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Patterns of Processing Bias for Emotional Information Across Clinical Disorders: A Comparison of Attention, Memory, and Prospective Cognition in Children and Adolescents With Depression, Generalized Anxiety, and Posttraumatic Stress Disorder

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This study investigated theoretical claims that different emotional disorders are associated with different patterns of cognitive bias, both in terms of the cognitive processes involved and the stimulus content that is preferentially processed. These claims were tested by comparing clinically anxious (generalized anxiety disorder [GAD], posttraumatic stress disorder [PTSD]) and clinically depressed children and adolescents on a range of cognitive tasks measuring attention, memory, and prospective cognition, with both threat-related and depressogenic stimulus materials. The results did reveal some relative specificity of processing in that the anxious participants exhibited a greater selective attentional bias for threat relative to depressogenic material with no such difference being apparent in the depressed sample. However, this bias was only clear-cut on a dot-probe measure of attentional processing and not on a modified Stroop measure, and indeed threat-related bias on the 2 tasks was uncorrelated. On the prospective cognition task, anxious participants exhibited an other-referent bias in their risk estimations regarding future negative events that was absent in the depressed sample. No specificity effects were evident on the memory task. The results are discussed in terms of the strengths and weaknesses of carrying out direct comparisons across groups and tasks versus drawing conclusions from overall patterns across multiple studies.

One of the stronger claims of the cognitive approach to understanding and treating emotional disorders has been the notion that such psychopathology is characterized by biased or distorted processing of emotional information in ways that serve to maintain the disorder in question (e.g., Beck, Emery, & Greenberg, 1985; Beck, Rush, Shaw, & Emery, 1979). So, in the prototypical case of depression, the suggestion is that individuals experiencing depressed mood will exhibit processing preferences to perceive, attend to, remember, and think about negative (depression-related) information as compared to other types of material. Such biased processing makes it likely that the depressed mood will be exacerbated. Consequently, the mood-related biases will themselves be further enhanced and so on, in a vicious circle.

Research examining adult populations with emotional disorders has generated considerable support for such theoretical claims (see Williams, Watts, MacLeod, & Mathews, 1997, for a review) across a number

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of cognitive domains using paradigms drawn from mainstream cognitive psychology. However, this body of research on adult samples has also raised some interesting issues concerning the specificity of processing biases. There are two types of specificity that have come under scrutiny: first, the possibility that biases associated with emotional disorders are found in some cognitive domains but not others and, indeed, that different disorders are uniquely associated with biases in different cognitive domains; and second, whether biases associated with particular types of affective material (e.g., depression-related, threat-related) are specific to some disorders but not others (the content-specificity hypothesis; e.g., Beck et al., 1979).

Again, within the adult literature there has been broad support for both of these types of specificity. Anxiety disorders have generally been found to be associated with biases for emotional material on tasks tapping attentional processing but not on tasks measuring mnemonic processing, whereas the reverse has generally been found to be the case for depressive disorders. Both types of psychopathology, on the other hand, have been found to be associated with biases in other cognitive domains such as judgment, thinking, and the interpretation of ambiguous information. Furthermore, biased processing in both types of psychopathology has been found to be stronger for pathology-congruent material. That is, threat- and danger-related material in anxiety- and depression-related material in depression (see Williams et al., 1997, for a review of these bodies of literature).

This research program examining profiles of cognitive bias in adult clinical populations is well established and has a considerable pedigree. However, it has only been relatively recently that researchers have turned their attention to the examination of putative cognitive biases in children and adolescents with emotional disorders. For the most part, this research in younger participants has revealed similar profiles of cognitive processing biases to those found in adults, in the domains of memory and attention. So, studies of attention using different experimental paradigms (the emotional Stroop task and the dot-probe task; see the Method section for descriptions) have generally (though not always) revealed biases in favor of threat-related information in subclinically anxious children (Vasey, El-Hag, & Daleiden, 1996), children and adolescents with a diagnosis of generalized anxiety disorder (GAD; Taghavi, Moradi, Neshat-Doost, Yule, & Dalgleish, in press; Taghavi, Neshat-Doost, Moradi, Yule, & Dalgleish, 1999; Vasey, Daleiden, Williams, & Brown, 1995), children with posttraumatic stress disorder (PTSD; Dalgleish, Moradi, Taghavi, Neshat-Doost, & Yule, 2001; Moradi, Taghavi, Neshat-Doost, Yule, & Dalgleish, 1999), children with simple phobias (Kindt, Bierman, & Brosschot, 1997; Kindt & Brosschot, 1999; Martin, Horder, & Jones, 1995), and children whose parents have PTSD (Moradi, Neshat-Doost, Taghavi, Yule, & Dalgleish, 1999). However, similar studies in children with clinical depression or mixed depression-anxiety have generally revealed no evidence of bias for depression-congruent information (Neshat-Doost, Moradi, Taghavi, Yule, & Dalgleish, 1999a; Neshat-Doost, Taghavi, Moradi, Yule, & Dalgleish, 1997; Taghavi et al., 1999). As in the adult literature, the distinction is not completely clear-cut. For example, processing bias for threat as indexed by the modified Stroop paradigm in younger anxious participants is much clearer in studies in which the stimuli are presented in massed format and in which the samples are older than in single-word presentation studies and studies with younger samples (see Vasey & MacLeod, 2001, for a review).

