (17.6)	27.5 (18.1)	39.8 (20.1)

Note. Standard deviations are provided in brackets. *N*=23 for the threat training group, and *N*=21 for the non-threat training group, for all means shown.

Immediate influence of the training procedure on mood state.

The central hypothesis to be tested in this study was that interpretive bias can causally mediate anxiety vulnerability, as assessed by reaction to the video stressor. However, it was first important to exclude the alternative possibility, that the training simply influenced mood state directly, rather than modifying emotional vulnerability. This was investigated by analyzing anxiety and depression VAMS scores obtained prior

to and immediately following the trials designed to train and assess interpretive bias. These anxiety and depression VAMS ratings were each subjected to a 2-way, mixed-design ANOVA, with one between-groups factor, training group (non-threat training group vs. threat training group), and one within-participants factor, mood assessment point (pre-training vs. post-interpretive assessment mood assessment point). No significant effects were obtained. There was no main effect of training group in the analyses of anxiety or depression scale scores (Fs<1). In addition, the main effect of assessment point was not significant for anxiety, $F(1, 46) = 2.45$, or for depression scale scores, $F(1,46) = 3.98$, *ns*. Most crucially, the training group by mood assessment point interaction was not significant for anxiety VAMS scores, $F(1, 42) = 0$, *ns*, or for depression VAMS scores, $F(1, 42) = 0.02$, *ns*, thus suggesting no evidence of any differential change in state anxiety or depression, between the groups, during the interpretive training/assessment trials themselves.

Influence of the training procedure on emotional vulnerability.

The central causal hypothesis was then assessed by analyzing VAMS scores obtained by each group before and after viewing the video stressor. Both anxiety and depression VAMS scores were subjected to a 2-way, mixed-design ANOVA, in which the between-groups factor was training group (threat training group vs. non-threat training group), and the within-participants factor was mood assessment point (pre-stress vs. post-stress mood assessment point).

The main effect of assessment point did not reach significance in this analysis, $F(1,42) = 3.59$, *ns.* The main effect of training group also was not significant, $F(1, 42) = .13$, *ns*. More centrally, in light of the evidence suggesting the induction of a group

difference in interpretive bias, the hypothesis that interpretive bias causally mediates anxiety vulnerability predicts a corresponding group difference in the magnitude of the anxiety elevation shown in response to this stressor, thus producing a training group by mood assessment point interaction. This interaction was indeed significant in the analysis of anxiety VAMS scores, $F(1, 42) = 4.13$, $p<.05$, and was the only significant effect revealed by the ANOVA carried out on these scores. The nature of this interaction was fully consistent with the predictions of the causal hypothesis, with only the threat training group displaying significant elevation in anxiety VAMS scores in response to the video stressor, $F(1, 22) = 10.3$, $p<.05$, with an increase in mean score of 17.6 points on the anxiety VAMS. In contrast, the non-threat training group displayed a non-significant decrease of 0.7 points in mean anxiety VAMS score from pre-stress to post-stress, $F(1, 20) = 0.01$, *ns*.

An ANOVA with the same factor structure, performed upon depression VAMS scores, also revealed a only a single significant effect (all other Fs$\leq$2). Again, this was a significant training group by mood assessment point interaction, $F(1, 42) = 5.10$, $p<.05$. The nature of this interaction was consistent with the suggestion that the group difference in interpretive bias causally mediated elevation in state depression, as well as state anxiety. Thus, only the threat training group displayed a significant increase in mean depression VAMS scores, increasing by 12.3 points from pre-stress to post-stress mood assessment points, $F(1, 22) = 8.32$, $p<.05$. In contrast, the non-threat training group displayed a non-significant decrease of 2.8 points in mean depression VAMS scores from pre-stress to post-stress, $F(1, 20) = 0.28$, *ns*.

Discussion

The pattern of selection times obtained in the interpretive assessment trials was consistent with the expectation that the fragment-completion interpretive training task would induce a group difference in interpretive bias, effectively replicating the pattern of findings obtained in Study 4. Thus, as in the previous study, participants receiving a non-threat-congruent training contingency, relative to those receiving a threat-congruent training contingency, subsequently displayed shorter response times to select non-threat, relative to threat associates of the preceding ambiguous primes. However, this was not the case when preceding primes were unambiguous, which would be expected if the training simply induced a group difference in the capacity to select associates of differentially valenced meanings. Furthermore, a follow-up analysis revealed that this group difference on ambiguous primes could be statistically distinguished from the groups' patterns of responding on unambiguous prime trials. Thus, these findings are consistent with the conclusion that the training induced a group difference in interpretive bias, with other accounts of these training effects that do not refer to the ambiguity of the primes quite strongly rejected excluded. This pattern of findings on selection time data was not compromised by the apparent presence of any differential speed-accuracy trade-off in selection of targets, given that no effects were obtained when accuracy data were analysed.

It is interesting to note that the pattern of selection times for targets following ambiguous primes indicated that, following training, the participants differed in their capacity to select targets related to the non-threatening meanings of the homographs, but did not differ in their capacity to select threat associates. One possible interpretation of this pattern of responses is that while the tendency to more or less frequently access

neutral interpretations may be trained, access to threat interpretations may be less amenable to such training. However, without appropriate additional baseline conditions (e.g., a pre-training measure of interpretive bias), it would be inappropriate to draw such precise conclusions regarding the changes in processing induced by the training.

Having obtained statistical support for the efficacy of the training procedure in producing a group difference in interpretive bias, the issue as to the causal contribution of such a difference to variations in anxiety vulnerability could then be addressed. The possibility that the training might have directly influenced mood state was excluded by the analyses of VAMS scores obtained prior to and following the training. This permitted the assessment of the central hypothesis, that interpretive bias can causally mediate anxiety vulnerability, through analyses of anxiety VAMS scores obtained prior to and following the video stressor. Consistent with this causal hypothesis, the group encouraged to interpret ambiguity in a non-threat-congruent fashion, relative to the group encouraged to interpret ambiguity in a threat-congruent fashion, displayed an amelioration of the expected state anxiety elevation in reaction to the video stressor. However, this group difference was not specific to anxiety reactions, being also evident on depression VAMS scores, which again revealed amelioration of depressive reaction to the video stressor for the non-threat training group, relative to the threat training group. Thus, these findings are consistent with the hypothesis that a negative interpretive bias may causally mediate vulnerability to experience elevation in states of anxiety and depression.

Conclusion

In summary, the present experiment provided evidence that interpretive bias can indeed causally mediate anxiety vulnerability, while suggesting that this causal role may not be specific to anxiety, perhaps influencing vulnerability to depression also. Following the induction of a detectable group difference in interpretive bias, the groups then displayed congruent differences in their vulnerability to experience elevation in state anxiety and depression in response to a subsequent stressor. Thus, interpretive bias was implicated as a causal mediator of negative emotional experience in reaction to this stressor. It was considered important to also determine whether or not converging support for this hypothesis would be forthcoming from the subsequent studies in this phase, since each of these employed a different combination of training and assessment methodologies to assess the hypothesis that interpretive bias can play a causal role in the mediation of anxiety vulnerability.

CHAPTER 7

ASSESSMENT OF THE CAUSAL HYPOTHESIS USING RELATEDNESS-JUDGMENT TRAINING AND ASSESSMENT TASKS

In Study 5, the induction of a group difference in interpretive bias gave rise to a congruent group difference in vulnerability to elevation in anxiety and depression, in the absence of any direct and differential influence of the training procedure upon mood state. Thus, direct support was provided for the hypothesis that interpretive bias can causally mediate anxiety vulnerability, though suggesting that the causal role of this bias may not be specific to anxiety. This chapter describes a converging test of the causal hypothesis provided by the second study of Phase 2. In this study, a different interpretive training procedure was employed to induce a group difference in interpretive bias. This was followed by the same interpretive assessment and stress procedures as employed in Study 5.

Study 6

As described in Chapter 1, an interpretive training variant of the relatedness-judgment task was developed by Grey & Mathews (2000, Experiment 3). A variant of this task was used in earlier studies of the present research program, but only to assess interpretive bias (Studies 4 and 5). In the present study, an interpretive training version of the relatedness-judgment task was employed in an attempt to induce a group difference in interpretive bias. The influence of this relatedness-judgment interpretive training was then assessed using an assessment (i.e., non-contingent) version of this same task in the interpretive assessment phase. Following this interpretive training and

assessment, participants were exposed to the video stressor and their emotional reactions to this stressor were rated. These ratings enabled assessment of the consequences of the training manipulation for anxiety vulnerability.

The interpretive training variant of the relatedness-judgment task employed a similar contingency manipulation to that used in the fragment-completion interpretive training procedure. Thus, in the interpretive training trials, for participants in the threat training group, the target display which followed each prime always included an associate of the threatening meaning of the prime, together with an unrelated threat word. The reverse was the case for the non-threat training group. For these participants, the target display always included an associate of the non-threatening meaning of the prime, together with an unrelated non-threat word. Of course, the rationale for the introduction of this contingency was the same as for the fragment-completion interpretive training. That is, it was anticipated that the relative advantage provided to the members of a particular training group by systematically accessing the appropriately valenced meanings of the ambiguous primes, would lead them to develop a congruent interpretive bias.

The assessment version of the relatedness-judgment task used in the current experiment was identical to that employed in the previous study. So, following training, participants received trials in which threat/non-threat homographs and unambiguous valenced primes each appeared with equal frequency. Homograph primes in this phase were followed by target displays that included related threat or non-threat associates equally often, in each case accompanied by valence-matched foils. In parallel, the unambiguous primes were threatening or non-threatening in meaning with equal frequency, and were followed by target displays which always included related

associates, along with valence-matched foils. It was anticipated that if this training procedure induced the intended group difference in interpretive bias, then this would be reflected in training-congruent differences in latency to identify the associates of the valenced prime meanings, but only for trials in which the primes were ambiguous. In these trials, it was predicted that the non-threat training group, relative to the threat training group, would display shorter response times to select non-threat relative to threat associates of the preceding ambiguous primes.

The final stress phase employed the same video stressor and mood measures as were employed in the previous study. Thus, as well as obtaining anxiety and depression VAMS prior to and following this stressor, these VAMS scores were also obtained prior to and following the trials designed to train and assess interpretive bias, to assess the alternative possibility that the training procedure might directly influence mood state. On the basis of the hypothesis that interpretive bias can causally mediate anxiety vulnerability, predictions for scores obtained prior to and following the stressor were contingent on the success of the interpretive training. It was predicted that if the interpretive training successfully induced a group difference in interpretive bias, then the non-threat training group, relative to the threat training group, would subsequently display a reduced degree of elevation in state anxiety in response to the video stressor, as indicated by anxiety VAMS scores. Further, it was reasoned that if the causal mediation of this bias is specific to anxiety, then this disproportionate elevation should be evident in anxious, but not in depressive VAMS scores.

Method

Overview

As with Experiment 5, this experiment involved three phases: An interpretive training phase, an interpretive assessment phase, and a stress phase.

Once again, in the interpretive training phase, participants were assigned either to the threat training group or to the non-threat training group, distinguished by the nature of the interpretive bias that the training contingency was designed to encourage. In the subsequent interpretive assessment phase, homograph primes and unambiguous valenced primes appeared with equal frequency. Ambiguous primes were followed with equal frequency by target displays involving related threat or non-threat associates, together with unrelated valence-matched words. In parallel, unambiguous primes were equally often threatening or non-threatening, in each case followed by related associates paired with unrelated valence-matched words. The main dependent variable was the participant's response time to select the target which was related to the prime in each trial of the interpretive assessment phase. Through analysis of these selection times, the success of the interpretive training was assessed.

As with Study 5, this study investigated the influence of the interpretive training upon mood state and emotional vulnerability, and this aspect of the design also considered the influence of the training group assignment (threat training group or non-threat training group). Mood ratings were obtained before and after the interpretive training/assessment phases and prior to and following the video stressor. The dependent variables here were scores on visual analogue mood scales (VAMS) assessing anxiety and depression, ranging from 1 to 100. Thus, the direct influence of the interpretive

training upon mood state during the interpretive training and assessment phases, and its influence upon subsequent emotional vulnerability, were each assessed.

Participants

The participants in this study were 24 male and 24 female psychology students who participated for course credit. There were an equal number of male and female participants in each training group. Their characteristics at test time are presented in Table 7.1.

Table 7.1

Mean Characteristics Of Participants In Study 6

Characteristic	Non-threat training group	Threat training group
STAI–T	36.3[a]	38.7
	(9.9)	(9.6)
STAI–S	38.6	36.2
	(16.4)	(8.1)
BDI–II	7.3	7.1
	(6.6)	(4.2)
Age (yrs)	17.8	18.8
	(1.5)	(4.0)

Note. Standard deviations are provided in brackets. *N*=24, in each training group.
STAI-T = State-Trait Anxiety Inventory, trait scale; STAI-S = State-Trait Anxiety Inventory, state scale; BDI-II = Beck Depression Inventory, 2nd edition.
[a] One participant from the non-threat training group did not complete the STAI–T, but completed all other measures.

The training groups did not differ significantly in state anxiety, as revealed by STAI-S scores, $F(1, 46) = 0.38$, *ns*, or in trait anxiety, as revealed by STAI–T scores, $F(1, 45) = 0.67$, *ns*, or in depression, as revealed by BDI-II scores, $F(1, 46) = 0.96$, *ns*, or in age, $F(1, 46) = 1.23$, *ns*.

Materials

State-Trait Anxiety Inventory and Beck Depression Inventory, second edition.

As with Experiment 5, this experiment employed the STAI–T (State-Trait Anxiety Inventory, trait scale), the STAI-S (State-Trait Anxiety Inventory, state scale), and the BDI–II (Beck Depression Inventory, second edition) to assess state anxiety, trait anxiety and depression, respectively. These measures were described in Chapter 6.

Visual analogue mood scales.

Once again, anxiety and depression VAMS (visual analogue mood scales; see Chapters 5 and 6) were used to assess state anxiety and state depression at four mood assessment points: Prior to, and directly following, the trials comprising the interpretive training/assessment phases (pre-training and post-interpretive assessment); and prior to and following the video stressor (pre-stress and post-stress).

Lexical stimuli.

The stimuli employed in practice trials, and in the interpretive training and interpretive assessment phases, were identical to those employed in Study 5, with two exceptions. Firstly, the word-fragments were not used because they were redundant for the current tasks. Secondly, an additional 160 unambiguous words with threatening meanings and 160 unambiguous words with non-threatening meanings were added to

the stimulus set, to be employed as valence-matched foils within the relatedness-judgment interpretive training task.

Computer hardware.

The hardware employed here was identical to that employed in Studies 4 and 5.

Experimental Tasks

Interpretive training phase: Relatedness-judgment interpretive training task.

A variant of the relatedness-judgment task described in Chapter 4 was employed here as a means of interpretive training. In this interpretive training variant, the allocation of stimuli across training trials created a training contingency paralleling that adopted for the fragment-completion interpretive training task employed in Studies 4 and 5 (see Chapters 4 and 6). Thus, the associate to be identified was always related to the threatening meaning of the prime for the threat training group, and was always related to the non-threatening meaning of the prime for the non-threat training group. In each trial, the target display contained the relevant associate and one of the valence-matched foils created for this purpose. As with the earlier fragment-completion interpretive training task, there were 160 trials in this present interpretive training phase, 128 of which involved the threat/non-threat homographs as primes and 32 of which involved unambiguous primes which were congruent with the valence of training group assignment. Allocation of stimuli from the critical *nonets* to this interpretive training task was handled in exactly the same way as for the fragment-completion interpretive training task described in the previous chapter. Thus, for each participant, 32 *nonets* were assigned to the interpretive training phase, with this allocation rotated within

training groups such that each of the *nonets* appeared in training an equal number of times.

Interpretive assessment phase: Relatedness-judgment interpretive assessment task.

This interpretive assessment task was identical to the assessment task variant employed in Study 5 (see Chapter 6) and the allocation of stimuli to this task in the present study was handled in the same way as in the previous experiment. Additionally, as for the previous study, the allocation of *nonets* across interpretive training and interpretive assessment phases was rotated across the participants. This was performed such that the assignment of different *nonets* to the interpretive training phase, and to the first and second half of trials in the interpretive assessment phase, was counterbalanced within each training group.

Stress phase: Video stressor.

The video stressor employed in Study 5, described in Chapter 6, was also used in this present study.

Procedure

The procedure was the same as for Study 5, with the exception that the relatedness-judgment interpretive training task was employed instead of the fragment-completion interpretive training task. Instructions provided to participants for completion of the training task were thus modified accordingly, being equivalent to the instructions provided for completion of the relatedness-judgment interpretive assessment task in Studies 4 and 5 (see Chapter 4). In all other respects, the procedure was identical to that of Study 6.

Results

Selection Times

Mean response times for trials in the interpretive assessment phase in which associates were correctly selected, under each experimental condition, are provided in Table 7.2[8].

Table 7.2

Mean Selection Times (ms) Obtained In Study 6

	Ambiguous prime		Unambiguous prime	
	Non-threat associate	Threat associate	Non-threat associate	Threat Associate
Non-threat training group	1785 (434)	1858 (553)	1522 (456)	1653 (548)
Threat training group	1933 (427)	1892 (437)	1587 (397)	1557 (435)

Note. Standard deviations are provided in brackets.

Selection time data were analysed using 2-way, mixed-design ANOVAs, in which the between-groups factor was training group (threat training group vs. non-threat training group) and the within-participants factor was target associate (threat associate vs. non-threat associate). For targets following unambiguous primes, this analysis revealed a training group by target associate interaction that reached significance in the analysis of log-transformed versions of these scores, $F(1, 46) = 4.24$,

[8] Once again, visual inspection revealed that the distribution of selection times was positively skewed. Therefore, these data were subjected to log transformation. Unless otherwise specified, the outcomes of analyses performed upon transformed data were equivalent to those of untransformed selection times.

p<.*05*, reflecting a relative speeding to select threat, relative to non-threat associates, for the threat training group, relative to the non-threat training group, following these unambiguous primes (see Table 7.2). Therefore, there was some indication that the training simply induced a group difference in the capacity to identify associates of differentially valenced primes, independent of the primes' ambiguity.

However, in contrast to the significant effects obtained in the previous experiment, the analysis of response times for targets following ambiguous primes did not reveal any significant effects. Most crucially, the target associate by training group interaction was not significant, $F(1, 46) = 1.31$, *ns*, thus providing no evidence that this relatedness-judgment interpretive training served to effectively induce a group difference in interpretive bias. No other significant effects were revealed by these analysis (all other *F*s<2).