In the memory domain, there has been no evidence to support a memory bias for threat in generally anxious children (Taghavi, 1996), though there is evidence of a weak bias effect in children and adolescents with PTSD in favor of negative information (Moradi, Taghavi, Neshat-Doost, Yule, & Dalgleish, 2000) and conflicting evidence for subclinically anxious children (Bishop, Dalgleish, & Yule, 2002; Daleiden, 1998). In contrast, there is strong evidence of a memory bias for negative information in subclinically and clinically depressed children and adolescents (Bishop et al., 2002; Hammen & Zupan, 1984; Hughes, Worchel, Stanton, Stanton, & Hall, 1990; Neshat-Doost, Taghavi, Moradi, Yule, & Dalgleish, 1998; Zupan, Hammen, & Jaenicke, 1987).

In contrast to the similarity of bias effects across child–adolescent and adult populations in the domains of memory and attention, in the domain of prospective cognition, the profiles of adult and child data across disorders are markedly different. In adult samples, both clinically anxious and clinically depressed participants estimate that future negative events are more likely to happen relative to healthy controls. What is more, there is a self-referent bias, with such negative events being judged as more probable for the self than for a nonspecific other (Butler & Mathews, 1983, 1987). However, in child and adolescent samples, there is no overall elevation in probability estimation for negative events associated with either clinical depression or anxiety (neither GAD nor PTSD). The effect of the reference manipulation is also different, with depressed children judging negative events as equally likely to happen to self and other and with anxious children judging them as more likely to happen to others (Dalgleish et al., 1997, 1998, 2000).

Clarifying the nature of any specificity in the relation between information-processing biases and a given emotional disorder, whether it be in adults or in younger populations, is clearly important not only in terms of understanding the basic cognition–emotion relations associated with different disorders but also in terms of developing cognitive-based treatments targeted at different cognitive operations. To date, the pursuit of such clarity has generally been through inferring profiles of information processing across disorders by conflating findings from different studies, performed with different methodologies, on different groups of participants, and by different research groups. Clearly, there are pluses and minuses associated with such an approach. Any differences between studies and across disorders may reflect more superficial aspects of methodology rather than anything fundamental to the cognitive processing blueprint for a given form of psychopathology. In addition, suppose that on a given task, clinical group A evidenced a weak bias for processing emotional information, whereas in a separate study clinical group B did not exhibit such a bias. It may not be the case that the strength of bias in the two clinical groups is significantly different. It could merely appear to be the case due to the significant effect in one study and the absence of such an effect in another. Without the ability to statistically compare the two findings in a meaningful way, this problem cannot be overcome. However, the strengths of this multistudy approach are that, if a similar profile of findings is generated repeatedly, despite differences in methodology, samples, and so forth, then one can have more confidence that something fundamental about the underlying cognitive blueprint is being described.

An alternative approach to the extraction of processing profiles from multiple studies is to analyze data in which participants from different clinical groups all carried out the same range of cognitive paradigms. This allows direct statistical comparison of the processing profiles of, for instance, anxious participants versus depressed participants on attention tasks versus memory tasks. The advantage of this approach is that it allows one to conclude with more certainty that a particular effect is specific to one clinical group or to one task or to some combination of the two. However, a potential weakness of this approach is that any methodological flaws in the studies or any peculiarities of the samples will have a strong effect on the final patterns of data and limit the generalizability of the findings.

In the final analysis, both approaches—the conflation across multiple studies and the direct statistical comparison across paradigms and samples—are necessary to achieve some form of understanding of the problem space at hand. This study represents the first attempt that we are aware of to use the second of these approaches (direct statistical comparison) in the domain of cognition–emotion processes in younger clinical and healthy populations.

The study involved analyzing data on cognitive processing profiles for both threat- and depression-related information across a range of memory, attention, and prospective cognition tasks for child and adolescent participants with a diagnosis of either major depressive disorder (MDD), GAD, or PTSD, in comparison to healthy controls. To carry out these analyses, some previously published data (Dalgleish et al., 1997, 2000, 2001; Moradi, Taghavi, et al., 1999; Moradi et al., 2000; Neshat-Doost et al., 1999a; Neshat-Doost et al., 1997; Taghavi et al., 1999, in press) and some unpublished data (Moradi, 1996; Neshat- Doost, 1997; Taghavi, 1996) were combined.

When generating hypotheses for these analyses, it is important to bear in mind that the individual profiles of data for clinical groups and experimental paradigms assessed separately are largely a known quantity as they are already published. However, what is unknown is the *relative* nature of the effects across anxious and depressed samples for the different tasks. Consequently, the hypotheses set out in this article are about this comparison only and are derived from the relevant theories. In the case of the prospective cognition task, even the relative nature of the effects is already partially known, and so the hypotheses regarding this paradigm are discussed separately.

Drawing on the theoretical work of Williams et al. (1997) and Beck (Beck et al., 1979; Beck et al., 1985) alluded to earlier, the first hypothesis under investigation in this analyses was that depressed and anxious (GAD and PTSD) groups of children and adolescents would differ in the pattern of cognitive biases that they exhibit across both stimulus content and type of cognitive task (attention tasks vs. memory tasks). Specifically, anxious children (relative to healthy control and depressed children) will show biased processing in favor of threat-related information (relative to depression-related information) on tasks examining attentional (but not on tasks directly examining mnemonic) processes. In contrast, depressed children (relative to healthy control and anxious children) will show a bias in favor of depression-relevant information (relative to non–threat-related information) on tasks directly involving mnemonic processes (but not attentional processes). Secondary analyses consider any further specificity that might distinguish the two different anxiety disorders of GAD and PTSD.

As already noted, generating hypotheses regarding relative processing across clinical groups for the prospective cognition paradigm is a somewhat thornier issue because the data that are to be analyzed (and that have been previously published) already include a comparison across generally anxious and depressed samples. Specifically, the extant data reveal that generally anxious children and adolescents judge negative events as more likely to happen to others than to themselves, whereas this effect is absent in depressed children and adolescents. It is important to note that this pattern is in contrast to the hypotheses generated by the cognition–emotion theories relevant to this domain, which would predict a self-referent bias for both clinical groups. It seems somewhat misleading to ignore the existing data when generating hypotheses, and so it is perhaps useful to restrict consideration to only the hypothesis regarding the new analyses involving the PTSD sample and to make that hypothesis data driven rather than theoretically driven. The data-driven hypothesis for the prospective cognition data would therefore be that the extant pattern of findings in the literature would hold up when PTSD participants, along with generally anxious participants, are included in the analysis.

Method

Participants

There were four groups of participants: depressed, generally anxious, PTSD, and healthy controls. All participants were between 7 and 18 years old, and their primary language was English. The clinical groups were recruited from a variety of hospital outpatient and inpatient services in the United Kingdom (Maudsley Hospital, Leigh House, the Thelma Golding Centre, Bethlem Hospital, Brixton Child Guidance Clinic, Bloomfield Centre, St. Thomas' Hospital, and Camberwell Child Guidance Clinic). There were 19 participants (10 girls and 9 boys) in the depressed group. The selection criterion was a primary diagnosis of MDD according to the *Diagnostic and Statistical Manual of Mental Disorders* (4th ed. [*DSM–IV*]; American Psychiatric Association, 1994) criteria, in the absence of a comorbid anxiety disorder. There were 24 participants (12 girls and 12 boys) in the PTSD group. The selection criterion was a primary diagnosis of PTSD with no comorbid MDD according to *DSM–IV* criteria. There were 24 participants (11 girls and 13 boys) in the generally anxious group. The selection criterion was a primary diagnosis of GAD according to *DSM–IV* criteria, again with no comorbid MDD.

The diagnoses of the clinical groups were determined in a clinical interview by mental health teams, including psychiatrists and psychologists, before the participants were introduced to the experimenter(s). Children and adolescents and their parents were interviewed to determine diagnoses. Information from the parents was used to ascertain the developmental history of the participants and to corroborate the pattern of presenting symptoms. Consensual diagnosis by all members of the team was a criterion for selection. Diagnosticians had no access to the self-report data prior to diagnosis. Children and adolescents with a diagnosis of mixed depression–anxiety (American Psychiatric Association, 1994) were specifically excluded from the study. The clinical children and adolescents were asked to participate in the study before the commencement of any medication regime. The results of the self-report measures showed that they were in a very negative mood at the time of the study (see the Results section).

The control group consisted of 26 participants (16 girls and 10 boys) with no history of emotional disorder according to parents and teachers. Controls were either recruited from local schools or were the children of staff members. To control for the effects of confounding variables, the control group was selected to be as comparable as possible to the clinical groups for sex, age, vocabulary level, and reading ability (see the Results section).

Informed consent was obtained from the participants or their parents, as appropriate, and the studies and consent procedures were approved by the Ethics Committee of the Institute of Psychiatry, University of London.

Materials and Measures

Depression Self-Rating Scale (DSRS; Birleson, 1981). The DSRS was developed by Birleson (1981) to measure symptoms of depression in childhood. Its 18 items cover the major areas of mood disturbance. The content validity, the internal consistency, and stability of the DSRS have been found to be satisfactory (Birleson, 1981).

Revised Children's Manifest Anxiety Scale (RCMAS; Reynolds & Richmond, 1978). The RCMAS, a revision of the Children's Manifest Anxiety Scale, was designed to assess the presence or absence of a variety of anxiety-related symptoms. The RCMAS consists of 37 items, of which 28 measure anxiety and the remaining 9 comprise a lie scale. Research has shown that the reliability of the RCMAS is high (Reynolds & Richmond; 1978).

British Picture Vocabulary Scale (BPVS; Dunn, Whetton, & Pintilie, 1982). The BPVS was developed to measure the receptive (hearing) vocabulary for standard English of participants who have grown up in a standard English-speaking environment. The BPVS is available in a short form for rapid screening and a long form for more detailed investigation (Dunn et al., 1982). The short form was used here.