Given that a significant training group x target associate interaction was revealed for unambiguous primes but not for ambiguous primes, a follow-up analysis was then performed in order to determine the degree to which the patterns of performance on trials involving each type of prime could be discriminated. Thus, an analysis was then performed which incorporated prime ambiguity (ambiguous prime vs unambiguous prime) as an additional factor. This analysis did not reveal a significant 3-way interaction of training group by target associate by prime ambiguity, $F(1, 46) = .134$, *ns*. In fact, the only significant effects revealed by this analysis were a main effect of prime ambiguity, $F(1, 46) = 64.8$, $p<.001$, and a training group by target associate interaction, $F(1, 46) = 4.5$, $p<.05$ (all other *F*s< 1, except the non-significant effect of prime ambiguity by valence, $F(1, 46) = 2.19$, *ns*). Thus, it appears that the pattern of

performance on ambiguous prime trials was not significantly different from that shown on unambiguous prime trials.

Selection Accuracies

The percentages of trials on which accurate responses were made in this interpretive assessment phase are provided in Table 7.3.

Table 7.3

Mean Selection Accuracies (%) Obtained In Study 6

	Ambiguous prime		Unambiguous prime	
	Non-threat associate	Threat Associate	Non-threat associate	Threat associate
Non-threat training group	83.7 (11.0)	87.7 (8.1)	93.7 (7.1)	90.0 (9.7)
Threat training group	88.7 (10.6)	86.8 (9.0)	95.5 (4.0)	92.8 (8.6)

Note. Standard deviations are provided in brackets.

Selection time was considered the main indicator of performance in this study. However, selection accuracies were analysed to ensure that the absence of significant variation across conditions in speed of responding, did not reflect the presence of systematic variation in accuracy of performance across these same conditions. Selection accuracy data were thus subjected to the same 2-way mixed design ANOVAs that included the between-groups factor training group (threat training group vs. non-threat training group) and the within-participants factor target associate (threat associate vs.

non-threat associate). The analysis of selection accuracies for targets following unambiguous primes revealed only one significant effect: a main effect of target associate, $F(1, 46) = 5.79$, $p<.05$, with participants on average more accurate to select the associate of the preceding prime when this was threatening, rather than non-threatening (94.6% vs. 91.4%). However, the training group by target associate interaction was not significant for these scores, $F(1, 46) = 0.13$, *ns*. No other significant effects were revealed by this analysis, or by the analysis performed on selection accuracies for targets following ambiguous primes (all Fs<2). Most importantly, the training group by target associate interaction again was not significant, $F(1, 46) = 1.91$, *ns.* Therefore, regardless of the nature of the prime, there was no indication that participants in the two training groups differed significantly in their patterns of accuracy on the relatedness-judgment interpretive assessment task. Thus, as for the analyses of selection times, there was no suggestion from these analyses that the relatedness-judgment interpretive training task served to induce a group difference in interpretive bias.

Visual Analogue Mood Scale Scores

Given the failure to establish a significant group difference in interpretive bias, there was no basis to predict, from the causal hypothesis under scrutiny, that this training procedure would serve to produce a group difference in anxiety vulnerability. Indeed, evidence of such a group difference in emotional vulnerability could suggest that some aspect of the training methodologies employed in these studies, other than their capacity to induce a group difference in interpretation, might underlie the induced group difference in emotional vulnerability observed in Study 5. The direct influence of

the training procedure upon mood state, and its influence upon emotional vulnerability, were each examined by analysing appropriate VAMS scores. Mean anxiety and depression VAMS scores obtained from each training group at each of the four mood assessment points are presented along with associated standard deviations in Table 7.4.

Table 7.4

Mean Visual Analogue Mood Scale Scores Obtained In Study 6

	Mood assessment points			
	Pre-training	Post-interpretive assessment	Pre-stress	Post-stress
		Anxiety scale		
Non-threat training group	33.1 (25.5)	36.5 (25.8)	32.8 (18.6)	43.3 (23.8)
Threat training group	33.6 (24.9)	36.4 (21.3)	33.2 (21.5)	52.7 (24.0)
		Depression scale		
Non-threat training group	34.2 (28.0)	25.4 (17.9)	30.6 (14.9)	39.7 (22.3)
Threat training group	34.7 (26.1)	26.1 (17.5)	33.2 (20.3)	42.0 (16.2)

Note. Standard deviations are provided in brackets.

Immediate influence of the training procedure on mood state.

Initial analyses assessed the direct influence of the training procedure on mood state. Thus, VAMS scores obtained on the anxiety scale directly prior to and following the trials comprising the interpretive training/assessment phases were entered into a 2-way mixed-design ANOVA, in which the between-groups factor was training group (non-threat training group vs. threat training group) and the within-participants factor was mood assessment point (pre-training vs. post-interpretive assessment). No significant effects were revealed by this analysis. Most importantly, the training group by mood assessment point interaction was not significant, $F(1, 46) = 0.01$, *ns,* thus providing no evidence that the interpretive training manipulation influenced state anxiety directly.

The same analysis employed for scores obtained for the depression scale (pre-training vs. post-interpretive assessment), revealed only a main effect of mood assessment point, $F(1, 46) = 4.52$, $p<.05$. On average, regardless of training group assignment, participants displayed a decrease in depression from before to after the interpretive training/assessment phases (34.4 to 25.8, respectively). The training group by mood assessment point interaction was not significant however, $F(1, 46) = 0.99$, *ns*, indicating that depression, like state anxiety, was not detectably influenced by training group assignment.

No other significant effects were revealed by these analyses (all other Fs<1).

Influence of the training procedure on emotional vulnerability.

Analyses were then performed to assess whether, despite the absence of any induced group difference in interpretive bias, these groups displayed any induced difference in emotional vulnerability. A 2-way, mixed-design ANOVA was performed

upon VAMS scores obtained on the anxiety scale, prior to and following the video stressor. In this analysis, the between-groups factor was training group (non-threat training group vs. threat training group) and the within-participants factor was mood assessment point (pre-stress vs. post-stress). The only significant effect revealed by this analysis was a main effect of mood assessment point, $F(1, 46) = 12.70$, $p<.01$, reflecting an overall increase in anxiety scale scores from before to after the presentation of the video stressor (33.0 to 48.0, respectively). Therefore, the stressor effectively elevated state anxiety for all participants. The 2-way interaction involving training group and mood assessment point was not significant, $F(1, 46) = 0.29$, *ns,* thus providing no statistical support for the presence of a training-linked difference in interpretive bias. Thus, the absence of a significant group difference in interpretive bias following training was in this study accompanied by the corresponding absence of a group difference in anxiety vulnerability.

The same analysis was repeated for depression VAMS scores obtained prior to and following the video stressor. Once again, the only significant effect was a main effect of mood assessment point, $F(1, 46) = 9.19$, $p<.01$, with participants on average showing an increase in depression scale scores (31.9 to 40.9), consistent with the expectation that this stressor would elicit an emotional response. Again, for depression as for anxiety VAMS scores, the training group by target associate interaction was not significant, $F(1, 46) = 0.01$, *ns*. Therefore, these groups also did not display a differential elevation in depression, indicating that the absence of an induced group difference in interpretive bias was accompanied by the absence of a group difference in vulnerability to negative emotion.

No other significant effects were revealed by these analyses (all other Fs<1).

Discussion

The response times recorded during the interpretive assessment phase did not serve to confirm the prediction that the training procedure would induce a group difference in interpretive bias. Specifically, while there was some indication that the training groups may have differed in their capacity to select differentially valenced associates of the unambiguous primes, the training groups did not display significantly distinct patterns of response time in identifying associates of the differentially valenced meanings of the homograph primes. It was thus not inconsistent with the causal hypothesis when the analyses of anxiety and depression VAMS scores could not confirm that the groups displayed differential emotional reactions to the stressor.

In consideration of the hypothesis that interpretive bias can causally mediate anxiety vulnerability, these findings can be contrasted with the findings of the previous study. In each case, the assessment of the causal hypothesis was contingent upon the confirmation of the success of the training in inducing a group difference in interpretive bias. In contrast to Study 5 in which the training induced a detectable group difference in interpretive bias, in the present study, the presence of this group difference was not statistically supported. Also, in this present study, a group difference in emotional reactivity to the stressor could not be confirmed, while in contrast, a significant training group difference was detected in the previous study. Given that the groups differed in their capacity to respond to differentially valenced targets, these findings do not support the suggestion that aspects of such training methodologies which differ between the training groups (such as the differentially valenced training stimuli) but which do not necessarily result in differential interpretive bias, are sufficient in themselves to influence vulnerability. Instead, these findings are consistent with the possibility that the

induced group difference in emotional vulnerability observed in Study 5 indeed resulted from the induced group difference in interpretive bias obtained in that previous experiment.

Of course, as is the case for null results in general, conclusions based on the non-significant findings of this study must be made with some reservation. Most directly, possibilities may be entertained that the effects of this training were sufficiently small as to escape detection on the assessments employed in this study, and/or that the assessments were themselves relatively insensitive. Indeed, trends in the data from the interpretive assessment phase were not inconsistent with the possibility that the training might have had some real, though undetected, influence upon interpretation. In particular, it was observed that the group difference shown in the capacity to select differentially valenced associates of unambiguous primes, did not significantly differ from that shown in the capacity to select associates of valenced primes. This could perhaps have eluded confirmation as a result of the relatively low degree of effectiveness of this particular training methodology. Specifically, for trials involving ambiguous primes, the non-threat training group were speeded for non-threat associates relative to threat associates by 73 ms. The reverse was the case for the threat training group, who were speeded for threat associates, relative to non-threat associates, following ambiguous primes, by about 41 ms. Thus, these patterns of selection time, though not statistically confirmed, were consistent with what was predicted in terms of the non-threat training group being speeded relative to the threat training group, for non-threat related, relative to threat related, associates.

Of course, the crucial prediction, derived from the causal hypothesis, is that if the training induces a group difference in interpretive bias, then a congruent group

difference in anxiety vulnerability should also be shown. In fact, trends in terms of means obtained in VAMS scores from prior to and following the stressor were in the direction suggested by this hypothesis, though again, of course, these trends could not be confirmed statistically. Specifically, for this pre-stress to post-stress interval, the non-threat training group's mean VAMS for the anxiety scale increased by 10.5 points, which was approximately half the increase of 19.5 points reported by the threat training group. Thus, though in each case these trends could not be supported statistically, they were minimally consistent with the direction of effects predicted by key hypotheses regarding the manipulation of interpretive bias, and subsequent variation in anxiety vulnerability.

An additional account of these null findings which may be worth considering is that a group difference in interpretive bias, however effectively induced by the interpretive training procedure, was not sensitively detected on this particular interpretive assessment task. If this were the case, then the failure for these suggestive trends in selection times to reach significance might have resulted from the insensitivity of the task. This is interesting to consider, in particular in light of some suggestions made in Chapter 5 regarding findings from Phase 1 of this research. It will be recalled that the successful manipulation of interpretive bias in Study 4, using the fragment-completion training task, was preceded by a failure to obtain such a training effect using an alternative variant of this same fragment-completion task in Study 3. One of the accounts of this discrepancy, the *training-assessment distinctiveness* account, is premised on the notion that training effects are more reliably detected on assessments which involve distinct task requirements, under which conditions irrelevant practice effects may be less likely to obscure induced differences in interpretation. In this present

study, the same task was employed in interpretive training and interpretive assessment phases. In contrast, the *training-assessment* distinctiveness account suggests that interpretive training effects should be most sensitively detected in those studies in which a different task is employed in interpretive assessment from that employed in interpretive training, as was the case for Study 5, in which a group difference in interpretive bias was successfully detected.

This latter explanation for the failure of these findings to reach significance was directly tested in Study 7. In this following experiment, the present relatedness-judgment interpretive training task was used, but a variant of the fragment-completion task was instead employed in the interpretive assessment phase. Of course, the suggestion that a real induced group difference in interpretive bias was poorly detected in the present study, raises the question as to the anticipated emotional consequences of this effect. If it were indeed the case that the training in this present study induced a real group difference in interpretive bias, but this induction did not have detectable consequences for anxiety vulnerability, then this would run contrary to the causal hypothesis.

Regardless of their plausibility, such alternative accounts of non-significant trends as those provided above, are necessarily speculative. A more conservative conclusion at this point would be that the findings obtained in this study are consistent with the hypothesis that interpretive bias can causally mediate anxiety vulnerability, though not providing direct support for this hypothesis. Exposure to training materials and associated task conditions, in the absence of any significant influence upon interpretation, was not sufficient to causally mediate anxiety vulnerability, in terms of anxious reaction to a stressor, to a detectable degree. Only when interpretive bias was

directly modified, as in Study 5, could a group difference in anxiety vulnerability subsequently be inferred. Thus, the findings of the present study are not inconsistent with the causal hypothesis under scrutiny, and provide no support for alternative explanations, contrary to the causal hypothesis, for the findings of the previous study.

CHAPTER 8

ASSESSMENT OF THE CAUSAL HYPOTHESIS USING RELATEDNESS-JUDGMENT TRAINING AND FRAGMENT-COMPLETION ASSESSMENT TASKS

In Study 6, a training variant of the relatedness-judgment task was not found to induce a detectable group difference in interpretive bias, as assessed in terms of response time to identify associates of ambiguous primes in an assessment version of the same relatedness-judgment task. This training procedure also did not induce differential anxious reactivity to a video stressor. Though these findings are not inconsistent with the hypothesis that interpretive bias causally mediates anxiety vulnerability, the causal hypothesis would be assessed more directly if a training group difference in interpretive bias was successfully induced and subsequently detected, as in Study 5.

The study described in this chapter represented the third experiment in this series designed to assess the causal hypothesis and used the same basic design as employed in the previous two studies. The present study also used the same relatedness-judgment interpretive training task employed in Study 6. However, in contrast to Study 6, the success of this training was here assessed using the fragment-completion task, as opposed to the relatedness-judgment task, as a method of interpretive assessment. Thus, this study provided a second appraisal of the effectiveness of this relatedness-judgment training task, on a task which it was suggested in Chapter 7 might be more sensitive to group differences in interpretive bias induced by the relatedness-judgment training. Following interpretive training and assessment, the same stressor and

mood state assessments were employed to assess the consequences of this training for anxiety vulnerability.

Study 7

As with the previous two studies, Study 7 was comprised of three distinct phases: An interpretive training phase, an interpretive assessment phase, and a stress phase. In the interpretive training phase, as in Study 6, participants in each training group completed one of two training variants of the relatedness-judgment task. On each trial, their task was to indicate which of two possible words was an associate of the prime. As in the previous study, for those assigned to the threat training group, every target associate was related to the threatening meaning of the corresponding prime, with the intention of encouraging the development of a threat-congruent interpretive bias. For those assigned to the non-threat training group, the contingency was reversed, with every target associate being related to the non-threatening meaning of the prime, to encourage participants to develop a non-threat-congruent interpretive bias.

The present study diverged from Study 6 by assessing the success of this interpretive training using the fragment-completion task in the interpretive assessment phase. The interpretive assessment variant adopted the same task parameters (SOA, etc) employed in the fragment-completion interpretive *training* task used in Studies 4 and 5. However, the training contingency was now removed, to create an assessment task that paralleled the relatedness-judgment interpretive assessment task employed in the previous three studies. Thus, in this interpretive assessment phase, participants received equal numbers of trials in which the prime was a threat/non-threat homograph, or an unambiguous valenced word. Once again, the associates from which word-fragment

targets were derived were equally often related to the threatening or non-threatening meanings of homograph primes. Unambiguous primes were threatening or non-threatening with equal frequency, followed in each case by word-fragment targets derived from related associates.

Under these circumstances, it was reasoned that a group difference in the capacity to identify associates of the differentially valenced prime meanings, irrespective of the ambiguity of the primes themselves, would be revealed by a group difference in relative completion scores for word-fragments derived from associates of the non-threatening relative to threatening meanings of the unambiguous primes. On the other hand, a group difference in interpretive bias would be revealed by a group difference in relative completion scores for word-fragments derived from associates of the non-threatening, relative threatening meanings of the primes, only when these primes were threat/non-threat homographs.

As in the previous two experiments, the stress phase followed the interpretive training and interpretive assessment phases, and in this phase participants again were presented with the video stressor employed in Studies 5 and 6. State anxiety and state depression were again rated by participants on VAMS scales at four points in the study: directly prior to and following the trials comprising the interpretive training/assessment phases, and directly prior to and following the video stressor. Thus, once again, this design assessed the possibility that the training would influence mood state directly, as well as the possibility that it would serve to modify emotional vulnerability.

The hypothesis that interpretive bias can causally mediate anxiety vulnerability, provided the same predictions here as for the previous two studies. It was predicted that if the training succeeded in inducing a group difference in interpretive bias, then there

would be a parallel group difference in the tendency to display elevation in anxious, but not in depressive, mood state, in response to the stressor. It was further predicted that if interpretive bias causally mediates vulnerability to depression, as well as anxiety, then this group difference in reactivity would be shown on depressive, as well as anxious, mood scales.

Method

Overview

This study followed the basic structure of the two previous studies, with three principal phases: an interpretive training phase, an interpretive assessment phase, and a stress phase. As in Study 6, the interpretive training phase employed the relatedness-judgment task. Once again, participants were assigned to one of two training groups (the threat training group or the non-threat training group), which differed in terms of the valence of target associates which participants were required identify to as being related to preceding primes.

During the subsequent interpretive assessment phase which employed the fragment-completion interpretive assessment task, trials involving threat/non-threat homographs (ambiguous prime trials) and trials involving unambiguous primes were each presented in equal numbers. The homograph primes were followed in equal numbers of trials by word-fragment targets derived from associates of each valence of meaning. Similarly, the unambiguous primes, which were equally often threatening or non-threatening in meaning, were always followed by word-fragments derived from associates of related meaning. The dependent variable in this interpretive assessment phase was the percentage of trials, within each condition, on which the fragment was

completed. Thus, group differences in interpretive bias would be revealed by group differences in the relative completion scores for fragments related to the threatening and non-threatening meanings of ambiguous homograph primes.

To investigate the influence of this interpretive training procedure upon mood state and emotional vulnerability, mood ratings were once again obtained at four mood assessment points. Ratings were obtained prior to and following the interpretive training/assessment phases, to assess the direct influence of the training upon mood state; and prior to and following the stress phase, to assess the influence of the interpretive training procedure upon the subsequent expression of emotional vulnerability. The dependent variables were again scores on two visual analogue mood scales (VAMS), ranging from 1 to 100, which assessed state anxiety and state depression, respectively.

Participants

Twenty-four male and 24 female psychology students participated in the experiment in exchange for course credit, with an equal number of each gender in each training group. The participants were randomly assigned to training groups, apart from the gender constraint. The characteristics of participants assigned to each training group are provided in Table 8.1.

Between-groups ANOVAs revealed no statistically significant group differences in state anxiety, as measured on the STAI-S, $F(1, 46) = 0.06$, *ns*, or in depression, as assessed on the BDI-II, $F(1, 46) = 0.53$, *ns*. However, unexpectedly, the groups differed significantly in terms of trait anxiety, as indicated by STAI-T scores, $F(1, 46) = 4.74$, $p<.05$, with the non-threat training obtaining a greater mean STAI-T score than the

threat training group (43.0 vs. 37.2). The groups also differed significantly in terms of age, $F(1, 46) = 4.56$, $p<.05$, with the non-threat training group significantly younger than the threat training group (17.8 vs. 19.4). The possible implications of these chance differences are further considered in the Discussion section of this chapter.

Table 8.1

Mean Characteristics Of Participants In Study 7

Characteristic	Non-threat training group	Threat training group
STAI–T	43.0	37.2
	(9.5)	(8.8)
STAI–S	36.7	36.1
	(7.2)	(9.3)
BDI–II	11.0	9.3
	(6.4)	(9.2)
Age (yrs)	17.8	19.4
	(1.1)	(3.5)

Note. Standard deviations are provided in brackets. $N = 24$ for each training group.
STAI–T = State-Trait Anxiety Inventory, trait scale; STAI-S =state-trait anxiety inventory, state scale;
BDI–II = Beck Depression Inventory, second edition.

Materials

Lexical stimuli.

The stimuli employed in this study were identical to those employed in Studies 4, 5, and 6, with the additional inclusion of the foils used in the relatedness-judgment interpretive training task as described in Study 6.

State-Trait Anxiety Inventory and Beck Depression Inventory, *second edition.*

As in previous studies, the STAI-S (State-Trait Anxiety Inventory, state scale) STAI-T (State-Trait Anxiety Inventory, trait scale), and the BDI-II (Beck Depression Inventory, second edition) were used to assess state anxiety, trait anxiety, and depression, respectively (see Chapter 6 for details).

Visual analogue mood scales.

Participants rated their mood on the same anxiety and depression VAMS that were used in Studies 5 and 6 (see Chapters 5 and 6 for details). These ratings were made at four mood assessment points: pre-training, post-interpretive assessment, pre-stress and post-stress.

Computer hardware.

The hardware employed in this study was the same as was employed in relevant parts of Studies 5 and 6.

Experimental Tasks

Interpretive training phase: Relatedness-judgment interpretive training task.

The relatedness-judgment interpretive training task, described in Chapter 7, was again used in the interpretive training phase of this study.

Interpretive assessment phase: Fragment-completion interpretive assessment task.

The parameters within this fragment-completion interpretive assessment task were identical to those associated with the fragment-completion interpretive training task used in Studies 4 and 5, with respect to SOA, position of prime and target, and task requirements (see Chapter 4). However, as with the relatedness-judgment interpretive

assessment task used in Studies 5 and 6, in this task there were equal numbers of trials in which the prime was either a threat/non-threat homograph or an unambiguous valenced prime. Additionally, for the homograph primes, the associates from which the word-fragments were derived were equally often related to threatening and non-threatening prime meanings. Similarly, the unambiguous primes were equally often threatening or non-threatening, and were always followed by word-fragments derived from related associates.

There were 64 assessment trials. With the exception of the unrelated foils, which were not employed in this task, the distribution of the critical *nonets* to ambiguous and unambiguous prime conditions, and to trials involving threat and non-threat associates, followed the conventions adopted for the relatedness-judgment interpretive assessment task.

Additionally, as for the previous studies, the allocation of particular *nonets* to the interpretive training and assessment phases was rotated across participants. This meant that within each training group, the different *nonets* appeared equally often within the interpretive training phase, and within the first and second half of trials in the interpretive assessment phase.

Video stressor.

The same video stressor employed in Studies 5 and 6 was once again used to evoke an emotional response from participants (see Chapter 6).

Procedure

This study employed the relatedness-judgment task in the interpretive training phase and the fragment-completion task in the interpretive assessment phase. In all

other respects the procedural details for this study were the same as for the previous two studies.

Results

Completion Scores

Mean completion scores for word-fragments under different conditions, obtained during the interpretive assessment phase, are presented, along with corresponding standard deviations, in Table 8.2.

Table 8.2

Mean Completion Scores (%) Obtained In Study 7

	Ambiguous prime		Unambiguous prime	
	Non-threat associate	Threat associate	Non-threat associate	Threat associate
Non-threat training group	59.4 (16.8)	56.9 (13.9)	65.4 (17.7)	64.3 (16.0)
Threat training group	55.6 (16.9)	58.6 (15.9)	61.3 (18.4)	65.5 (17.6)

Note. Standard deviations are provided in brackets.

Completion scores for fragments presented within ambiguous and unambiguous prime trials were analyzed using 2-way mixed-design ANOVAs. The factors in each analysis were training group (threat training group vs. non-threat training group) and target associate (threat associate vs. non-threat associate). Most importantly, the training

group by target associate interaction was not significant either in unambiguous prime trials, $F(1, 46) = 1.48$, *ns*, or in ambiguous prime trials, $F(1, 46) = 1.14$, *ns*. Thus, these analyses yielded no evidence to suggest that the training procedure induced a group difference in interpretive bias in this study. There were no other significant effects revealed by these analyses (all other Fs<1).

Completion Times

Table 8.3 presents completion times for fragments presented in the interpretive assessment phase, along with corresponding standard deviations[9].

Table 8.3

Mean Completion Times (ms) Obtained In Study 7

	Ambiguous prime		Unambiguous prime	
	Non-threat Associate	Threat associate	Non-threat associate	Threat associate
Non-threat training group	2202 (492)	2402 (498)	1970 (476)	2103 (455)
Threat training group	2217 (570)	2210 (613)	1955 (522)	2130 (458)

Note. Standard deviations are provided in brackets.

Completion times were analyzed to ensure that the absence of systematic variation across the experimental conditions in terms of accuracy (i.e., completion

[9] As for other studies in this series, these completion times were also analyzed following log transformation in order to correct for skewness in the distribution. Analyses of these transformed data revealed the same pattern of effects in all cases.

score), did not reflect the translation of differential completion difficulty into systematic variations in *speed* of fragment completion. Thus, completion times were subjected to the same 2-way mixed design ANOVAs. The between groups factor was training group (threat training group vs. non-threat training group), and the within-participants factor was target associate (threat associate vs. non-threat associate).

As for the completion score data, the training group by target associate interaction was not significant, either for fragments following unambiguous primes, $F(1, 46) = 0.07$, *ns*, or for those following ambiguous primes, $F(1, 46) = 0.98$, *ns*. Thus, irrespective of the nature of the prime, there was no evidence that the groups showed differential speed in completing target fragments related to threatening and non-threatening prime meanings, suggesting that the training induced no detectible group difference in interpretive bias. No other significant effects were revealed by these analyses (all $Fs<1$ except for the non-significant main effect of target associate in unambiguous prime trials, $F(1, 46)=3.73$, *ns*).

Visual Analogue Mood Scale Scores

Mean anxiety and depression VAMS scores obtained at each mood assessment point, for each training group, together with corresponding standard deviations, are provided in Table 8.4. The direct influence of the training procedure upon mood state was appraised first, by analyzing VAMS scores obtained both prior to and following the trials comprising the interpretive training/assessment phases. The influence of the training procedure upon emotional vulnerability was then assessed by examining the groups' mood state elevation in response to the video stressor.

Table 8.4

Mean Visual Analogue Mood Scale Scores Obtained In Study 7

	Mood assessment points			
	Pre-training	Post-interpretive assessment	Pre-stress	Post-stress
		Anxiety scale		
Non-threat training group	25.3 (19.5)	33.5 (21.9)	46.1 (24.8)	41.7 (18.3)
Threat Training group	30.9 (27.8)	32.2 (22.2)	39.5 (23.4)	42.6 (19.9)
		Depression scale		
Non-threat training group	38.2 (26.5)	33.5 (21.1)	39.4 (18.3)	39.6 (16.8)
Threat Training group	33.6 (26.4)	27.2 (21.4)	31.9 (19.3)	39.3 (16.1)

Note: Standard deviations are provided in brackets.

Immediate influence of the training procedure on mood state.

The direct influence of the training procedure upon state anxiety and state depression was assessed by subjecting the relevant VAMS scores obtained directly before and after the interpretive training, to 2-way, mixed-design ANOVAs. In each case, the between-groups factor was training group (threat training group vs. non-threat

training group) and the within-participants factor was mood assessment point (pre-training vs. post-interpretive assessment). These analyses did not reveal any significant effects. Specifically, there was no significant of assessment point, for the anxiety scale, $F(1, 46) = 2.85$, *ns*, or for the depression scale, $F(1, 46) = 2.17$, *ns.* There also was no main effect of training group in either case (Fs <1). Most importantly, the training group by mood assessment point interaction was not significant for the anxiety scale scores, $F(1, 46) = 0.13$, *ns*, or for depression scale scores, $F(1, 46) = 0.05$, *ns*. Thus, there was no indication that the training manipulation *directly* influenced state anxiety or depression.

Influence of the training procedure on emotional vulnerability.

The influence of training manipulation upon anxiety vulnerability was then assessed by analyzing anxiety VAMS scores obtained prior to and following the stressor in a similar mixed-design, 2-way ANOVA. This analysis included training group as a between-groups factor, and mood assessment point (pre-stress vs post-stress) as a within-participant factor. Of course, given the failure to detect an induced group difference in interpretive bias, the causal hypothesis did not provide the prediction that a group difference in anxiety vulnerability would be detected. This analysis did not reveal any significant effects (all Fs<2). Most relevantly, the training group by mood assessment point interaction was not significant, $F(1, 46) = 1.39$, *ns*, thus providing no indication that the training manipulation modified anxiety vulnerability.

A second ANOVA, carried out on depression VAMS scores, incorporated the same factors. This analysis also revealed no significant effects (all Fs<2), most relevantly a non-significant training group by mood assessment point interaction, $F(1, 46) = 1.52$, *ns*. Thus, there was no evidence from these analyses that the training

manipulation served to modify vulnerability to elevation in state depression, as for state anxiety.

Discussion

In this study, analyses of responses recorded in the interpretive assessment phase did not confirm the expectation that the training would create a group difference in interpretive bias. Specifically, the training groups did not differ significantly in terms of their propensity to complete target fragments related to the threat, relative to non-threat meanings of the ambiguous primes. Given this failure to induce a group difference in interpretive bias, the hypothesis that interpretive bias causally mediates anxiety vulnerability actually provided no basis for predicting that the training manipulation would induce a group difference in anxiety vulnerability. Indeed, were such a group difference in vulnerability observed under these circumstances, it might provide support for an alternative interpretation of the findings from Study 5. Specifically, it might be implied that the differential emotional vulnerability induced by the training procedure in Study 5, was not functionally related to the induced group difference in interpretive bias, instead being related to some other aspect of the training manipulation (e.g., the exposure of the groups to differentially valenced training materials). In the present study, however, the analyses of mood ratings did not indicate the presence of a group difference in vulnerability to anxiety, or depression, following the training. That is, analyses of VAMS scores obtained by the training groups prior to and following the stressor revealed no evidence of a group difference in elevation of anxiety VAMS scores, or of depression VAMS scores.

Of course, as for the findings from the Study 6, it is important to recognise the usual limitations in drawing conclusions on the basis of these null findings. Thus, it may be worth critically evaluating possible alternative explanations of these findings in terms variation in the sensitivity of interpretive assessment procedures, and/or variation in the magnitude of training effects.

One alternative explanation provided for the similar non-significant findings of Study 6 concerned the potential for insensitivity in the arrangement of interpretive training and assessment tasks used in that study. It will be recalled that this hypothesis was derived from an account originally provided to explain the discrepancy between the findings of Studies 3 and 4 in Phase 1. Specifically, the *training-assessment distinctiveness* account suggests that training effects upon interpretive bias may be more sensitively detected using a different task in interpretive assessment from that employed during training. Thus, it was suggested that in Study 6, where the relatedness-judgment task was used in interpretive training, the consequences of this training for interpretation may not have been adequately detected because the relatedness-judgment task was also used to assess the effectiveness of this training.

In fact, this present study provided a direct assessment of this suggestion by investigating the effects of the relatedness-judgment interpretive training upon performance on a fragment-completion task in the interpretive assessment phase. Thus, the *training-assessment distinctiveness* account was not supported by the failure to detect significant group differences in interpretive bias in this study. Indeed, taken together, this study and Study 6, each employing a different interpretive assessment task, both failed to detect a significant group difference in interpretive bias following this relatedness-judgment training. It therefore seems unlikely that explanations framed

in terms of the sensitivity of a particular interpretive assessment task, alone, or in interaction with the training task, can adequately account for the failure to detect induced group differences in interpretive bias in these studies.

Another possibility is that the training might have had a real influence upon interpretive bias, and by hypothesis upon vulnerability, but that the small magnitude of this training effect meant it could not be detected statistically. This possibility might more plausibly account for the findings of this study and of Study 6 together. This is plausible in particular because of the non-significant trends in the findings of this present study, which were consistent with the predicted direction of effects, and were also similar to those trends observed in the interpretive assessment and stress phases in the previous study. Once again, the responses in the interpretive assessment phase were not inconsistent with what would be anticipated had the training induced a group difference in interpretive bias. Specifically, the non-threat training group displayed a 2.5% advantage in mean completion score for non-threat, relative to threat related word-fragments following the ambiguous primes. This was in contrast to the threat training group, which displayed a 3.0% advantage in the opposite direction. Similarly, the trends in terms of the training groups' mean elevations in anxiety and VAMS scores, in response to the stressor, were consistent with the direction of effects that would be predicted if the training procedure had induced a group difference in anxiety vulnerability. Thus, the non-threat training group displayed a 4.4 point *reduction* in mean anxiety VAMS score from pre-stress to post-stress mood assessment points. In contrast, the threat training group displayed an increase in mean anxiety VAMS score of 3.1 points. Of course, the failure to confirm these trends as statistically significant in

each case precludes strong conclusions from being drawn, but it is interesting to note these trends were not inconsistent with the predicted directions of effects.

An additional, speculative, account for the non-significant outcome of this study relates to the questionnaire measures provided prior to the interpretive training, which indicated that the groups differed, from the outset, in terms of trait anxiety. It is certainly possible that this unexpected difference, which could only have arisen by chance as a result of the random allocation procedure, might have influenced the outcome of the study. This is a plausible possibility given the well established association between trait anxiety and interpretive bias. As with all studies in this second phase, this study was designed to detect a group difference in interpretive bias following the interpretive training, but did not exact any information as to the interpretive biases of these groups prior to the training. Given that participants in the non-threat training group were characterised by a higher mean level of trait anxiety than was the case for participants in the threat training group, it is possible that the non-threat training group, relative to the threat training group, might have entered into the interpretive training possessing a tendency to interpret the homograph primes in a threat-congruent manner. Thus, it could be the case that the non-threat training ameliorated this pre-existing threat-congruent bias in this group of participants. The consequence of this amelioration, following training, could still be a small, non-significant group difference in interpretive bias in the expected direction, despite a large *change* in bias during the interpretive training phase, consistent with training condition. It may also be that this non-threat training reversed a tendency for this more highly trait anxious group of participants to display a disproportionate anxious reaction to the subsequent stressor.

Thus, this training may have in fact influenced interpretive bias, and anxiety vulnerability, each to greater degrees than could be detected.

Of course, such alternative interpretations of these findings are extremely speculative, and it may be circumspect to adopt the more conservative interpretation, that this particular training manipulation did not influence interpretive bias. Thus, a more parsimonious conclusion would be simply that the relatedness-judgment task may be a less effective method of manipulating interpretive bias than the fragment-completion interpretive training task. This latter task was twice demonstrated to induce a group difference in interpretation, in terms of capacity to identify differentially valenced associates of homograph primes. In contrast, two attempts to induce group differences in interpretive bias using the relatedness judgment training task both yielded no significant effects, with a similar sample-size in each study.

Despite this failure in each study using the relatedness-judgment training task to induce a significant group difference in interpretive bias, the causal hypothesis is not compromised by these findings, because of the observation that each manipulation also failed to induce a significant group difference in the intensity of anxious reactivity to the stressor.

The next chapter reports the final study in the series of four conducted to assess the causal hypothesis, which employed the fragment-completion interpretive training task used in Studies 4 and 5. Therefore, since it was likely that this task would once again induce a group difference in interpretive bias, it was also considered likely that this would enable the direct assessment of the prediction that anxiety vulnerability would consequently be modified.

CHAPTER 9

ASSESSMENT OF THE CAUSAL HYPOTHESIS USING FRAGMENT-COMPLETION TRAINING AND ASSESSMENT TASKS

The study described in this chapter represented the fourth in the series designed to empirically assess the hypothesis that interpretive bias can causally mediate anxiety vulnerability. The previous two studies which employed the relatedness-judgment interpretive training task did not obtain evidence of induced group differences in interpretive bias. Consistent with the causal hypothesis, these studies also did not obtain evidence for training-linked differences in anxiety vulnerability.

In contrast to those previous two studies, this present study employed the fragment-completion task, rather than the relatedness-judgment task, in interpretive training. The fragment-completion task was previously demonstrated to successfully induce a group difference in interpretive bias (in Studies 4 and 5), as measured using a different task in interpretive assessment. In this study, a non-contingent version of this fragment-completion task was instead used to assess the success of the interpretive training. Following this, the consequences of this training for anxiety vulnerability were again investigated using the video stressor.

Study 8

As with all studies in Phase 2, this experiment incorporated an interpretive training phase, an interpretive assessment phase, and a stress phase. As always, in the interpretive training phase, participants were assigned to the threat training group or to the non-threat training group. In the case of the fragment-completion task used in this

study, this training group assignment determined the nature of the word-fragments which followed the primes during the interpretive training phase. Thus, the groups of participants who received the different training contingencies were encouraged to adopt opposing interpretive biases in their employment of the primes to facilitate completion of the word-fragment targets. The threat training group was presented with threat-related word-fragment targets and so was encouraged to interpret the ambiguous primes in a threat-congruent fashion. The non-threat training group was presented with non-threat related word-fragment targets and thus was encouraged to interpret the ambiguous primes in a non-threat-congruent fashion.

The success of this interpretive training was assessed in the subsequent interpretive assessment phase, which involved the fragment-completion interpretive assessment task. During this interpretive assessment phase, participants were presented with an equivalent number of trials in which the prime was a threat/non-threat homograph or an unambiguous, valenced word. In trials involving the homograph primes, fragments were equally often derived from associates of threatening or non-threatening prime meanings. Similarly, for trials involving unambiguous primes, these primes were equally often threatening or non-threatening, and the targets were always derived from associates of these prime meanings.