Wechsler Objective Reading Dimensions (WORD, Basic Reading; Rust, Golombok, & Trickey, 1993). The Basic Reading test of the WORD is an individually administered scale designed for assessing decoding and word-reading ability of children and adolescents. Data on the internal consistency, test–retest stability, and interscorer reliability of

WORD scores show a consistently high level of accuracy with only a few exceptions. Accumulated data from studies of WORD indicate that the WORD subtests are valid (Rust et al., 1993).

Cognitive Tasks

The dot-probe, Stroop, and memory tasks were presented via an IBM personal computer (Thinkpad 755C TF1) with a 26.41-cm active-matrix TFT color LCD screen. A locally constructed two-switch button-key connected to the computer allowed participants' responses on the recognition task to be recorded by the computer. Stimuli were presented using locally written software.

The attentional probe dot task. Forty-eight emotional words were used in the study: Thirty-two words related to threat, and another 16 were depression-related words (e.g., *sad*). The words were selected from a source of words produced by 231 primary and secondary school pupils suitable for the age range of participants in this study (Neshat-Doost, Moradi, Taghavi, Yule, & Dalgleish, 1999b). Neshat Doost et al. developed a questionnaire that consisted of 10 questions focusing on 3 emotional categories (positive, depression, and threat), 2 types of self-descriptive adjectives (positive and negative), and 2 neutral categories (semantically related and semantically unrelated). Each emotional word was matched with a neutral word for both length and frequency to make 48 critical word pairs. Another 148 neutral word pairs were chosen from a normative set (Neshat-Doost et al., 1999b), with each pair matched for word length, to act as filler items.

Each word pair was presented for 1500 msec[1] with one word above the other and was separated on the vertical axis by a distance of 3 cm (visual angle less than 2 degrees). The word pairs were presented in random order. The words were presented in black capital letters, 8 mm high. On the 48 critical trials (threat-neutral and depression-neutral word pairs) and on 48 of the filler trials, a dot probe replaced either of the two displayed words (after 1500 msec) and remained on the screen until the participant's response. On the other 100 filler trials, there was no probe and the next word pair followed after a delay of 1000 msec following the offset of the previous word pair. On each critical trial, the threat- or depression-related word could appear with equal probability in either the upper or lower screen position. The probe could follow in either position with equal probability, yielding two independent factors: Threat Position and the position of the subsequent visual probe (Probe Position). The combination of these two factors gives rise to four possible conditions: two probe positions (upper and lower) and two threat positions (upper and lower). For each participant, 12 of the 48 critical trials were allocated to each condition. Each participant was tested individually.

The participants sat in front of the computer screen at a distance of 50 cm in a quiet room to perform the task. Participants were instructed to read aloud the top word of each word pair that appeared on the screen. They were informed that some word pairs would be followed by a small dot and were instructed to respond as quickly as possible to this dot with a button press. There was a short practice session of 12 trials that included 4 probe trials but no emotional words. Afterward, participants were asked if they would like to have more practice. If so, the practice trials were readministered. Participants then began the main experiment, which lasted approximately 15 min. Participants received a break in the middle of the task for 3 min.

Modified Stroop task. Sixty words were used as stimuli in the modified Stroop task. These consisted of 12 words from the same 5 word categories that were used in the memory task (see below): positive, categorized neutral, depression-related, threat-related, and trauma-related. The words were again selected from Neshat-Doost et al. (1999b). All of the five categories were matched for length and frequency according to this source. Again, for the purposes of these analyses, the two sets of threat words were combined. Each word was presented twice, once in each of two colors chosen at random from green, yellow, red, and blue. To carry out the task, participants sat 50 cm from the computer screen. There were 18 practice trials using uncategorized neutral words. One hundred twenty stimulus words were then presented in a new fully random order for each participant. The presentation time for each word was 1.7 sec. The intertrial interval was 2.0 sec. Words were presented one at a time. Participants were asked to ignore the word and say out loud the color into the microphone as quickly as possible.

Memory task. The word stimuli for the memory task consisted of 60 words: 12 threat-related words (e.g., *horror*) 12 depression-related words (e.g., *hopeless*), 12 happy words (e.g., *pleasant*), 12 categorized neutral words (animals; e.g., *alligator*), and 12 trauma-related words (e.g., *accident*). The frequency and the length of the words in each category were matched with the other four categories (Neshat-Doost et al., 1999b). The words were presented in random order on a computer screen. As with the other tasks, for the purposes of these analyses the two sets of threat words were combined. To carry out the task, participants sat

[1]This duration was determined during piloting and is consistent with previous studies on younger populations (Vasey et al., 1995, 1996).

in front of the computer, 50 cm from the screen. Each of the to-be-remembered words was presented in the center of the screen for 7 sec. Participants were asked to repeat each word three times, to think about it, and to think about whether the word made sense to them. Participants were also asked to try to remember each word, because they would be asked to write the words down at the end. When all of the 60 words had been presented, participants were asked to count forward aloud by twos (2, 4, 6, …) for 1.5 min, to control for the effects of recency. Following counting, participants were asked to write down as many words as they could remember for 5 min. Participants were informed that the spelling of the words that they remembered was not important.

The Subjective Probability Questionnaire. This measure consisted of 19 items generated by the authors (Tim Dalgleish and Rachel Canterbury) to reflect common negative situations in the lives of school-age children. All questions asked the participant to estimate the likelihood of a given negative event happening either to the participant (self-referent) or to another child (other-referent). An example of a self-referent item would be "How likely is it that you will have a big argument with your best friend in the next couple of weeks?" and an example of an other-referent item would be "How likely is it that Andy will be very ill and miss a lot of school this year?" The measure comprised 9 self-referent items and 10 other-referent items. With the exception of one of the other-referent social items, all items were matched for content and differed only on reference. Thus, for example, the corresponding self-referent item to the one mentioned earlier was "How likely is it that you will be very ill and miss a lot of school this year?"

Participants rated the likelihood of different events on a 10-cm visual analogue scale anchored with "definitely won't happen" and "definitely will happen." Probability indexes were not listed along the scale. Participants were required to mark the scale to indicate their probability estimate. The 19 items were presented in a fixed, pseudo-random order. The proviso was that events with similar content (that is, differing only with respect to reference) were separated by at least two other items. All items were matched for length and the specificity of the event. One of the 19 test items ("How likely is it that Susan's friends will make fun of her in the next week?") was repeated near the beginning and end of the measure, making 20 items in total. This provided an index of consistency in the participant's responding. Participants were also given two practice items.

Questions were scored by obtaining a measurement in centimeters from the left-hand anchor of the analogue scale. Summary scores were obtained by calculating the means for the self-referent and other-referent sets of questions (so, for the purposes of this study, social and physical threat items were combined to facilitate comparison with the other tasks). Across 80 participants in the original study (Dalgleish et al., 1997), the subjective probability measure had a Cronbach's α of 0.82 and a split-half reliability of 0.83. Cronbach's αs for the self- and other-referent subsets were 0.60 and 0.81, respectively. Correlation between responses for the repeated item was reasonable ($r = .63$, $p < .0001$), indicating a similar response profile at the beginning and end of the measure.

Procedure

The four tasks were presented in a fixed order—memory task, dot-probe task, Stroop task, subjective probability task—over separate testing sessions spanning several days. The memory task was placed first so as to avoid memory for words presented on the other tasks contaminating the recall profile.

Results

Participant Characteristics

Means and standard deviations are shown separately for age, reading, vocabulary, and the measures of psychopathology for the three clinical groups and the control group in Table 1. To clarify differences among the groups, one-way analysis of variance (ANOVA) and post hoc multiple comparison tests ($p < .01$ due to

Table 1. *Means and Standard Deviations for Age, BPVS, WORD, RCMAS, and the DSRS*

	Depressed		Controls		Generally Anxious		PTSD	
	M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Age in years	15.58	1.62	15.15	1.44	13.57	3.18	12.83	2.87
BPVS	96.17	10.66[a]	91.42	18.54	94.50	19.88	98.17	15.16
WORD	101.84	15.46	98.50	14.60	98.71	20.57	100.58	12.35
RCMAS	21.42	3.93	8.12	5.69	17.46	5.61	14.63	8.07
DSRS	23.63	5.85	7.50	3.90	13.92	5.50	13.63	7.20

Note: BPVS = British Picture Vocabulary Scale; WORD = Wechsler Objective Reading Dimensions; RCMAS = Revised Children's Manifest Anxiety Scale; DSRS = Depression Self Rating Questionnaire; PTSD = posttraumatic stress disorder.
[a]One of the depressed participants did not complete the BPVS.

the number of tests) were used. Variance across groups was heterogeneous for age, BPVS scores, and RCMAS scores. Consequently, Games–Howell post hoc tests (suitable for heterogeneous variance) were used for these measures. For the other measures, a Scheffe test was used.

The ANOVAs revealed that there were no differences among the four groups on vocabulary level as indicated by BPVS scores or on reading ability as indicated by WORD scores, *F*s < 1. The groups also did not differ on sex ratio. $\chi^2 = 1.51$, *ns*. However, as expected, the groups did differ on their scores on the RCMAS, $F(3, 92) = 19.39$, $p < .0001$, and on the DSRS, $F(3, 92) = 29.44$, $p < .0001$ (see Table 1). Post hoc tests on the RCMAS ($p < .01$) revealed that the depressed participants scored higher than the PTSD and generally anxious groups, the two anxiety groups did not differ from one another, and the three clinical groups scored higher than the controls. Post hoc analyses of the DSRS ($p < .01$) revealed that the depressed patients scored higher than the two anxious groups, who did not differ from each other. Again, all three clinical groups scored higher than the controls. The groups were also significantly different on age, $F(3, 92) = 6.49$, $p < .01$. Post hoc tests revealed that the PTSD group was significantly younger than the depressed group.