Of course, as in previous studies, the unambiguous primes were included to address the possibility that the training might simply induce a group difference in the ease of processing associates of differentially valenced prime meanings without actually influencing interpretive bias. It was anticipated that this would be reflected in an advantage for the non-threat training group, relative to threat training group, in completing non-threat, relative to threat, word-fragments, even in trials involving

unambiguous primes. The desired outcome, that the training would induce a group difference in interpretive bias, instead would be reflected in a group difference in completing non-threat, relative to threat, word-fragment targets, only for trials where these targets followed threat/non-threat homograph primes.

As in the other studies in Phase 2, the stress phase followed the interpretive training and interpretive assessment phases. During this final phase, the same video stressor as employed in Studies 5, 6 and 7, was presented in order to elicit a negative emotional reaction from participants. As always, participants completed anxiety and depression VAMS ratings prior to and following this stress induction, as well as prior to and following the interpretive training/assessment trials. These latter ratings permitted the direct influence of the training upon mood state to be assessed.

As for the other studies in Phase 2, under these experimental conditions, the assessment of the hypothesis that interpretive bias can causally mediate anxiety vulnerability was contingent upon the success of the training in inducing a group difference in interpretive bias. It was predicted that if the training groups could be induced to differentially interpret emotionally ambiguous stimuli, then the non-threat training group, relative to the threat training group, would display reduced elevation in state anxiety in reaction to the video stressor. It was reasoned that if the causal influence of interpretive bias is not specific to anxiety vulnerability, but also contributes to vulnerability to depression, then this group difference would be displayed in ratings of state depression, as well as of state anxiety, in reaction to the stressor.

Method

Overview

Once again, this study incorporated three phases: An interpretive training phase, an interpretive assessment phase, and a stress phase.

During the interpretive training phase, participants were assigned to the threat training group or to the non-threat training group. This group assignment determined the valence of the word-fragment targets which followed primes during this phase, and thus the type of interpretive bias encouraged.

In the interpretive assessment phase, threat/non-threat homographs were followed with equal frequency by word-fragment targets derived from threat and non-threat associates. Similarly, the unambiguous primes could be threatening or non-threatening with equal probability, and were always followed by word-fragment targets derived from related associates. The dependent variable in this interpretive assessment phase was the percentage of fragments completed by participants.

The interpretive assessment phase was followed by the stress phase in which participants were presented with the video stressor. As always, VAMS (visual analogue mood scale) scores were obtained from participants in each training group. These VAMS ratings assessed the direct influence of the training upon mood state (pre-training and post-interpretive assessment), as well as its impact upon subsequent emotional vulnerability as indicated by emotional reaction to the video stressor (pre-stress and post-stress). Both anxiety and depression VAMS scores were recorded. The dependent variable was the intensity of the mood state score obtained in each case, which ranged from a minimum of 1 to a maximum of 100.

Participants

The participants were 48 first-year undergraduate psychology students who participated in the experiment in exchange for course credit. Participants were selected at random from a pool of volunteers. Twelve male and 12 female participants were allocated to each training group. The characteristics of the participants in these training groups, obtained at time of testing, are presented in Table 9.1.

Table 9.1.

Mean Characteristics Of Participants In Study 8

Characteristic	Non-threat training group	Threat training group
STAI–T	40.3	38.8
	(10.1)	(10.4)
STAI–S	38.2	35.2
	(9.9)	(8.3)
BDI–II	10.3	9.2
	(8.7)	(8.2)
Age (yrs)	18.0	19.7
	(1.6)	(4.3)

Note. Standard deviations are provided in brackets. *N*= 24 for each training group.
STAI-S = State-Trait Anxiety Inventory, state scale; STAI-T = State-Trait Anxiety Inventory, trait scale;
BDI-II = Beck Depression Inventory, second edition.

The training groups did not differ significantly in age, $F(1, 46) = 3.62$, *ns;* nor in terms of trait anxiety, state anxiety, or depression, as indicated by scores obtained, respectively, on the STAI-T, $F(1, 46) = 0.27$, *ns*; the STAI-S, $F(1, 46) = 1.29$, *ns*; and the BDI-II, $F(1, 46) = 3.62$, *ns.*

Materials, Tasks and Procedure

This study employed the same fragment-completion interpretive training task as was used in Studies 4 and 5, and assessed the influence of this training using the fragment-completion interpretive assessment task employed in Study 7. In all respects, the materials, tasks, and procedure were identical to those employed in the relevant previous studies.

Results

Completion Scores

Mean fragment completion scores and corresponding standard deviations, obtained by participants in each training group within each condition of the interpretive assessment phase, are provided in Table 9.2.

Table 9.2

Mean Completion Scores (%) Obtained In Study 8

	Ambiguous prime		Unambiguous prime	
	Non-threat associate	Threat associate	Non-threat associate	Threat associate
Non-threat training group	61.0 (14.9)	60.5 (13.7)	69.2 (14.7)	64.8 (13.4)
Threat training group	56.7 (16.4)	67.5 (17.1)	67.9 (19.1)	67.7 (20.3)

Note. Standard deviations are provided in brackets.

The completion scores for the interpretive assessment phase were analysed using 2-way mixed-design ANOVAs which incorporated as factors, training group (threat training group vs. non-threat training group) and target associate (threat associate vs. non-threat associate). For targets following unambiguous primes, the training group by target associate interaction was not significant, $F(1, 46) = 1.14$, *ns*, thus providing no evidence for the induction of a group difference in the capacity to process associates of the valenced prime meanings, independent of the differential interpretation of the primes themselves. There were also no other significant effects revealed for targets following the unambiguous primes (Fs< 2).

The analysis of completion scores for targets following the ambiguous primes revealed a non-significant main effect of training group, $F(1, 46) = .029$, *ns*. There was a significant main effect of target associate, $F(1, 46) = 4.24$, $p<.05$, with participants more accurate in completing target fragments associated with the threatening, relative to non-threatening, meanings of the homograph primes (64.2% vs. 58.8%, respectively). This difference implied that across both training groups, participants were more likely to impose threat-congruent interpretations, than non-threat-congruent interpretations, upon the ambiguous primes. However, this main effect was modified in the predicted training group by target associate interaction, $F(1, 46) = 5.02$, $p<.05$. The nature of this interaction is depicted in Figure 9.1. As can be seen from the figure, for ambiguous primes in the interpretive assessment phase, the participants in the threat training group displayed an enhanced capacity to provide completions for threat associates relative to non-threat associates, in comparison to the non-threat training group. This thus supports the conclusion that the training induced a group difference in interpretive bias, congruent with the valence of training group assignment.

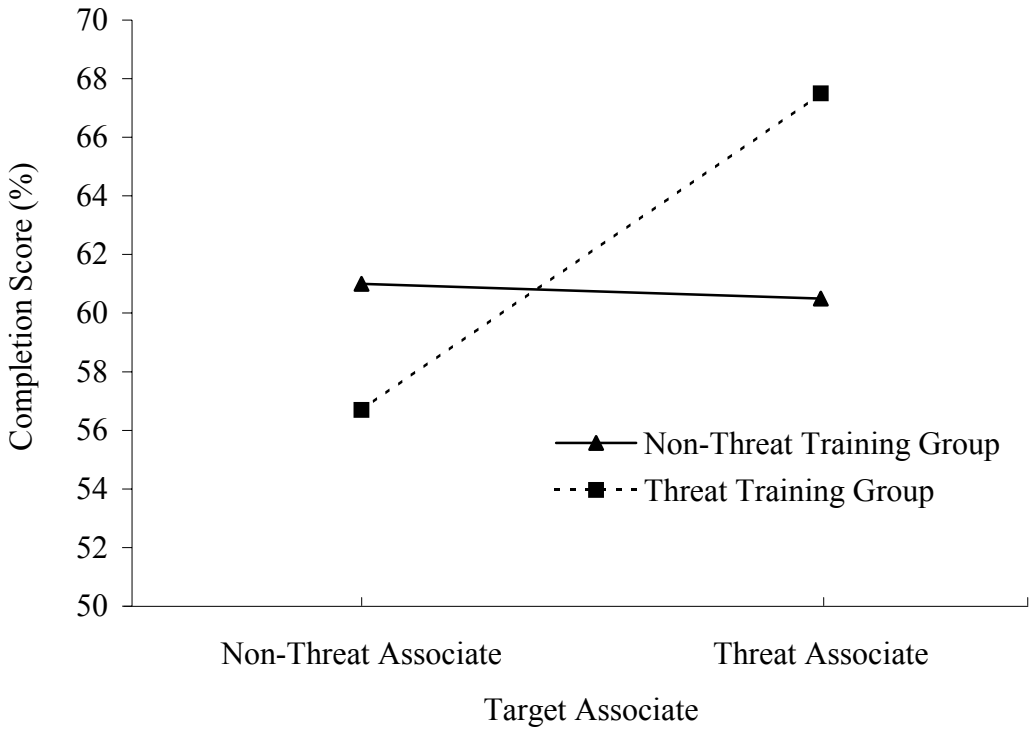

Figure 9.1. Mean completion scores for target associates following ambiguous primes in the interpretive assessment phase of Study 8.

As for study 5, a follow-up analysis was then conducted in order to determine the degree to which this observed group difference for ambiguous primes differed from patterns of responding shown for targets following unambiguous primes. Thus, completion times for both kinds of prime were entered into a single overarching analysis which included prime ambiguity (ambiguous prime vs unambiguous prime) as a factor. In fact, this analysis did not reveal the expected three-way interaction of training group by target associate by prime ambiguity. Rather, this analysis revealed a significant interaction of training group and target associate, $F(1,46) = 4.92$, $p<.05$

which was not significantly modified by the prime ambiguity factor, $F(1, 46) = 1.52$, *ns*. Thus, this analysis did not conform that the pattern of responses observed in the ambiguous prime condition differed significantly from those observed for the unambiguous prime condition.

The only other significant effects revealed by this overarching analysis were not central to the experimental hypotheses: A main effect of prime ambiguity, $F(1, 46) = 12.58$, $p<.001$, and a significant prime ambiguity by target associate interaction, $F(1, 46) = 6.8$, $p<.05$ (all other Fs<1).

Completion Times

Completion times for each condition in the interpretive assessment phase are provided in Table 9.3.

These completion time data were analysed primarily to exclude the possibility that the training group by target associate interaction observed on completion scores might have reflected the presence of a speed-accuracy trade-off for these trials. The analyses performed here thus mirrored the analyses performed for completion score data. Completion times for ambiguous and unambiguous prime trials were analysed in mixed-design 2-way ANOVAs, in which the factors were training group (threat training group vs. non-threat training group) and target associate (threat associate vs. non-threat associate)[10].

[10] As for previous analyses of completion times, these scores were also analyzed following log transformation as a correction for observed skewness in the distribution. These analyses provided findings which were consistent with the outcomes of analyses of untransformed data in all respects.

Table 9.3

Mean Completion Times (ms) Obtained In Study 8

	Ambiguous prime		Unambiguous prime	
	Non-threat associate	Threat associate	Non-threat associate	Threat Associate
Non-threat training group	2081 (481)	2348 (591)	1903 (479)	1949 (468)
Threat training group	2146 (593)	2220 (426)	2011 (448)	1991 (575)

Note. Standard deviations are provided in brackets.

Only one significant effect was revealed by the analyses of completion times**.** Specifically, for the targets which followed ambiguous primes, there was a main effect of target associate, $F(1, 46) = 4.32$, $p<.05$, with participants speeded in the completion of fragments derived from non-threat, relative to threat, associates (2113 ms vs. 2284 ms, respectively). Thus, the scores for this significant main effect implied the reverse pattern of performance to the scores obtained for the same significant main effect in completion score data, suggesting that this performance difference reflected the operation of a speed/accuracy trade-off. The suggestion provided earlier, that on average, participants were characterised by a threat-congruent interpretive bias, was therefore rejected by the detection of this contrary effect.

Most crucially, the target associate by training group interaction was not significant, either for targets following unambiguous primes, $F(1, 46) = 0.20$, *ns* or for targets following ambiguous primes, $F(1, 46) = 1.38$, *ns*. Thus, there was no indication

that the groups were differentially speeded in responding to word-fragments of one valence relative to the other, irrespective of the nature of preceding primes. Therefore, the pattern of completion latencies provided no reason to doubt the conclusion, based on completion scores, that the training contingency induced a group difference in interpretive bias. No other significant effects were revealed by these analyses (all other Fs<1).

Visual Analogue Mood Scale Scores

Mean VAMS scores and associated standard deviations for each training group at each respective mood assessment point are provided in Table 9.4. Two participants from the non-threat training group and one participant from the threat training group did not make the final VAMS rating, once again because the participants believed prematurely that the experimental session was terminated. This occurred despite efforts to ensure that the instruction to make this final rating was properly understood in this study. The ratings provided in the table, and thus incorporated into analyses within this section, are those with these three participants excluded.

Table 9.4

Mean Visual Analogue Mood Scale Scores Obtained In Study 8

	Mood assessment points			
	Pre-training	Post-interpretive assessment	Pre-stress	Post-stress
		Anxiety scale		
Non-threat training group	37.7 (26.1)	41.4 (23.4)	44.4 (22.6)	45.6 (22.1)
Threat training group	33.0 (18.7)	34.8 (21.2)	40.1 (20.6)	44.0 (23.9)
		Depression scale		
Non-threat training group	35.1 (25.6)	33.9 (22.4)	37.9 (19.6)	45.7 (18.6)
Threat training group	38.1 (25.9)	29.7 (15.3)	34.5 (17.7)	42.6 (19.1)

Note. Standard deviations are provided in brackets. N=22 for the non-threat training group and N=23 for the threat training group, for all relevant means.

Immediate influence of the training procedure on mood state.

As always, it was important to first assess the possibility that the training simply influenced mood state directly, by analysing VAMS scores obtained directly prior to and following the interpretive training/assessment trials. VAMS scores obtained on the anxiety scale, immediately prior to and following the interpretive training/assessment trials, were entered into a 2-way, mixed-design ANOVA, in which the between-groups

factor was training group (non-threat training group vs. threat training group) and the within-participants factor was mood assessment point (pre-training vs. post-interpretive assessment). This analysis did not reveal any significant effects (all *F*s<1). Most importantly, the training group by mood assessment point interaction was not significant, $F(1, 43) = 0.11$, *ns*, thus providing no indication that the groups responded to the training, immediately and directly, with differential state anxiety elevation. In an identical analysis upon depression scale ratings, the training group by mood assessment point interaction also was not significant, $F(1, 43) = 1.72$, *ns*. This analysis also did not reveal any other significant effects (all *F*s<1 except for the non-significant main effect of assessment point, $F(1, 46) = 2.23$, *ns*). Thus, like anxiety, depression also was not directly modified by the training group assignment.

Influence of the training procedure on emotional vulnerability.

Analyses were then performed for scores obtained prior to and following the subsequent stress phase involving the video stressor. Anxiety VAMS scores were analysed in a 2-way, mixed-design ANOVA, in which the between-groups factor was training group (non-threat training group vs. threat training group) and the within-participants factor was mood assessment point (pre-stress vs. post-stress).

Because evidence was obtained that the training successfully induced a group difference in interpretive bias, the hypothesis that interpretive bias can causally mediate anxiety vulnerability provided the prediction of a significant training group by mood assessment point interaction for these data. In fact, this interaction was not significant, $F(1, 43) = 0.09$, *ns*, and there were no other significant effects revealed by this analysis (all *F*s<1). Thus, the central hypothesis under examination, that interpretive bias can causally mediate anxiety vulnerability, was not supported. An identical analysis upon

depression scale ratings revealed only a significant main effect of assessment point, F(1, 43) = 8.39, p<.05, with participants displaying a significant elevation in depression ratings from prior to following their exposure to the video stressor (36.2 vs 44.1, respectively). However, once again, the training group by mood assessment point interaction failed to reach significance, $F(1, 43) = 0$, *ns*. The main effect of training group was also non-significant, $F(1, 43) = .43$, *ns.* Thus, there was no evidence of any group difference in vulnerability to elevation in anxiety or depression, thus providing no support for the causal role of interpretive bias in emotional vulnerability.

Discussion

In the interpretive assessment phase of this study, the group of participants who received training encouraging a threat-congruent interpretive bias, relative to those who received the opposite training, displayed enhanced completion of word-fragments derived from threat associates, relative to non-threat associates of the homographs. This group difference was not shown in the baseline, unambiguous prime condition which permitted no differential interpretation to be imposed upon the primes. However, in contrast to Study 5, a follow-up analysis of these scores did not sustain the expectation that the training effect shown for targets following homograph primes could be statistically discriminated from that shown for targets following unambiguous primes. Thus, while these findings are certainly consistent with the possibility that the training induced a group difference in interpretive bias, the failure to statistically discriminate patterns of performance on ambiguous and unambiguous prime trials prevents us from ruling out alternative accounts of this training effect.

Not withstanding the failure to obtain the expected three-way interaction in this case, it is interesting to consider the possibility that this fragment-completion interpretive training task might provide a more effective means of interpretive training than the relatedness-judgment task used, unsuccessfully, in Studies 6 and 7. If so, then one possible explanation for this differential effectiveness is provided by Grey and Mathews (2000). These researchers suggest that training tasks which require participants to actively generate emotional meanings may serve as more effective means of manipulating interpretive bias. Thus, the crucial difference between these training procedures could be the requirement, in the fragment-completion training task, for participants to actively generate alternative solutions to the fragments, using the primes as cues. This may be contrasted with the relatedness-judgment task, where responses are a matter of selecting between alternative solutions already provided on screen, rather than generating these solutions.

However, prior to assessing the veracity of such an explanation, it would be important to confirm that the training tasks do actually significantly differ in their capacity to induce a group difference in interpretive bias. The observation that, in separate instances, the different training procedures induced group differences in interpretive bias which did or did not reach statistical significance, when contrasted to unambiguous prime baselines, does not confirm that respective magnitudes of training effects obtained in these different studies were significantly different. In fact, this issue, as to the relative efficacy of the different training tasks, could only be conclusively addressed by directly comparing the magnitude of these training effects within one overarching analysis.

In the present study, a training procedure was employed has previously been evidenced to induce a group difference in interpretive bias, and indeed, the patterns of observed responses in interpretive assessment were not inconsistent with this possibility. However, the training groups were not observed to respond to the video stressor with differential elevation in state anxiety or depression, thus providing no support for the hypothesis that interpretive bias can causally mediate emotional vulnerability. The conclusion warranted by these findings differs from the respective conclusions invited by the results of the other three studies comprising Phase 2. Each of the other studies yielded evidence which was at least minimally consistent with the hypothesis that interpretive bias can causally mediate anxiety vulnerability. Most strikingly, these findings diverge from those obtained in Study 5. In Study 5, analyses of participant responses suggested that an induced group difference in interpretive bias causally mediated a congruent group difference in anxiety vulnerability, thus providing strong support for the causal hypothesis.

Of course, one possible explanation for this disparity would be that interpretive bias simply plays no causal role in anxiety vulnerability, with the group difference in interpretive bias detected in Study 5 in fact being linked coincidentally, but not causally, to the subsequent detection of group difference in anxious reaction to the stressor. There are also alternatives to this conclusion which could preserve the status of the causal hypothesis.

One such alternative possibility is that the difference in interpretive assessment task employed in Studies 5 and 8 may in some way account for their different findings. In contrast to Study 5, which used the relatedness-judgment task in interpretive assessment, this present task used the fragment-completion interpretive assessment task.

It seems plausible to suppose that the interpretive assessment trials may, as well as assessing interpretive bias, to some extent extinguish the effects of the interpretive training. This is because the training contingency is eliminated in these assessment trials, and the advantage afforded participants in each training group, through applying particular interpretive biases upon primes in responding to targets, is thus removed. The possibility has already been considered that the fragment-completion task might represent a more effective means of interpretive training than the relatedness-judgment task. If this is the case, then the fragment-completion task might also more effectively extinguish the effects of interpretive training, when employed in interpretive assessment. Thus, it could be that a group difference in anxiety vulnerability was induced in this present study to the same degree as in Study 5, but was more effectively extinguished by the interpretive assessment trials which only in this present case involved the fragment-completion task. This could also account for the failure to distinguish patterns of performance in ambiguous and unambiguous prime trials in the present case. To investigate this possibility, a second interpretive assessment might be provided following the stress phase, to assess the presence or absence of the group difference in interpretive bias detected on the earlier interpretive assessment.

Another possibility is that the fragment-completion interpretive training task, though inducing a group difference in interpretive bias in each case, had a smaller magnitude of influence in this present study than in Study 5, and in fact was not significantly more effective than the relatedness-judgment training procedure employed in Studies 6 and 7. If this were the case, then it would not necessarily be contrary to the causal hypothesis if the training in this present study also did not influence emotional vulnerability to a detectable degree.

As with those possibilities raised with respect to the effectiveness of the training procedures, the explanations given above are premised upon a particular assumption which has not been directly assessed. Specifically, that the statistical significance of a predicted group difference in some studies, and its non-significance in others, implies that the magnitudes of effects in these different cases differ significantly. In fact, to draw strong conclusions on the basis of the profile of findings obtained across all four studies, it would be necessary to compare the magnitudes of interpretive training effects and of group differences in vulnerability obtained in the different studies, each within an overarching analysis. This was the goal of the next chapter of this thesis.

CHAPTER 10

COMPARATIVE ANALYSES OF FINDINGS FROM PHASE 2 STUDIES

The four studies executed in this second phase were designed to assess the hypothesis that interpretive bias can causally mediate anxiety vulnerability. As noted throughout, assessment of this central hypothesis was contingent upon the success of the interpretive training received prior to the assessment of vulnerability. Thus, for the hypothesis to be sustained, it was necessary that the detection of an induced difference in interpretive bias between training groups should precede the detection of a congruent group difference in anxiety vulnerability. As indicated in Chapter 5, in order to ensure the validity of conclusions, it was considered important to test the causal hypothesis in a series of studies, into which variations in training and assessment procedure could be introduced. It was also not undesirable that the effectiveness of the different training procedures in manipulating interpretive bias might vary. It was reasoned that if the influence of such training upon vulnerability is indeed mediated by its impact upon interpretive bias, then a less effective training procedure should also less effectively influence anxiety vulnerability.

In discussing the findings of each individual study, it was noted that group differences in interpretive bias and/or anxiety vulnerability could be statistically supported in some studies but not in others. A summary of the effect sizes obtained in the two central interaction analyses of all four Phase 2 studies, is provided in Table 10.1.

Table 10.1.

Effect Sizes (η^2) of Training Effects Obtained in Phase 2 (n=48)

Study	Interpretive training task	Interpretive assessment task	Interaction	
			Training group by target associate	Training group by assessment point
5	Fragment-completion	Relatedness-judgment	.123	.090*
6	Relatedness-judgment	Relatedness-judgment	.028	.024
7	Relatedness-judgment	Fragment-completion	.024	.029
8	Fragment-completion	Fragment-completion	.098	.002**

Note. Training group by target interaction refers to the analysis of ambiguous primes in each case. The training group by assessment point interaction refers to the analysis of anxiety ratings., from pre-stress to post-stress.

* $N = 44$

** N=45

However, before drawing any firm conclusions concerning the causal hypothesis on the basis of these findings, it was considered important to determine which apparent discrepancies between studies were statistically significant and which were not. This would make it possible to directly determine whether, as predicted by the causal hypothesis, differentially effective interpretive training serves to differentially modify emotional vulnerability. This issue was investigated by analyzing the findings from these studies within one overarching analysis.

Overview Of Analyses

There were three series of analyses performed and each is described in a separate section below. Firstly, the initial emotional measures provided to participants in each study were statistically contrasted in order to ensure that, across the studies, these participants did not differ in terms of initial emotional characteristics. Thus, these analyses compared the questionnaire scores across the two training groups for the two classes of interpretive training task (fragment-completion training vs. relatedness-judgment training) and the two types of interpretive assessment task (fragment-completion assessment vs. relatedness-judgment assessment).

Secondly, an analysis was performed to compare the relative magnitude of the interpretive training effects obtained across the four experiments. In this analysis, a standardized measure of performance was computed for each interpretive assessment task (the performance index), so that this data could be subjected to a single overarching analysis. This analysis again incorporated the factors interpretive training task (fragment-completion training vs. relatedness-judgment training) and interpretive assessment task (fragment-completion assessment vs. relatedness-judgment assessment) in addition to the training group and target associate factors.

Thirdly, a pair of analyses compared the anxious and depressive reaction to the stressor shown by threat and non-threat training groups across all four studies, by analyzing the VAMS scores obtained in all studies prior to and following the stressor. Thus, these analyses now incorporated interpretive training task (fragment-completion training vs. relatedness-judgment training) and interpretive assessment task (fragment-completion assessment vs. relatedness-judgment assessment) in addition to the training group and mood assessment point factors.

Participant Characteristics Across Studies

The characteristics of the one hundred and ninety-two first year undergraduates who participated in the four studies are provided in each of the chapters describing each of the four individual studies (Chapters 6-9). Each of the participant characteristics (STAI-T, STAI-S, BDI-II and age) was subjected to a 3-way, between-groups ANOVA, which incorporated the factors interpretive training task (fragment-completion training vs. relatedness-judgment training) and interpretive assessment task (fragment-completion assessment vs. relatedness-judgment assessment), as well as training group (threat training group vs. non-threat training group).

In the analysis of participant age, only a single significant effect was revealed which was a main effect of training group, $F(1, 184) = 9.02$, $p<.05$. Participants assigned to the non-threat training group were significantly younger than those assigned to the threat training group (17.9 years vs. 19.1 years, respectively). No other significant effects were revealed by this analysis.

The ANOVAs carried out on STAI-T, STAI-S, and BDI-II scores did not reveal any significant main effects or interactions. Most crucially, across the studies, the main effect of training group was not significant for STAI-S, $F(1, 184) = 0.82$, *ns*, or for STAI-T, $F(1, 183) = 1.92$, *ns*, or for BDI-II, $F(1, 184) = 0.21$, *ns*. Thus, there was no evidence that those assigned to the threat training group differed significantly in emotional characteristics from those assigned to the non-threat training group. As these non-significant main effects were also not significantly modified by interpretive assessment task or interpretive training task factors (all $Fs<3$), there was no indication that these emotional characteristics differed significantly for participants receiving the four different combinations of these tasks.

Performance Index Scores In Interpretive Assessment Phases

It will be recalled that two studies employed the relatedness-judgment task in the interpretive assessment phase, whereas two studies employed the fragment-completion task. The relatedness-judgment task was designed to reveal interpretive bias using selection time as the dependent measure, whereas the fragment-completion task was designed to reveal interpretive bias using completion score as the dependent measure. Therefore, in order to subject these measures to a single ANOVA, it was necessary to convert them to a common metric. Shorter, as opposed to longer, selection times are indicative of enhanced performance, whereas for completion scores, larger, as opposed to smaller scores reflect enhanced performance. Therefore, selection times from the relatedness-judgment interpretive assessment task were reverse-scored, so that larger scores on this measure now represented enhanced performance, as was already the case for completion scores. Following this, scores on these dependent measures, obtained under each target associate condition, were converted to standardised z-scores[11]. Thus, a common *performance index* was created, with a higher score on this performance index reflecting an enhanced capacity to process the targets. The calculated performance index scores for targets of each type, following ambiguous primes, are provided, along with associated standard deviations, in Table 10.2.

[11] As for the individual analyses of data from each study, these overall indices were also computed for unmodified, and log transformed data, and the findings obtained in analyses of each type of data were equivalent in all respects.

Table 10.2

Mean Performance Index Scores (z-scores) Obtained In Interpretive Assessment Phases Of Studies 5-8

Interpretive training Task	Interpretive assessment task	Non-threat associate	Threat associate
	Non-threat training group		
Fragment-completion	Fragment-completion	0.090 (0.94)	0.061 (0.86)
	Relatedness-judgment	0.216 (0.78)	-0.194 (1.09)
Relatedness-judgment	Fragment-completion	-0.005 (1.06)	-0.168 (0.88)
	Relatedness-judgment	0.317 (0.89)	0.167 (1.14)
	Threat training group		
Fragment-completion	Fragment-completion	-0.178 (1.03)	0.504 (1.07)
	Relatedness-judgment	-0.421 (1.11)	-0.197 (1.06)
Relatedness-judgment	Fragment-completion	-0.245 (1.07)	-0.058 (1.00)
	Relatedness-judgment	0.014 (0.88)	0.097 (0.89)

Note. Standard deviations are provided in brackets.

These performance index scores were entered into a 4-way, mixed-design ANOVA, in which the between-groups factors were training group (non-threat training group vs. threat training group), interpretive training task (fragment-completion training vs. relatedness-judgment training) and interpretive assessment task (fragment-completion assessment vs. relatedness-judgment assessment)[12]. The within-participants factor was target associate (threat associate vs. non-threat associate). It was expected that if, across these studies, the assignment of participants to training groups introduced a congruent group difference in interpretive bias, then this would be reflected in a training group by target associate interaction. If the magnitude of this training effect differed distinguishably across different combinations of interpretive training task and/or interpretive assessment task, then this 2-way interaction was expected to be modified by the interpretive training task and/or interpretive assessment task factors.

This analysis revealed two significant effects. Firstly, there was a significant interaction of interpretive training task by interpretive assessment task, $F(1, 184) = 4.61$, $p<.05$. Participants who completed the fragment-completion task in the interpretive training phase performed better at the fragment-completion task than at the relatedness-judgment task in the interpretive assessment phase, (0.119 vs. -0.149, respectively), compared to those who completed the relatedness-judgment task, who displayed the opposite pattern of enhancement (-0.119 vs. 0.149, for fragment-completion and relatedness-judgment interpretive assessment tasks, respectively). Thus, practice with the requirements of a particular task during interpretive training, appears

[12] In keeping with the previous chapters which have considered outcomes from log transformed data, these analyses were performed also for indices derived from log transformed scores, and outcomes of analyses were equivalent in each case.

to have led to enhanced performance on that same task, as opposed to the opposite task, during the interpretive assessment phase.

It is important to note that this significant interpretive training task by interpretive assessment task interaction was not significantly modified by training group, $F(1, 184) = 0.33$, *ns*, or by target associate, $F(1, 184) = 1.79$, *ns*, or by these factors in combination, $F(1, 184) = 0.01$, *ns*. Thus, the influence of practice on performance of each task during assessment did not significantly differ for each type of target associate and valence of training received. It is likely then, that this performance facilitation, for those who completed the same, as opposed to the opposite task in each phase, reflected practice with aspects of the training which were irrelevant to their emotional effects. For example, as suggested previously, those completing fragment-completion training might have come to memorise some of the more probable solutions for a particular sequence of letters, thereby becoming better at completing letter combinations per se, with this effect unrelated to any emotionally-relevant effects of the training.

The second significant effect from this analysis was of greatest importance for the question as to the effectiveness of these training procedures. This was a significant training group by target associate interaction, $F(1, 184) = 11.93$, $p<.01$. The nature of this interaction is depicted in Figure 10.1.

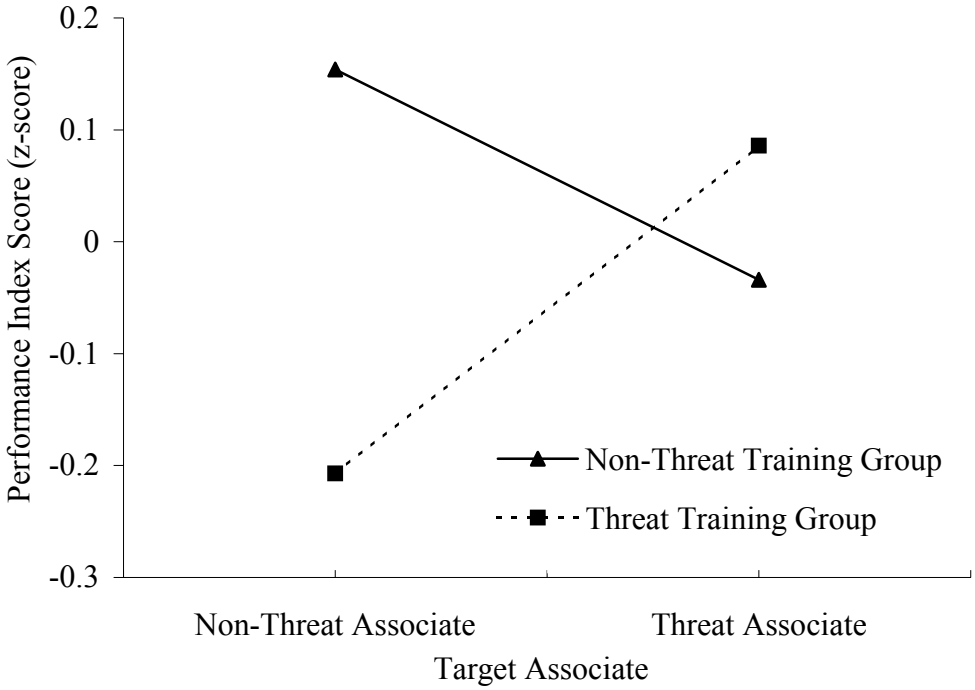

Figure 10.1. Mean performance index scores for target associates following ambiguous primes in the interpretive assessment phases of Studies 5-8.

As can be seen from the figure, the non-threat training group, relative to the threat training group, displayed facilitated processing of targets associated with the non-threatening, relative to threatening, meanings of preceding homographs. This pattern of findings clearly indicates that across studies, the variants of the single-word priming methodology employed to train interpretive bias were, on average, successful in inducing a training-congruent group difference in interpretive bias.

The training group by target associate interaction was not significantly modified on the basis of interpretive training task, $F(1, 184) = 1.87$, *ns*. Thus, these analyses did not support the suggestion made in Chapters 8 and 9, that the fragment-completion training was more effective than the relatedness-judgement training in inducing a group

difference in interpretive bias. In addition, the training group by target associate interaction also was not significantly modified by interpretive assessment task, $F(1, 184) = 1.79$, *ns.* Thus, as previously indicated, the training group by target associate interaction was not significantly modified by interpretive training task and interpretive assessment task factors in combination, $F(1, 184) = .001$, *ns.* Thus, this analysis also provided no support for the suggestion made in Chapter 7, that the influence of training upon interpretive bias might be more sensitively detected using a different task in interpretive assessment from that used in interpretive training. The non-significance of this interaction also provided no support for the suggestion made in Chapter 9 that, though statistically significant, the influence of this training upon interpretive bias in Study 8 might have been significantly smaller in magnitude than the training effect observed in Study 5.

In sum, the outcome of this analysis supported the conclusion that the training procedures influenced interpretive bias in the anticipated direction, with the non-threat training group more likely than the threat training group to interpret emotional ambiguity in a non-threat-congruent fashion. Conversely, these findings were inconsistent with speculations raised previously, based on the premise that the detected training effects may have significantly differed in magnitude in any possible combination of the four different studies.

Mood State Reactivity To The Video Stressor

The anxiety and depression VAMS (visual analogue mood scale) ratings, provided by each training group in each study directly prior to and following the video stressor, are provided in relevant sections of Chapters 6 to 9. The impact of the training

group assignment on emotional vulnerability across the four studies was examined by subjecting pre-stress and post-stress VAMS scores to 4-way, mixed-design ANOVAs. Each analysis included the three between-participant factors, training group (threat training group vs. non-threat training group), interpretive training task (fragment-completion training vs. relatedness-judgment training) and interpretive assessment task (fragment-completion assessment vs. relatedness-judgment assessment), as well as the within-participants factor mood assessment point (pre-stress vs. post-stress).

The analysis of performance index scores reported in the previous section indicated that the training group assignment led to the intended group difference in interpretive bias. Furthermore, this training effect did not significantly differ in magnitude across the studies employing different combinations of interpretive training task and interpretive assessment task. Therefore, based on the hypothesis that interpretive bias can causally mediate anxiety vulnerability, it was predicted that across these same studies, the training groups would also display differential anxious reaction to the stressor, irrespective of the particular type of interpretive training or interpretive assessment task employed. Thus, a significant training group by mood assessment point interaction, unmodified by any other factors, was predicted to be revealed by the analysis of anxiety VAMS ratings.

This analysis revealed a main effect of mood assessment point, $F(1, 177) = 9.80$, $p<.05$, reflecting an overall elevation in state anxiety following the video stressor, of approximately 6.3 points. Thus, it could be concluded that the video stressor effectively elicited an anxious response from participants in these studies. Most importantly, this main effect was qualified by the predicted interaction of training group by mood assessment point, $F(1, 177) = 5.28$, $p<.05$. The nature of this interaction was consistent

with the prediction that the anxious reaction to the video stressor would be relatively ameliorated in the group trained to interpret ambiguity in a non-threat-congruent fashion. The threat training group showed a significant increase of 11 points in anxiety VAMS scores in response to the video stressor, $F(1, 90) = 14.97$, $p<.001$. In contrast, the non-threat training group showed a non-significant increase of 1.7 points, $F(1, 87) = 0.34$, *ns*, thus representing approximately one sixth of the increase shown the by the threat training group.