Dot-Probe Performance

To minimize the influence of outlying data points, probe detection latencies less than 100 msec and more than 3 sec were omitted, in line with previous research (Mogg, Mathews, & Eysenck, 1992). To facilitate interpretation of the data, MacLeod and Mathews (1988) provided a formula in which the relation between emotion word position and probe position was simplified to provide a single index of attentional bias by substituting the appropriate detection latencies into an equation:

$$\text{Attentional Bias Score} = [(UP/LE - UP/UE) + (LP/UE - LP/LE)] / 2$$

In this formula, UP/LE corresponds to detection times when the upper area is probed but the emotional word is in the lower area, and so on. This algorithm calculates the mean speed of detection latencies to probes in the same area as the emotional stimuli by subtracting them from equivalent probe detection times when the emotional stimulus is in a different location. A value of zero indicates that the emotional stimulus exerts no differential influence on the detection latencies for probes in either area. To the extent that any participants attended selectively to the area where the emotional stimulus appeared, thus detecting probes disproportionately rapidly in this area, the equation will result in a correspondingly large positive value. To the extent that participants moved attention away from the area where this emotional stimulus appeared, it will result in an appropriately large negative value. Attentional bias scores were derived for depression-, and threat-related words for these analyses. These index scores across the four groups of participants are presented in Table 2.

To test the principle hypothesis of a difference in attentional processing in depressed and anxious children and adolescents, the two anxiety-disorder groups were first combined. A mixed within-between-participants repeated measures ANOVA with one between-participant factor (Group: depressed, anxious, control) and one within-participant factor (Word Type: depressed, threat) revealed no main effects: Group, $F(2, 90) = 1.71$, $p > .18$; Word Type, $F(1, 90) = 1.29$, *ns*. However, there was a significant Group × Word Type interaction, $F(2, 90) = 3.10$, $p < .05$. Follow-up paired-sample *t* tests for each group separately revealed that for both the depressed group and control groups, there was no effect of Word Type, *t*s < 1. However, there was a significant effect of Word Type for the anxious group, $t(47) = 2.83$, $p < .01$, with attentional bias for threat material being significantly greater than attentional bias for depression-related material. Examining the simple main effect of Group for each Word Type revealed no Group effect for either the threat Word Type, $F(2, 92) = 2.21$, $p = .12$, or for the depression Word Type, $F(2, 92) = 1.10$, *ns*.

A follow-up analysis comparing generally anxious participants and PTSD participants was performed to investigate whether the significant effect of Word Type in the combined anxious group was a function of anxiety disorder. An ANOVA revealed a main effect of Word Type reflecting greater attentional bias to threat words relative to depression words, $F(1, 46) = 8.08$, $p < .01$, but no effect of anxiety disorder group and no interaction, lowest $p > .14$.

As already noted, in the case of the dot-probe task, an index score greater than zero indicates a processing bias in favor of the experimental stimulus, whereas an index score of less than zero indicates a bias away from the experimental stimulus. Consequently, if a bias score is significantly different from zero, this suggests an absolute processing bias for the stimulus material in question. To investigate this for the anxious and depressed groups, for threat and depression bias, a series

Table 2. *Mean Indexes of Bias for Threat Words and for Depression Words Across the Four Groups on the Dot Probe Task (msec)*

	Depressed		Controls		Generally Anxious		PTSD	
	M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Threat	–18	104	–7	95	67	150	17	104
Depression	–2	95	5	80	–22	193	–73	165

Note: PTSD = posttraumatic stress disorder.

of one-sample t tests with a test value of zero were carried out. The results revealed a significant bias in the combined anxious group, $t(47) = 2.05$, $p < .05$, in favor of threat-related material, but no other significant effects. One-sample t tests were then carried out for the two anxious groups (PTSD, GAD) separately. The GAD group produced a threat bias score that was significantly greater than zero, indicating an absolute attentional bias for threat, $t(23) = 2.19$, $p < .05$. The PTSD group produced a depression bias score that was significantly less than zero, $t(23) = 2.17$, $p < .05$, indicating an absolute bias away from depressogenic material. No other effects were significant.

Modified Stroop Performance

The mean reaction times to color-name threat- and depression-related words (with the reaction times to neutral words subtracted for comparability with the dot-probe data in which bias toward emotional words is relative to neutral distractors) across the four groups are presented in Table 3.[2]

As with the dot-probe task, to test the principle hypothesis of a putative difference in attentional processing between depressed and anxious children and adolescents, the two anxiety-disorder groups were combined. The planned mixed within-between participants, repeated measures ANOVA with one between-participant factor (Group: depressed, anxious, control) and one within-participant factor (Word Type: depressed, threat) was carried out. Modified Stroop performance for positive words was not included as it was not relevant to the hypothesis. The results revealed a trend toward a main effect of Group, $F(2,80) = 2.60$, $p = .08$, though no effect of Word Type, $F < 1$. There was no significant Group × Word Type interaction, $F < 1$.[3]

Correlation of Dot-Probe and Modified Stroop Performance

A Pearson correlation was carried out across all participants between the indexes of threat bias on the modified Stroop and dot-probe tasks. The results revealed a correlation of almost zero between performance on these two measures, $r(82) = .003$, *ns*.

Table 3. *Mean Indexes of Stroop Performance (msec) for Threat and Depression-Related Words Across the Four Groups*[a]

	Depressed		Controls		Generally Anxious		PTSD	
	M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Threat	18	59	1	39	73	124	25	72
Depression	6	79	6	40	82	193	34	98

Note: PTSD = posttraumatic stress disorder. Two depressed patients, five generally anxious patients, and three PTSD patients did not complete this task.
[a]With reaction times to neutral words subtracted.

Table 4. *Mean Indexes of Memory Bias for Threat and Depression-Related Words Across the Four Groups*[a]

	Depressed		Controls		Generally Anxious		PTSD	
	M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Threat	2.20	1.55	1.76	1.39	2.35	1.27	1.70	1.40
Depression	3.60	1.84	2.92	1.38	2.70	1.72	2.57	1.41

Note: PTSD = posttraumatic stress disorder. Nine depressed patients, one PTSD patient, one generally anxious patient, and one control did not complete this task.
[a]With reaction times to neutral words subtracted.

Memory Performance

The mean numbers of threat- and depression-related words recalled (with the numbers of neutral words subtracted for comparability with the attention tasks) across the four groups are shown in Table 4.[4]

As with the previous tasks, to test the principle hypothesis of a possible difference in mnemonic processing in depressed versus anxious children and adolescents, the two anxiety-disorder groups were combined. A mixed within-between participants, repeated measures ANOVA with one between-participant factor (Group: depressed, anxious, control) and one within-participant factor (Word Type: depressed, threat) was carried out. Again, performance with respect to positive words was not included as it was not relevant to the hypotheses. The results revealed a main effect of Word Type, $F(1, 78) = 4.57$, $p < .05$, though not of Group, $F(2, 78) = 1.69$, $p = .19$. There was no significant Group × Word Type interaction, $F < 1$.[5]

[2]A univariate ANOVA of Stroop performance for the neutral words alone revealed a significant effect of Group, $F(3, 82) = 13.03$, $p < .001$. Post hoc Scheffe tests indicated that the three clinical groups were slower on neutral word color-naming relative to the controls, with the PTSD group also being slower than the generally anxious group (*p*s < .05).

[3]This analysis was repeated with the two anxious groups (PTSD, generally anxious) considered separately. The ANOVA revealed no significant effect main effect of Word Type, $F < 1$, a trend for an effect of Group, $F(3, 79) = 2.71$, $p = .05$, but no Group × Word Type interaction, $F < 1$.

[4]A univariate ANOVA comparing the four groups on memory for neutral words alone revealed no main effect of Group, $F(3, 82) = 1.99$, *ns*.

[5]This analysis was repeated with the two anxious groups (PTSD, generally anxious) considered separately. The ANOVA revealed a trend toward a main effect of Word Type, $F(1, 77) = 3.90$, $p = .05$, no effect of Group, $F(3, 77) = 1.03$, *ns*, and no Group × Word Type interaction, $F < 1$.

The Subjective Probability Task

Mean subjective probability estimates for negative self-referent and other-referent events for the four groups are presented in Table 5. Unlike the attention and memory tasks, differential subjective probability performance for depression-related and threat-related events was not assessed.

The first-pass analysis again involved combining the two anxious groups to see if the previously published findings (Dalgleish et al., 1997) involving the generally anxious versus depressed participants held up when a PTSD group was included in a combined generally anxious/PTSD sample. A mixed, repeated measures ANOVA was performed with one within-participants factor (Reference: self, other) and one between-participants factor (Group: anxious, depressed, control). This examined whether the three groups differed in their overall probability estimates for negative events (a main effect of Group) or in the referential (self vs. other) bias of their probability estimates (a Group × Reference interaction).

The results of the full-factorial ANOVA revealed no main effect of Group, $F < 1$, but a highly significant effect of Reference, $F(1, 84) = 16.84$, $p < .001$. There was also a Group × Reference interaction, $F(2, 84) = 3.52$, $p < .05$. A series of paired-sample *t* tests revealed that for the combined-anxious and control groups, there was a significant other-referent bias with negative events being estimated as more likely to happen to others than to the self, *t*s > 3.9, *p*s < .001. In contrast, the depressed group showed no referential bias, $t(16) = .00$, $p > .99$. This pattern mirrors the published data (Dalgleish et al., 1997) involving only generally anxious and depressed participants. The similarity in processing profiles across the two anxious groups was also evidenced by the fact that a further mixed-model ANOVA directly comparing the two anxious groups revealed no significant Group × Reference interaction, $F < 1$.

Discussion

This study examined profiles of cognitive processing across tasks assessing attention, memory, and prospective cognition in groups of child and adolescent participants with GAD, MDD, or PTSD, relative to the performance of healthy controls. The hypothesis relating to the attention and memory tasks was that anxious children (relative to healthy control and depressed children) would show biased processing in favor of threat-related information (relative to depression-related information) on tasks examining attentional (but not on tasks directly examining mnemonic) processes. In contrast, depressed children (relative to healthy control and anxious children) should show a bias in favor of depression-relevant information (relative to threat-related information) on tasks directly involving mnemonic (but not attentional) processes.