This training group by mood assessment point interaction was not significantly modified by interpretive assessment task, $F(1, 177) = 1.09$, *ns*. Thus, there also was no support for the suggestion, made in Chapter 9, that the training influenced anxiety vulnerability to a greater degree in Study 5 than in Study 8. Further, this central interaction also was not significantly modified by interpretive training task, $F(1, 177) = 0.07$, *ns*, or by interpretive training task and interpretive assessment task in combination, $F(1, 177) = 0.77$, *ns*. Thus, there also was no support for the supposition that the training more effectively induced a group difference in anxiety vulnerability in Study 5 than in the other three studies.

In sum, these findings provided strong support for the causal hypothesis, with the training contingencies influencing interpretive bias, and anxiety vulnerability, each to extents which could not be differentiated on the basis of the interpretive training or interpretive assessment task employed.

The parallel analysis of depression ratings from prior to and following the video stressor revealed a main effect of mood assessment point, $F(1, 177) = 18.03$, $p<.001$, due to the elevation of depression ratings, following the video stressor, of approximately 6.4 points. Thus, the stressor appeared to significantly elevate state

depression, as with state anxiety. Nonetheless, in contrast to the analysis of anxiety ratings, the training group by mood assessment point interaction was not significant on these scores, $F(1, 177) = 3.44$, *ns,* suggesting that vulnerability to elevation in depression, unlike anxiety, was not significantly influenced by this interpretive bias manipulation.

Despite the failure to confirm the influence of the training contingency upon the depression VAMS scores, it was noted that the trends on the depression mood scale, for the different training groups, were consistent with those shown on the anxiety mood scale. Specifically, from pre-stress to post-stress mood assessment points, the non-threat training group displayed an increase of 3.6 points on the depression scale, which was less than half the increase of 9.2 points shown by the threat training group. It was therefore considered prudent, in ensuring that the training indeed influenced anxious reaction to the stressor to a greater degree than depressive reaction, to compare these scores in one inclusive analysis.

Scores on each mood scale were thus converted to z-scores, to allow their comparison. These standardised VAMS scores were entered into a 5-way mixed-design ANOVA, which, as before, included the factors interpretive training task, interpretive assessment task, training group, and mood assessment point, as well as a new factor, scale type (anxiety scale vs. depression scale). This analysis revealed a training group by mood assessment point interaction, $F(1, 177) = 5.70$, $p<.05$, which was not modified by interpretive training task, $F(1, 177) = 0.27$, *ns* or by interpretive assessment task, $F(1, 177) = 0.92$, *ns,* or by both factors in combination, $F(1, 177) = 2.24$, *ns*. Thus, once again, there was no indication that this effect varied significantly across the individual studies.

The nature of this 2-way interaction was, as would be expected, consistent with the trends obtained on the individual scales. The non-threat training group displayed a non-significant mean increase of 0.184 on this standardized scale, $F(1, 87) = 1.60$, *ns*, whereas the threat training group displayed a significant increase of 0.600 points, $F(1, 90) = 20.23$, $p<.001$.

Most crucial to the present issue, this training group by mood assessment point interaction was not modified by scale type, $F(1, 177) = 0.78$, *ns*. Nor was this interaction significantly modified by scale type in combination with any other factors. Thus, the implication of the separate analyses, that the group difference in interpretive bias causally mediated anxious reaction to a greater degree than depressive reaction, was not sustained by this analysis involving both mood scales.

Discussion

An analysis of performance index scores, involving all data obtained in Phase 2 studies, suggested that during the interpretive assessment phases of these studies, participants were biased in their interpretations of the homographs in a manner consistent with training group assignment. Specifically, relative to the threat training group, the non-threat training group displayed a facilitated capacity to process non-threat related, relative to threat related associates of the homograph primes. This suggests that the two groups differentially interpreted the homographs in the fashion encouraged by the training contingencies. Thus, this analysis revealed that on average, across the studies, the introduction of training contingencies successfully induced group differences in interpretive bias.

This analysis also revealed no evidence to suggest that one form of training was more effective than the other in inducing this observed group difference in interpretive bias, or that one form of interpretive assessment was more sensitive than the other in terms of its capacity to detect interpretive bias. Thus, there was no support for the suggestion that the fragment-completion training procedure more effectively modified interpretive bias than the relatedness-judgment training procedure. Similarly, these findings gave no indication that the influence of each interpretive training task was more readily detected on an alternative task, rather than on the same task, during interpretive assessment, as was suggested might be the case.

The analysis of the anxiety ratings provided by these participants before and after the video stressor yielded findings consistent with the prediction, provided by the causal hypothesis, that the induction of a group difference in interpretive bias should causally precipitate a group difference in anxiety vulnerability. Across all studies, the non-threat training group, who relative to the threat training group were induced to interpret ambiguous primes in a non-threat-congruent fashion, also displayed a relatively ameliorated tendency to react to the video stressor with anxiety.

Importantly, like the induced group difference in interpretive bias, this consequent group difference in anxious reaction to the stressor did not significantly vary across the studies using different combinations of interpretive training task and interpretive assessment task (i.e., fragment-completion or relatedness-judgment, in each case). Thus, in full support of the causal hypothesis, the training groups' patterns of anxiety reaction varied only on the basis of the training contingency, and not on the basis of particular variations in participant sample, or methodology. These findings are

therefore fully consistent with the hypothesis that interpretive bias can causally mediate anxiety vulnerability.

Thus, these analyses also failed to support specific suggestions made in discussions of individual studies, regarding variations in the influence of the different training procedures upon emotional vulnerability. There was no support for the assumption that the group difference in anxiety vulnerability induced in Study 5 was greater than that obtained in Studies 6 and 7. Nor was there any support for the suggestion that the group difference in anxiety vulnerability achieved by the training in Study 8 was smaller than that achieved in Study 5.

An overarching analysis of depression ratings obtained in each study, from prior to and following the stressor, was conducted separately from the analysis of anxiety ratings. This analysis did not support the conclusion that degree of depressive reaction to the video stressor was significantly modified by the training contingency. However, when standardized anxiety and depression ratings were entered into a single overarching analysis, this analysis could not confirm that the group difference in mood state elevation shown on the anxiety scale was larger than the group difference in elevation shown on the depression scale. Thus, while these findings strongly support the causal role of interpretive bias in anxiety vulnerability, they do not reject the possibility that it might also mediate vulnerability to elevation in other negative emotions, such as depression. The implications of these findings are further considered in the General Discussion, to follow.

CHAPTER 11

GENERAL DISCUSSION

Review of Findings

The empirical work described in this thesis was carried out in order to assess the hypothesis that interpretive bias can causally mediate anxiety vulnerability, and was executed in two phases. In Phase 1 (Studies 1-4), training procedures were developed which were designed to induce group differences in interpretive bias. In Phase 2, these training procedures were then used to assess the consequences of interpretive bias manipulation for emotional vulnerability, as assessed in emotional reaction to a subsequent stressor.

The first two studies in Phase 1 provided relevant information regarding the selection of stimuli for deployment in interpretive training and assessment tasks. An array of homographs with threatening and non-threatening meanings was initially selected, based on obtained ratings (Study 1). A fragment-completion priming task was then used to assess the capacity for these homographs, as primes, to facilitate access to representations of target stimuli related to their differentially valenced meanings (Study 2). This was investigated through the presentation of target fragments derived from related associates under primed and unprimed conditions. The completion of target fragments related to meanings of each valence was enhanced by the prior presentation of corresponding homograph primes. Thus, it could be inferred that access to the representations of associates related to the valenced meanings of the homographs was facilitated by presentations of the homographs themselves.

The objective of the next two studies (Studies 3 and 4) was then to confirm that tasks created using the stimuli developed in Studies 1 and 2 could systematically manipulate interpretive bias. In Study 3, the fragment-completion task employed in Study 2 was modified by introducing a training contingency that differed between two groups. For the threat training group, target fragments were always related to the threatening meanings of preceding homograph primes. Thus, it was advantageous for these participants to access the threatening meanings of the homographs in responding to the corresponding target fragments. The aim, across trials, was to induce in these participants a threat-congruent interpretive bias. For the non-threat training group, the target fragments were always related to the non-threatening meanings of the preceding homograph primes. The intention was to thus induce in this group of participants a non-threat-congruent interpretive bias. In fact, this study failed to provide any indication that this training procedure modified interpretive bias, as assessed in participants' subsequent performance on a non-contingent version of the same fragment-completion task.

In Study 4, following a number of significant modifications to the task, the efficacy of this fragment-completion training task in manipulating interpretive bias was again tested. In this case, a relatedness-judgment task was used to assess the effectiveness of the training. This assessment procedure also included a baseline unambiguous prime condition, to examine explanations for any observed influence of the training upon assessment task performance which did not implicate the modification of interpretive bias. Performance in this baseline condition provided no indication that the training induced a group difference in the capacity to select associates of differential valence in the absence of the requirement to differentially interpret the primes

themselves. However, as intended, the groups exposed to the two training conditions did now display quite different patterns of response time in identifying associates of differentially valenced meanings of homograph primes. The non-threat training group showed shorter response latencies, relative to the threat training group, in selecting non-threat related, relative to threat related associates, following these homograph primes. Thus, following this final study in Phase 1, it was possible to conclude that the constructed training procedure was capable of inducing group differences in interpretive bias. Further, there was no support provided for the alternative explanation, that this training procedure simply served to influence processing of associates of differential valence, without biasing interpretation of the ambiguous prime stimuli.

Having developed a training approach capable of modifying interpretive bias, it was possible to use this same approach within Phase 2 (Studies 5-8), to assess the central hypothesis, that interpretive bias can causally mediate anxiety vulnerability. In these studies, interpretive training and interpretive assessment procedures were now followed by a video stressor. Emotional reactivity was assessed using mood scales which preceded and followed the stressor. In addition, mood scales preceded and followed the interpretive training/assessment trials, to permit assessment of the immediate influence of the training upon mood state. On the assumption that such immediate emotional influence was not detected, it would then be possible to investigate the impact of interpretive bias manipulations upon subsequent emotional vulnerability, as assessed by emotional reaction to the final video stressor.

The four Phase 2 studies employed alternative combinations of the fragment-completion task and/or relatedness-judgment task used in Phase 1, in the training and assessment of interpretive bias. The individual studies in this phase revealed evidence of

variation in the effectiveness of the different task combinations in modifying interpretive bias and anxiety vulnerability. Consistent with the causal hypothesis, when an induced group difference in interpretive bias could not be detected (Studies 6 and 7), there was also no statistically significant evidence for the impact of the training upon anxiety vulnerability, whereas significant evidence that the training modified anxiety vulnerability was only obtained when the training also significantly modified interpretive bias (Study 5). However, in Study 8, the training task exerted a statistically significant influence upon interpretive bias while its impact upon anxiety vulnerability remained undetected.

To clarify whether or not these variations in significant effects across the studies were reliable, and so to better illuminate the actual association between induced interpretive bias and induced change in anxiety vulnerability, it was considered important to confirm the statistical significance of these supposed variations. Therefore, the data from all four studies in Phase 2 were analyzed together (see Chapter 10), to determine whether any of the effects obtained in the different studies were significantly different in magnitude. These analyses suggested that across studies, the opposing training contingencies induced congruent group differences in the capacity to respond to non-threat related, relative to threat related, associates of the ambiguous primes. This overall pattern of findings was thus fully consistent with the suggestion that these contingencies induced group differences in interpretive bias, congruent with the received valence of training. Furthermore, this analysis yielded no evidence that the different training procedures varied in terms of their effectiveness, or that different interpretive assessment tasks were differentially capable of detecting these effects.

For the causal hypothesis to be supported, it was necessary that variation in anxious reaction to the stressor should be consistent with the detected pattern of induced interpretive biases. This pattern of induced biases did not differ across the combinations of interpretive training or interpretive assessment task employed. Therefore, the causal hypothesis provided the expectation that the threat and non-threat trained participants should also display differential elevation in state anxiety in response to the stressor, irrespective of the particular interpretive training or assessment task received. Analyses of anxiety ratings obtained prior to and following the stressor across the four Phase 2 studies revealed this to be the case. The non-threat training group displayed a smaller elevation in state anxiety from before to after the video stressor, in comparison to the threat training group. This pattern of mood ratings did not vary significantly across the combinations of task employed to train or assess interpretive bias, and thus the pattern of induced difference in anxiety reactivity paralleled the pattern of induced group difference in interpretive bias. These findings thus provide strong support for the hypothesis that interpretive bias can causally mediate anxiety vulnerability.

This research focused on the role of interpretive bias in mediating vulnerability to anxiety, but measures of depression were also taken to assess the emotional specificity of this hypothesized causal mediator. When ratings of anxious and depressive reaction to the video stressor were compared within an overarching analysis, the impact of interpretive bias upon reactivity shown on anxiety mood scales could not be significantly discriminated from that shown on depression mood scales. Thus, these findings do not reject the possibility that a negative interpretive bias may causally underpin vulnerability to elevation in depression, as well as anxiety.

Implications

Relationship Of Findings To Prior Reported Attempts To Modify Interpretive Bias

The work described in this thesis has addressed some major inadequacies in the small number of studies that have attempted to directly manipulate interpretive bias, and to investigate the emotional effects of such manipulations. In Grey and Mathew's (2000) work, it was not clear whether their training procedures influenced interpretive bias, or simply directly modified the capacity to process target stimuli associated with differentially valenced prime meanings. The experiments developed in Phase 1 of this present research program addressed this difficulty by introducing a control condition in which these targets were not preceded by ambiguous primes. It was demonstrated that the induced group difference in processing of targets related to valenced prime meanings, was confined to those conditions under which the primes were ambiguous. Thus, these findings bolster the claim made by Grey and Mathews (2000), that such interpretive training procedures can manipulate interpretive bias.

In another series of studies employing a rather different interpretive training regime (see Chapter 1), Mathews and Mackintosh (2000) demonstrated that different groups of participants encouraged to interpret ambiguous textual vignettes in threat-congruent and non-threat-congruent fashions, displayed differential elevation in state anxiety, from before to after the interpretive training/assessment trials. However, it was not clear whether this group difference reflected the influence of interpretive bias manipulation upon anxiety vulnerability, or the direct influence of exposure to valenced training stimuli upon mood state.

In the present series of studies, these possibilities were disentangled by separately assessing change in mood state shown in direct response to the training trials,

and in subsequent emotional reaction to a later stressor. The present work has thus for the first time provided support for the conclusion that interpretive bias can causally mediate anxiety vulnerability, uncompromised by the difficulty inherent in Mathews and Mackintosh's (2000) work. The training induced a differential tendency to respond to the stressor with elevated state anxiety, even though the training procedure itself did not differentially influence mood state.

The interpretive training and assessment methods used in the present research program differed from those used by Mathews and Mackintosh (2000). Nonetheless, it is interesting to consider a particular discrepancy between the findings of the two series of studies. As noted, Mathews and Mackintosh reported observing differential state anxiety elevation amongst their training groups, from before to after the interpretive training/assessment trials. In the present series of studies, no such differential mood change was evident from ratings obtained prior to and immediately following the training/assessment trials. If it is supposed that the emotional change observed by Mathews and Mackintosh resulted from the differential interpretations imposed by training groups upon the stimuli used in interpretive training and assessment, then the different pattern of findings in each series of studies might be due to the differential emotional intensity of the stimuli employed in each case. In Mathews and Mackintosh's studies, the stimuli were extended vignettes describing social situations, whereas in the present studies the stimuli were only single ambiguous words. It is possible then, that the stimuli provided by Mathews and Mackintosh (2000) sustained a more intense capacity to evoke emotional reaction, depending upon their interpretation, such that differential interpretations of these stimuli, during the training procedure itself, had detectable emotional effects.

Theoretical Implications

Implications for models of anxiety vulnerability.

The findings of this research program support that class of cognitive models of anxiety vulnerability, reviewed in some detail in the introduction, which hold in common the hypothesis that an interpretive bias favouring the threat-congruent interpretations of ambiguity causally mediates anxiety vulnerability (Daleiden & Vasey, 1997; Eysenck, 1992a, 1997; Mathews & Mackintosh, 1998; Power & Dalgleish, 1997; Williams et al., 1988, 1997). Until now, this causal hypothesis has represented speculation only, originating from the consistent observation, in empirical findings, that anxiety vulnerable individuals are characterised by an elevated tendency to interpret ambiguous stimuli in a threat-congruent fashion. As highlighted in Chapter 1, demonstrating an association between interpretive bias and elevated anxiety vulnerability does not implicate a particular causal relationship in the explanation of this association. However, this present series of studies has yielded evidence that directly supports the particular causal pathway emphasised by these theories; specifically, that interpretive bias can causally mediate anxiety vulnerability.

Further refinement of our understanding of such models of anxiety vulnerability may come from studies designed to assess the relevance of various possible mechanisms postulated to underlie the observed interpretive bias. For example, to reiterate a model described in Chapter 1, Mathews and Mackintosh (1998) suggest that threat-congruent interpretive bias may originate from the operation of an early threat evaluation system. More specifically, threat representations of ambiguous stimuli, competing for processing resources with innocuous representations of these same stimuli, may be more or less likely to obtain processing priority, depending on the

system's responsiveness. Variations in the responsiveness of this threat evaluation system are thus suggested to determine individual differences in interpretive bias, and therefore, in anxiety vulnerability. At least three factors are suggested to influence the threat evaluation system's responsiveness, and thus the valence of interpretation likely to be imposed on a particular ambiguous stimulus. Firstly, individuals may vary in the variety and detail of threat representations that fall within its domain. Secondly, individuals may vary in terms of inherited variation in the system's threshold of activation. Thirdly, individuals may also vary in their capacity to suppress its activation through concentration on task demands. Studies could potentially be designed to independently manipulate such factors (other than inherited variation), and the impact of each such independent manipulation upon interpretive bias and vulnerability then assessed. Such work could serve to refine understanding of the cognitive mechanisms underlying the expression of interpretive bias, and therefore of anxiety vulnerability.

As well as providing direct support for cognitive models of anxiety vulnerability in general, the present findings are certainly consistent with the various models of anxiety pathology which suggest that interpretive bias may causally elicit or sustain anxious symptomatology (e.g., Beck & Clark, 1988, 1997; Beck et al. 1985; D . M. Clark, 1986; D. M. Clark & Wells, 1995; Rapee & Heimberg, 1997). Nonetheless, caution should be taken in generalising the implications of these findings to explanations of anxiety disorder. This is not only because the individuals tested in the present studies came from the normal range of anxiety vulnerability (rather than being selected on the basis of clinical diagnosis), but also because some models emphasise threat-congruent interpretations of specific classes of ambiguous stimuli (such as interoceptive stimuli) as important in the aetiology of specific anxiety disorders.

For example, D. M. Clark's (1986) model of panic disorder emphasises the notion that panic disordered individuals may interpret benign variations in interoceptive stimuli as threatening, and that these interpretations may contribute to the intensity of panic. A finding which is consistent with this view, is that panic disordered individuals are more likely to experience panic attacks when induced to hyperventilate (cf. Nardi et al., 2004). Hyperventilation may replicate respiratory changes which are interpreted as threatening by panic disordered individuals. Adaptations of the training procedures used in the present series of studies could enable laboratory investigation of the hypothesis that threat-congruent interpretive bias can lead to disproportionate state anxiety elevation under conditions of hyperventilation. Non-anxious individuals might first be provided with interpretive training involving threat/non-threat homographs with threatening meanings related to physical illness or disorder (e.g., stroke). When subsequently induced to hyperventilate, the group of individuals who receive threat-congruent training should display a greater degree of elevation in state anxiety than the individuals receiving the non-threat-congruent training. Other procedures which elevate bodily arousal could similarly be used as stressors in assessing the influence of interpretive bias upon response to such interoceptive cues.

Of course, even work specifically involving assessment stimuli and stressors relevant to a model of a specific anxiety disorder, could not conclusively demonstrate the importance of interpretive bias for this disorder, if experiments were confined to non-anxious individuals. Further extension, for the above example, could thus involve the provision of training designed to reduce threat-congruent interpretations for physically relevant stimuli in panic disordered individuals. Relative to a non-contingent baseline condition, the effectiveness of such training in reducing these individuals'

experience of panic in physical-challenge situations, such as hyperventilation, would then provide further direct support for D. M. Clark's (1986) model of panic disorder. Of course, other models of specific anxiety disorders could receive similar investigation (e.g., Clark & Wells, 1995; Rapee & Heimberg, 1997).

Implications for the relationship of interpretive bias to different negative emotions.

In the analysis of findings from Phase 2 of this research, separate analyses of anxiety and depression ratings could not sustain the hypothesis that the training group assignment influenced depressive reaction to the stressor, whereas of course the groups differed significantly in their degree of anxious reaction. However, the overall analysis of scores from both scales also could not confirm that anxious reactivity to the stressor was modified more than depressive reactivity. Thus, the hypothesis that a negative interpretive bias may causally mediate vulnerability to depression as well as anxiety was not rejected by these findings. This outcome may parallel the broader literature, in which interpretive assessment studies have been more equivocal in demonstrating the association of interpretive bias with depression than with anxiety. Studies using techniques susceptible to response bias and demand effect explanations have, as for anxiety vulnerability, suggested that interpretive bias and depression may indeed be associated, for clinically depressed, in comparison to non-depressed individuals (e.g., Butler & Mathews, 1983; Miller & Norman, 1986), as well as for normal variations in depression (e.g., Forgas, Bower, & Krantz, 1984; Kavanagh & Bower, 1985).

However, of those studies which have investigated the association of interpretive bias with depression, only a small number have been reported which are not susceptible to response bias and demand effect criticisms. The outcomes of these studies have been

inconsistent. Lawson and MacLeod (1999) employed a priming procedure to assess interpretive bias in high and low depressed individuals. Participants were presented with sentences with negative and neutral meanings, followed by targets which could be related to either of these meanings. The responses of high, relative to low depressed participants, gave no indication that depressed individuals imposed negative, as opposed to neutral interpretations upon these sentences. In contrast, Lawson, MacLeod, and Hammond (2002), using a physiological measure of interpretation, obtained evidence suggesting that the high depressed individuals, relative to the low depressed group, indeed imposed negative interpretations upon emotionally ambiguous word stimuli.

It is possible that such discrepancies may be resolved by considering these findings in terms of the tripartite model of anxiety and depression provided by L. A. Clark and Watson (1991). In this model, anxiety and depression share a common, general distress component, while anxious arousal is suggested to be a specific characteristic of anxiety, and reduced positive affect is suggested to be specific to depression. As Pury (2004) has suggested, it could be that a negative interpretive bias causally mediates the component of general distress common to anxiety and depression. Lawson and MacLeod (1999), who failed to demonstrate the hypothesized direction of association between depression and negative interpretive bias, selected participants high and low on the Beck Depression Inventory (Beck et al., 1961), but who fell into the mid range of scores on the STAI-T. The BDI may be considered a measure of low positive affect, rather than of general distress, while the STAI may better capture the general distress component of emotional vulnerability (Andrade, Gorenstein, Vieira-Filho, Tung, & Artes, 2001). Thus, these researchers may have excluded the variation in that aspect of emotion which is actually mediated by negative interpretive bias (Pury, 2004).

Lawson et al.'s (2002) high and low depressed groups also differed on a measure of trait anxiety. Thus, in contrast to Lawson and MacLeod (1999), Lawson et al. (2002) may have successfully demonstrated the association between interpretive bias and the general distress component common to anxiety and depression.

In the present series of studies, it is possible that the mood measures captured by the VAMS scales were somewhat complicated by these different components of depression, which might be differentially associated with negative interpretive bias. This could thus explain the more equivocal support for the causal role of these manipulations in mediating depressive, as opposed to anxious reactivity. This explanation would require fuller assessment in causal investigations, perhaps by employing a measure of mood state which better captures the distinction made by L. A. Clark and Watson (1991), such as the Positive and Negative Affective Scale (PANAS: Watson, Clark, & Tellegen, 1988).

Applied Implications

These findings are consistent with those suggested approaches to treatment for various anxiety disorders which are premised, at least in part, upon the assumption that reducing the tendency for threat-congruent interpretations will ameliorate anxious symptoms (e.g., Beck et al., 1985; D. M. Clark & Ehlers, 1994; Salkovskis & Clark, 1991; Wells, 1997). Of course, the beneficial effects of a treatment strategy geared specifically towards modifying such interpretations would be supported more directly by the demonstration that such modification can also influence anxiety in disordered individuals, as opposed to the unselected participants involved in the present studies.

An intriguing possibility is that versions of the interpretive training procedures employed here might themselves serve as means of effectively reducing the symptoms of various anxiety disorders, through the amelioration of threat-congruent interpretive bias. The most direct extension of this present interpretive training approach would be in attempts to reduce the emotional impact of acutely stressful situations, such as where individuals with specific phobias are exposed to their feared stimuli or situations. For example, socially phobic individuals might receive training designed to ameliorate threat-congruent interpretations of social cues, prior to exposure to socially threatening situations, such as interviews and oral presentations.

Further extension of this training approach could involve provision of repeated sessions of the non-threat-congruent training to anxiety disordered individuals, with the goal of effecting sustained reduction of threat-congruent interpretive bias, and by hypothesis, anxious symptoms. Of course, it is likely that such training procedures, if effective in reducing the experience of anxious symptoms, would form one element in a broader regime designed to target the various factors which underlie the experience of particular anxiety disorders.

Limitations Of Present Approach

Within a single empirical thesis, it is necessary to restrict the scope of studies to a particular range of experimental designs, and to a particular range of stimuli, tasks, and populations. Therefore, it is necessary to consider the limitations of this particular research approach.

Specificity Of Interpretive Assessment Tasks And Materials

Like much of the previous work on interpretive bias, the interpretive training and assessment tasks employed in this present research used lexical stimuli. Furthermore, although the assessment task in some studies differed from the training task, priming methodologies were always used in each case. It therefore remains possible that the influence of these training tasks is confined to the cognitive processes required to interpret single ambiguous words, when these are presented in priming tasks such as those used in interpretive assessment.

A recent study investigating the influence of similar training tasks upon interpretations of homographs used a rather different assessment approach. Following completion of a version of Grey and Mathews' (2000) training procedure, Hertel, Mathews, Peterson, and Kintner (2003) then presented participants with homographs not presented during training, and participants were required to generate and describe images evoked by these stimuli. These researchers found that the valence of images reported to have been generated in this assessment was congruent with the valence of the interpretive bias encouraged during interpretive training. Thus, it appears that interpretive training effects, induced using similar procedures, may be detected using measures of interpretive bias other than priming tasks, at least for this same class of ambiguous stimuli.

Nonetheless, in assessing the induced biases, Hertel et al.'s (2003) studies still employed homographs, presented visually in interpretive training and assessment. Thus, it remains possible that this interpretive training might only influence the interpretation of visually presented homographs. It might then be useful to ensure that these training procedures can, at the very least, influence interpretations of ambiguous stimuli

presented in other sensory modalities. To do so, investigators could, for example, use a cross-modal priming approach (e.g., Gaskell & Marslen-Wilson, 2002) to modify these interpretive assessment tasks. In cross-modal priming tasks, the processing of primes presented in one modality (e.g., auditory) is assessed in terms of the degree to which prime presentation facilitates identification of related targets presented in an alternative modality (e.g., visual). To assess the influence of interpretive training upon processing in other sensory modalities, threat/non-threat homographs could thus be presented auditorily in interpretive assessment. The degree to which the present training procedures influenced the interpretation of stimuli presented in this alternative modality could be assessed in terms of responses to visually presented targets, related to the threatening and non-threatening meanings of preceding, auditorily presented primes.

Of course, even such cross-modal priming procedures could not exclude the possibility that the interpretive bias, induced using lexical stimuli in training, might apply only to the interpretations imposed upon lexical stimuli, or even simply upon homographs. Researchers have in several instances measured interpretive bias using other classes of stimuli, such as facial stimuli (e.g., A. Richards, French, Calder, & Webb, 2002; Winton, Clark, & Edelmann, 1995). For example, Richards et al. assessed high and low socially anxious individuals' interpretations of images of ambiguous facial expressions. These ambiguous images were created by interpolating (morphing) between initial endpoints representing different emotional extremes (e.g., fearful and happy). Thus, the face depicted in an image representing an equal combination of these emotional extremes might be interpreted as happy or as fearful. In Richards et al.'s study, relative to low socially anxious individuals, high socially anxious individuals displayed an enhanced sensitivity to detect fearful expressions when images of these

fearful facial expressions were morphed with images depicting faces showing other expressions (e.g., happiness or disgust). If the induced biases obtained in the present research are not specific to the interpretation of lexical stimuli, then it should be possible to detect these biases using novel classes of ambiguous stimuli, such as emotionally ambiguous facial images.

A further issue that may be considered with respect to the obtained pattern of training effects in the present research program, relates to the use of the unambiguous prime condition as a baseline in the majority of interpretive assessments. As will be recalled, the unprimed condition, in which targets appear in the absence of preceding primes, was replaced by the unambiguous prime condition in order to ensure that the overall tendency to employ the prime in responding to the target was not influenced by the inclusion of a large number of trials in which no meaningful priming stimulus was presented. It could be argued, however, that the use of this alternative unambiguous prime baseline prevents us from ruling out certain alternative accounts of the training effects. For example, it is possible that a general group difference in the activation of valenced information, brought about by the training, would be less easily detected in this condition than in a pure unprimed condition, since the unambiguous valenced primes might activate corresponding representations in all individuals to such an extent that pre-existing differences are no longer detectible on the basis of target responses. It might therefore be useful for future studies to re-introduce this unprimed condition into the assessment trials, in order to more firmly rule out this alternative account of the training effects.

Difficulty Of Generalizing From Induced To Naturally Observed Variations In Interpretive Bias

This work has demonstrated that interpretive bias *can* influence anxiety vulnerability, at least within a contrived laboratory setting. However, it has not been demonstrated that this bias *does* make a meaningful causal contribution to naturally occurring variations in interpretive bias and anxiety vulnerability. These findings do not exclude the possibility, for example, that the naturally observed association between interpretive bias and anxiety vulnerability is explained largely by the reverse direction of causation; specifically, that the more frequent and more intense state anxiety elevations experienced by vulnerable individuals predispose these individuals to develop threat-congruent interpretive bias. Though interpretive bias may causally mediate vulnerability, this particular direction of causation might be less important in explaining the real-world association.

More direct support for the role of interpretive bias as a meaningful causal mediator of anxious response to real-life stressors would come from studies employing repeated sessions of the same or a similar training procedure to effect more permanent changes in interpretive bias. Of course, in line with previous discussion, the goal of such long-term training should be to ameliorate threat-congruent interpretive bias in anxious individuals, rather than to enhance it. To the extent that interpretive bias is an important causal mediator of anxiety vulnerability, such permanent reductions in threat-congruent interpretive bias should also have important consequences for vulnerability.

Alternative Explanations For Influence Of Training Upon Anxiety Vulnerability

The training tasks were observed to induce task responses consistent with induction of group differences in interpretive bias, and also to produce subsequent group differences in anxiety reactivity. Certainly this is consistent with the hypothesis that this interpretive bias manipulation causally impacted upon anxiety vulnerability. Nonetheless, it remains possible that some feature of the training, other than its influence upon interpretive bias, might account for the induced group difference in emotional reactivity.

A key consideration here is that during the training trials themselves, the threat training group would likely have more frequently accessed threatening word meanings than the non-threat training group. This is not only because the training contingencies encouraged these groups to access differentially valenced homograph meanings, but also because the target stimuli required to instantiate each of the two training contingencies probably were themselves differentially valenced. This observation, that one training group were exposed to threatening meanings more often than the other training group, might then account for the subsequently observed group difference in emotional reaction, without alluding to the influence of this training upon interpretive bias.

For example, it might be suggested that the greater number of threatening meanings accessed by the threat training group led these participants to particular beliefs about the purposes of training. The elevated state anxiety ratings provided by this group may thus have reflected experimental demand effects. However, in informal debriefings following the experimental sessions, in no case did participants from either training group accurately articulate the aims or hypotheses of the studies. Most

importantly, this demand explanation would most plausibly give rise to a group difference in mood ratings that would be evident on scales provided directly following the interpretive training, but prior to the subsequent video stressor. In fact, across the four studies, on average, the threat training group provided slightly (though non-significantly) smaller mean anxiety and depression VAMS ratings, relative to the threat training group, at the mood assessment point which directly followed the interpretive training/assessment trials. Instead, it was anxious reaction to the video stressor which was influenced, consistent with the conclusion that the training modified anxiety vulnerability, rather than mood state per se. Thus, there is no compelling support for this demand effect explanation.

However, there may be more plausible explanations for the influence of the training procedure upon anxiety vulnerability which do not involve interpretive bias. One possibility is that the requirement to repeatedly access threat representations may have prompted rumination on negative personal experiences amongst the threat trained participants. Such negative rumination may have continued beyond the training trials. If the content of the video stressor was in some cases related to the content of this rumination, then this could have exacerbated the emotional impact of the video stressor, leading to disproportionate elevation in negative mood state amongst the threat trained participants, relative to the non-threat trained individuals.

The favored explanation, that interpretive bias was the relevant causal mediator of emotional reaction, more parsimoniously accommodates the correspondence between the pattern of changes in interpretive bias and in anxiety reactivity. Nonetheless, it would be reassuring to exclude such alternative explanations as the one described above. One approach to assessing this issue within future research studies could be to

introduce control conditions that replace the present training trials with exposure only to unambiguous threatening primes and associated targets, or only to unambiguous non-threatening primes and associated targets. These groups would not be expected to develop differential interpretive biases, but would still be predisposed to access threat representations with differing frequency. To the extent that the modification of interpretive bias is crucial to the influence of the present training procedures upon emotional reactivity, no group difference in emotional reactivity should be observed among participants receiving these suggested additional control conditions.

Future Directions

While these findings may hold some important implications for the understanding of anxiety vulnerability, and perhaps for the treatment of anxiety symptomatology, they also raise a number of questions and issues which could be addressed by subsequent research. A selection of such possibilities for future research are considered in this final section.

Assessing Alternative Explanations Of The Present Findings

Some alternative explanations for the observed influence of interpretive training upon emotional vulnerability were discussed in the limitations section. One goal of future work might be to empirically evaluate these alternative hypotheses, thereby tightening the conclusions which may be drawn on the basis of these findings.

In addition to those suggestions for studies made previously, one useful approach to excluding alternative explanations could be to modify the degree of ambiguity inherent in the stressors which are provided following interpretive training.

To the extent that the interpretation of ambiguity is the crucial factor in modifying anxiety vulnerability, some degree of emotional ambiguity in the stressor should be crucial to eliciting training-induced group differences in anxiety reactivity.

In the present studies, the video clips presented in the video stressor were selected not only to be ecologically valid, but also to provide some features which might be differentially interpreted (e.g., the outcomes of the scenarios). However, it would be possible to modify these video clips in a number of ways to enhance or reduce their inherent ambiguity. For example, the film clips could be digitally altered to obscure cues disambiguating certain threatening aspects of each situation, such as the state of health of the individuals depicted. Alternatively, completely different stressors (such as those involving social feedback) could also be modified to enhance or reduce their ambiguity.

Subgroups of participants from threat and non-threat training groups might each receive variants of such modified stressors which have been designed to be more or less emotionally ambiguous. To the extent that interpretive bias is an important factor in determining the influence of the training upon vulnerability, the training groups should show *differential* emotional reaction only to the ambiguous versions of these stressors.

Specifying Causal Relations Between Different Interpretive Biases And Different Emotions

The possibility that these findings might be understood in terms of the tripartite model of anxiety and depression provided by L. A. Clark and Watson (1991), has already been considered. In particular, it has been suggested that a negative interpretive bias might causally underpin the general distress component that is common to both

anxiety and depression. Pury (2004) has elaborated on this view, suggesting also that low positive affect, specific to depression, may be causally mediated by a tendency to access the less emotionally extreme interpretations of ambiguous stimuli, irrespective of valence. Thus, if ambiguous stimuli encountered in everyday life more often have potentially positive than potentially negative interpretations, then a tendency to impose neutral interpretations on ambiguous stimuli could prevent prone individuals from maintaining a positive emotional state.

To investigate this hypothesis, Pury (2004) assessed interpretive bias in individuals scoring high and low on a measure of a positive affect. The interpretive assessment was a priming methodology similar to the one employed by Lawson and MacLeod (1999), using emotionally ambiguous sentences as primes. In different trials, the prime sentences were ambiguously positive/neutral or negative/neutral. Individuals low in positive affect, relative to those high in positive affect, displayed slowing to respond to targets related to emotional, relative to neutral, interpretations of the ambiguous prime sentences, irrespective of whether the emotional interpretations permitted by these sentences were positive or negative. Thus, it was concluded that individuals low, relative to high, in positive affect, were biased to impose less emotionally extreme interpretations upon the primes.

These findings are consistent with the intriguing possibility that low positive affect may be causally mediated by an interpretive bias away from emotional interpretations. However, the way in which seemingly conflicting emotionally negative and non-emotional biases would operate in typically depressed individuals (i.e., those high in distress, as well as low in positive affect), remains to be explained. Nonetheless, other researchers have advanced similar hypotheses, suggesting that negative

interpretive bias, and positive interpretive bias, may demonstrate some degree of independence, and each may have independent emotional consequences (e.g., Byrne & Eysenck, 1993; Huppert, Foa, Furr, Filip, & Mathews, 2003). For example, Byrne and Eysenck (1993) allude to Tellegen's (1985) two-dimensional framework of mood and personality, in suggesting that individuals low in positive affect may produce fewer positive interpretations, and individuals high in negative affect may produce more negative interpretations, with some degree of independence between these biases.

In light of these suggestions, it may well be informative for future research to attempt to manipulate positive and negative interpretive biases independently. Such work could perhaps use variants of the present interpretive training methodologies. For different training conditions, the primes could be homographs with positive and neutral, or with negative and neutral, interpretations, and different training groups could be induced to access the emotionally relevant or the neutral interpretations in each case. The relative independence of these different training manipulations for subsequent interpretations of novel ambiguous stimuli, and for emotional reaction to various forms of mood induction (i.e., experiences with potentially positive, or else with potentially negative interpretations), could then be investigated.

In all such work, the interpretation of findings could be facilitated by matching the participants allocated to different training conditions on the basis of emotional characteristics such as trait anxiety. Although in the overall analysis of ratings from studies 5 to 8 there was no difference in emotional characteristics of training groups, in at least one case (Study 7) a pre-existing chance difference in the emotional vulnerability of participants assigned to the different training conditions was detected on the emotional questionnaires. Matching participants assigned to different training

conditions on the basis of such characteristics would not only prevent such chance differences, but also would likely reduce the degree of unsystematic variation in responses shown on bias and vulnerability assessments, thereby maximizing the chances of detecting real training effects.

Extension Of Interpretive Training Approach To Clinically Anxious Populations

The possibility that future work might attempt to extend this training approach to clinical populations has already been discussed, and two potential applications have been suggested. The first, and most direct, is that interpretive training might be used to reduce the emotional impact of a stressful situation, by provision of non-threat-congruent interpretive training immediately prior to exposure to the stressor. A second possibility is that amongst anxiety disordered individuals, interpretive training might be provided across a series of sessions, to achieve sustained reductions in interpretive bias, and therefore in anxiety symptoms.

If the delivery of this training approach was demonstrated to successfully reduce anxious symptoms, then another avenue again for the application of such interpretive training, could be in inoculation against the development of heightened anxiety vulnerability. Individuals with a known susceptibility to the development of anxiety disorders (e.g., first-degree relatives of anxiety disordered individuals) might be provided with non-threat-congruent interpretive training in order to prevent the initial development of anxious symptoms. Thus, such training procedures could be used as relatively unobtrusive means of preventing the development of anxiety symptomatology.

Given the possibility that such training might indeed have the capacity to significantly impact upon the interpretive bias, and thus vulnerability, of high anxiety vulnerable individuals, ethical consideration would suggest that such investigations should use no-training baseline conditions, rather than threat training conditions. Indeed, although participants in this present series of studies were not screened for vulnerability prior to training group allocation, it might be circumspect to do so, given that someone high in vulnerability might by chance be allocated to a negative training condition.

Of course, the successful application of these training procedures would have theoretical, as well as practical importance. The demonstration that non-threat-congruent interpretive training can serve to reduce anxious response to feared stimuli, or can reduce symptoms of an anxiety disorder, or can help to prevent the development of anxiety disorder, each would support the argument that interpretive bias plays an important role in the etiology of anxiety disorders.

Conclusion

Extensions of the methodologies described within this thesis may provide much scope for subsequent investigations, and the findings from such investigations may have great practical as well as theoretical relevance. In and of itself however, this present work may also have some important implications. First of all, it has been demonstrated that interpretive bias can be systematically and specifically manipulated through the modification of priming procedures. Secondly, the induction of group differences in interpretive bias has been demonstrated to be directly linked to congruent group differences in subsequent anxious reactivity to stress. Thus, most importantly, this work

has provided evidence which directly supports the hypothesis that threat-congruent interpretive bias can causally mediate anxiety vulnerability.

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APPENDIX I

INSTRUCTIONS PROVIDED FOR THE DOMINANCE RATING TASK

This task is concerned with word meanings. On each trial, a word will appear at the top of the screen in yellow. About a second after this word appears, two sentences will appear, one above the other. On each individual trial, each sentence will provide an alternative meaning for the word shown in yellow. Below each sentence will be a number which ranges from 0 to 100. Your task will be to rate the percentage of the time with which you consider the word to be used in a sense consistent with each definition in every day language, by adjusting these numbers.

Change these numbers by moving the mouse up or down. You will notice that as one number goes up, the other always goes down, so that the two numbers always add up to 100.

When you have adjusted the percentage under each meaning to the point that you feel best represents the way the word is used in every day language, press the left mouse key, and the next trial will begin.

Half way through the task, there will be a break. Press the return key to continue after this break. When you are ready to begin, press the return key.

APPENDIX II

INSTRUCTIONS PROVIDED FOR THE THREAT RATING QUESTIONNAIRE

This questionnaire involves a large list of words. Each target word is indicated by the asterisk next to it. Below each word is a short definition of the meaning of the word, given in brackets.

Beside each word is a scale, ranging from 0 (not at all threatening) to 6 (extremely threatening). Your task is to rate how threatening you find the target word, with the given definition in mind. Do this by circling a number on the scale provided, in accordance with how threatening you find the word.

You will see some words repeated in this questionnaire; this is in fact intentional. Please rate the word in accordance with the given definition on each occasion.

When you have finished this questionnaire, your task is complete.

APPENDIX III
ARRAY OF THREAT/NON-THREAT HOMOGRAPHS AND ASSOCIATES

Homograph	Non-threat associates				Threat associates			
abort	schedule	timetable	cancel	agenda	unborn	babies	foetus	pregnant
appeal	likeable	enticing	intense	sexual	verdict	plead	lawful	conviction
arms	akimbo	stretch	length	shoulders	weapons	pistols	shotguns	rifles
arresting	noticeable	stunning	obvious	attractive	policeman	officer	jailer	criminal
badger	animal	beaver	creature	night	bother	pester	torment	irritate
bark	plants	foliage	covering	trees	hound	growl	howling	yapping
bars	beers	tavern	drinks	alcohol	restrain	barricade	chain	behind
batter	pudding	pancake	fried	paste	bruise	mistreat	abuse	wife
beat	rhythm	accent	tempo	drumming	slapping	smash	strike	impact
bind	connect	fasten	attach	folder	problem	perplexity	crisis	dilemma
bit	fragment	little	piece	crumb	chew	teeth	tooth	snap
bitter	almonds	flavour	acrid	drink	vicious	resentful	hostile	scarred
blind	cover	shade	window	curtain	sight	eyes	darkness	seeing
block	cement	wooden	brick	metal	obstacle	deter	obstruction	prevent
blow	breathe	puff	inhale	exhale	misfortune	setback	calamity	trauma
body	shape	physique	person	naked	cadaver	carcass	corpse	remains
booking	theatre	reservation	tickets	office	apprehension	arrest	fining	prosecute
bound	gallop	bounce	skip	sprint	captive	confined	hostage	tight
box	shoe	cardboard	storage	carton	biff	match	assault	scuffle
brood	chicks	birds	eggs	newborn	meditate	obsess	ponder	angry
cane	sugar	wicker	furniture	bamboo	headmaster	smack	teacher	school
capital	expenditure	investment	assets	income	corporal	gassing	punishment	offense

Homograph	Non-threat associates				Threat associates			
cell	biology	amoeba	organism	microscopic	jail	warden	guard	prison
charge	price	credit	expensive	account	accusation	blame	allegation	indictment
chill	brisk	freezing	frost	wintry	frightening	fearsome	afraid	terror
chop	barbeque	butcher	steak	hamburger	knife	whacked	severed	hacked
committed	agreed	dedicated	worker	engaged	asylum	uncontrollable	breakdown	psychiatrist
cracked	mirror	varnish	porcelain	glasses	demented	madness	paranoid	insane
crank	crane	shaft	handle	wheel	phoney	idiot	eccentric	weirdo
critical	essay	analysis	criticism	particular	dangerous	condition	dying	deathly
cross	church	symbol	naughts	religion	complaining	fuming	annoyed	cranky
crush	orange	blend	squash	pineapple	massacre	crumble	destroy	defeat
decline	invitation	politely	offer	accept	frailty	sickness	degeneration	senility
die	gamble	game	throw	numbers	expire	perish	deceased	demise
dressing	clothed	adorning	robing	gown	treatment	gauze	hospital	bandage
execute	accomplish	achieve	effect	complete	electrocute	behead	punish	shoot
fine	miniscule	small	powdery	granular	legal	payment	infringement	ticket
fit	athletic	exercise	healthy	strong	convulsion	spasm	epileptic	seizure
frame	photograph	ornate	aluminium	painting	innocent	implicate	incriminate	evidence
gag	prank	laugh	funny	witty	kidnap	tied	muffle	mouth
graze	horses	farmers	fields	cattle	scrape	blood	scratch	knee
growth	height	maturation	shrink	development	cyst	fungus	tumor	swelling
hamper	picnic	basket	christmas	sandwiches	restrict	frustrate	impede	obstruct
hang	raincoat	jacket	picture	clothing	neck	noose	strangle	suicide
hit	popular	success	favourite	record	smite	violence	concussion	whack
incense	smell	smoky	scent	odour	aggravate	fury	inflame	provoke
infectious	music	laughter	attitude	enthusiasm	contagious	epidemic	plague	influenza

Homograph	Non-threat associates				Threat associates			
institution	traditional	wedding	family	marriage	hysterical	psychiatric	deluded	psychotic
late	punctual	early	arrival	delayed	morgue	cremation	grieve	buried
lie	relax	recline	horizontal	mattress	cheat	untrue	deceive	truth
low	level	temperature	roof	higher	unfair	underhanded	backstabbing	sneaky
maroon	clothing	brown	colour	reddish	stranded	shipwreck	isolated	helpless
mean	propose	expect	intention	intend	unkind	horrible	stingy	generous
mine	belonging	yours	possession	ours	ammunition	field	explosive	bomb
mug	tankard	flagon	teacup	vessel	attack	thief	robbery	assail
nuts	squirrel	raisins	monkey	hazel	madman	lunatic	unbalanced	crazy
odd	single	number	unmatched	unpaired	weird	bizarre	peculiar	unusual
parting	haircut	brushing	center	style	sorrow	separating	company	sadness
petrified	hardened	solidified	fossilized	granite	horrified	fearful	freaked	aghast
plot	scenario	storyline	thread	unfolding	against	secretive	gunpowder	conspire
pound	ounce	kilogram	heavy	scales	injure	wallop	pulverize	thump
punch	refreshment	bowls	liquor	juice	kicked	knuckles	boxing	knock
quiver	archer	quill	arrow	bowman	tremble	shudder	scared	palpitate
ram	ewes	paddock	horns	sheep	collide	crash	accident	barge
rank	soldier	order	general	military	repellent	putrid	offensive	repulsive
rattle	baby	noisy	percussion	instrument	fluster	unnerve	disturb	confuse
revolution	gyration	revolving	turning	rotation	overthrow	communism	insurrection	anarchy
row	column	houses	queue	series	debate	fight	argument	shouting
ruin	ancient	archaeology	castle	historical	career	bankrupt	reputation	impoverish
sack	container	potatoes	cloth	hessian	retrench	employment	unemployed	redundant
scan	peruse	scrutinize	survey	study	diagnosis	xray	medical	cancer
shady	cloudy	sunny	forest	shadow	character	dishonest	mysterious	suspicious
shaken	canister	cocktail	bottle	stirred	frightened	upset	traumatized	agitated
sharp	acute	shrewd	keen	clever	blade	razor	pointed	blunt

Homograph	Non-threat associates				Threat associates			
shell	beach	oyster	fishes	seaside	mortar	artillery	casing	grenade
shot	shaker	beverage	glass	spirits	killed	gunned	pistol	wounded
sink	washing	kitchen	bathroom	dishes	immersed	float	swimming	drown
slice	doling	plateful	portion	bowlful	slash	dissect	cleave	impale
sour	oranges	fruit	taste	lemons	displeased	unpleasant	dissatisfied	unhappy
stalk	flower	plant	leafy	branch	pursue	stealth	obsessed	creep
state	affirm	pronounce	declare	assert	plight	situation	panic	predicament
stern	cruise	captain	galley	yacht	tough	strict	frown	harsh
strained	separate	percolate	vegetable	sieved	worry	headache	tension	anxiety
striking	resemblance	pretty	extraordinary	dazzling	hitting	force	cuffing	pummeling
stump	oaken	trunk	lumber	fence	hobble	limb	maimed	amputation
temper	soothe	moderate	mitigate	soften	outrage	tantrum	furor	annoy
tense	future	verb	grammar	language	stressed	relaxed	nervous	anxious
terminal	aeroplane	airport	depot	station	fatal	illness	virus	disease
tramp	trundle	stomp	stamp	trudge	pauper	homeless	vagrant	despondent
twisted	string	contorted	straight	coiled	deranged	perverted	cruel	strange
undertaking	enterprise	venture	endeavour	effort	mortuary	burial	coffin	funeral
vault	jump	hurdle	lurch	launch	underground	cavern	chamber	enclosure
wake	yawn	arise	asleep	morning	ritual	death	respects	vigil
will	desire	power	determination	purpose	inheritance	solicitor	testament	testimony
wound	tightly	wrapped	twine	bundle	laceration	flesh	lesion	suture
wrench	screwdriver	tools	mechanic	unbolt	tugged	dislocate	strain	sprain

APPENDIX IV

INSTRUCTIONS PROVIDED FOR THE FRAGMENT-COMPLETION TASK (STUDIES 2 AND 3)

In this task, every trial will begin with the presentation of a set of three white crosses. You should always fix your eyes on these. Soon afterwards, these crosses will disappear and a string of yellow letters will appear, which may either be a word, or a series of 5 ‘X’s (‘XXXXX’).

After two seconds, a white word fragment will replace these yellow letters. The word fragment will be a real word with some of its letters replaced with dashes. On trials in which a yellow word is presented, it can be used as a cue in solving the word-fragment, which will be related in meaning.

Using the related yellow word to assist you where applicable, your task is to try to think of the word that matches the word fragment. As soon as you have thought of the word that matches the fragment, press the left mouse button. You will then be prompted to enter the first missing letter of the fragment.

You will be given a maximum of 6 seconds to respond to complete each word-fragment. Please try to be as quick, but also as accurate as you can, on each trial.

APPENDIX V

PRACTICE STIMULI EMPLOYED IN THE FRAGMENT-COMPLETION TASK (STUDIES 2 AND 3)

Prime	Associate
cabin	countryside
cat	mouse
deck	chair
eternal	forever
irregular	typical
irrigation	water
paused	halted
radio	sound
round	circle
sculpture	artist

APPENDIX VI

UNAMBIGUOUS THREAT PRIMES AND ASSOCIATES

Unambiguous threat prime	Threat associate
abandoned	destitute
abduction	ransom
appal	disgust
asphyxiate	smother
bully	dominate
carnage	slaughter
compulsive	addiction
conniving	deceiving
cyanide	arsenic
deaf	mute
disappoint	discourage
dreadful	awful
duress	coercion
edgy	excitable
emphysema	lungs
entombed	crypt
felon	convict
graveyard	sacred
ill	bedridden
infuriate	madden
injection	infusion
intruder	invader
lurk	dwell
maggots	flies
murder	homicide
persecute	victim
quarrel	bicker
resuscitate	revive

Unambiguous threat prime	Threat associate
selfish	greedy
startle	surprise
trespass	property
uproar	commotion

APPENDIX VII

UNAMBIGUOUS NON-THREAT PRIMES AND ASSOCIATES

Unambiguous non-threat prime	Non-threat associate
accumulate	acquire
art	gallery
boat	canoe
borrowing	lending
build	assemble
cereal	breakfast
craft	carpentry
delegate	assign
dolphin	porpoise
elephant	enormous
emperor	leader
gigantic	huge
halve	divide
humane	philanthropic
infant	young
infinite	limitless
janitor	servant
jukebox	songs
luggage	suitcase
mother	father
mountain	molehill
porch	pergola
pot	kettle
quest	tasks
rehearse	practice
trombone	trumpet
utensil	cooking

Unambiguous non-threat prime	Non-threat associate
variety	assortment
vitamins	minerals
wander	meander
weather	forecast
zebra	stripes

APPENDIX VIII

INSTRUCTIONS PROVIDED FOR THE MODIFIED VERSION OF THE FRAGMENT-COMPLETION TASK (STUDIES 4,5, 7 & 8)

In this task, every trial will begin with the presentation of a set of three white crosses. You should always fix your eyes on these. Soon afterwards, these crosses will disappear and a word in yellow letters will appear. After less than a second, a white word-fragment will appear, just below this yellow word. The word-fragment will be a real word with some of its letters replaced with dashes. This word will always be related in meaning to the yellow word.

Using the related yellow word to assist you, your task is to try to think of the word that matches the word-fragment. As soon as you have thought of the word that matches the fragment, press the spacebar. You should then enter the first missing letter of the word-fragment.

Do this as quickly and accurately as you can.

APPENDIX IX

INSTRUCTIONS PROVIDED FOR THE RELATEDNESS-JUDGMENT TASK

In this task, every trial will begin with the presentation of a set of three white crosses. You should always fix your eyes on these. Soon afterwards, these crosses will disappear and a word in yellow letters will appear. Less than a second later, two white words will appear below the yellow word. One of these words will be related in meaning to the yellow word.

Using the mouse, your task is to indicate which of the two words is related to the yellow word. If the word on the left is related to the yellow word, press the left mouse button. If the word on the right is related to the yellow word, press the right mouse button. Do this as quickly and accurately as you can.

APPENDIX X

PRACTICE STIMULI EMPLOYED IN STUDIES 4-8

Prime	Associated target	Irrelevant foil
cabin	countryside	kernel
cat	Mouse	parcel
deck	chair	dissolve
eight	nine	parrot
eternal	forever	patriarchal
fabric	material	puzzle
factory	manufacture	ivy
finger	thumb	mime
footprint	feet	helm
global	world	magnify
helicopter	pilot	labour
insulation	cooler	nautical
irregular	typical	measured
irrigation	water	notation
multiple	many	opera
normal	usual	panel
padlock	keys	gable
paused	halted	quilt
pyramid	egyptian	rabbit
radio	sound	scaffold
round	circle	discuss
sculpture	artist	rocket
travel	overseas	fraction
woman	feminine	manifest

APPENDIX XI

INSTRUCTIONS PROVIDED FOR COMPLETION OF VISUAL ANALOGUE MOOD SCALES

You will see a scale like this during the experiment on a couple of occasions. When this occurs, your task will be to indicate how you feel right at the moment at which the scale is presented, with reference to the words at the scale's ends. Each time, use the mouse to move the cursor to the position on the scale that indicates how you currently feel. When the cursor is at about the right point, press the left mouse button.