The data that relate to this first hypothesis revealed an interesting pattern of findings. On the dot-probe task, there was a difference between the information-processing profiles across the depressed and anxious groups. Anxious participants did indeed show greater attentional bias for threat-related material compared to depression-related material. Such an effect of stimulus material was not evident in the depressed sample. What is more, the anxious participants did show evidence of an absolute (rather than relative) bias in favor of threat-related processing on the dot-probe task. They showed no absolute bias relating to depressogenic material. This threat-related bias seemed to be a function of the performance of the GAD participants rather than the PTSD pants who evidenced no bias for threat but a bias away from depressogenic material. Finally, the depressed sample evidenced no absolute bias for either stimulus type.

In contrast to the dot-probe results, there was no significant difference in the information-processing profiles of the depressed and anxious groups in terms of their performance on the modified Stroop paradigm. In addition, a correlational analysis examining the relation between the degree of threat-related bias on the dot-probe task and on the modified Stroop task showed a correlation of almost zero. Finally, on the memory task there was again no significant difference between groups in terms of the information-processing profile across the different types of stimulus material.

The second hypothesis was that the analyses in which the anxious sample comprised both generally anxious patients and patients with PTSD should reveal a similar pattern of performance across groups on the prospective cognition task as the published data involving only generally anxious participants. That is, anxious participants would differ from the depressed group in terms of their judgments about the relative likelihood of self-referent and other-referent events. Specifically, anxious children and adolescents would judge negative events as more likely to happen to others than to themselves, whereas this effect would be absent in depressed children and adolescents. The results

Table 5. *Subjective Probability Estimates for Self-Referent (Self) and Other-Referent (Other) Negative Events Across the Four Groups*

	Depressed		Generally Anxious		PTSD		Controls	
	M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Self	5.58	2.07	5.59	1.72	4.45	2.23	4.89	1.27
Other	5.58	1.95	6.64	1.95	5.80	2.23	6.02	1.13

Note: PTSD = posttraumatic stress disorder. Two depressed patients, three PTSD patients, and one generally anxious patient did not complete this task.

supported this hypothesis with no evidence of a difference between GAD and PTSD participants.

Starting with the attentional data, the results of the dot-probe task offer some support for the contentions of Williams et al. (1997) and Beck (e.g., Beck et al., 1985) for specificity of content and process across anxiety and depression and across the semantic content of the stimulus material. The only group to show an absolute positive bias was the anxious group in the case of threat-related material. Furthermore, the extent of this bias in the anxious group was significantly greater than the degree of any depression related bias. However, one wrinkle in the dot-probe data is that the PTSD participants did not exhibit such a bias and indeed seemed to show a significant absolute bias away from depressogenic material. It is unclear how to interpret this effect. Furthermore, the theoretical predictions of Williams et al. and Beck and colleagues did not receive support from the modified Stroop data where there was no reliable difference between groups.

What does this pattern of results tell us about attentional bias for emotional information and emotional disorders in childhood and adolescence? There are a number of points that merit consideration here. The first is that the two tasks ostensibly measuring attention in this study (the dot probe and the modified Stroop) seem unlikely to be tapping identical underlying cognitive processes. Indeed, there is considerable debate in the literature as to whether the modified Stroop task is a measure of attention at all (e.g., Williams, Mathews, & MacLeod, 1996), with explanations in terms of response competition being equally compelling. Similarly, the dot-probe task is itself only a measure of the location of attention at a given point in time, namely, when the probe appears. For example, it could be that *all* participants shift attention toward threat but only in the case of anxious participants does attention *dwell* in that location. This attentional dwelling is then picked up by faster responses to a dot probe occurring some time after the initial onset of the threatening stimulus. One straightforward interpretation of these results would therefore be that some aspects of attention-like processing, as measured by the dot-probe task, are differentially biased in anxiety but other aspects of such processing, as coded by the modified Stroop task, show no such specificity. However, as noted in the Introduction, the modified Stroop task has generally produced inconsistent findings in younger samples, and the version of the task used here (involving single-word presentation) has tended to be insensitive, and so the pattern of data may be due to methodological difficulties relating to the version of the Stroop task used, rather than a function of the two different attentional paradigms tapping different aspects of the attention process. There are a number of other methodological caveats that are also relevant to the data, and these are discussed further below.

The memory task data in this study revealed no reliable differences across either group or stimulus material. At its simplest, this finding does not support the Williams et al. (1997) argument of a differential memory bias in depression. However, one significant caveat merits consideration before generalizing too extensively from this finding. That is the fact that the present memory task did not involve explicitly self-referent material. It may be the case, for example, that memory bias for non-self-referent negative material is not associated with emotional disorder, as there is a selective memory bias for self-referent negative material in depression, as has been found previously in the literature (e.g., Neshat-Doost et al., 1998). Indeed, this view has considerable currency in the literature (Williams et al., 1997). This study would therefore have been improved by the additional presence of a self-referent memory task. In addition, there are methodological issues regarding the power of the memory analyses, and these are considered further in the following.

Finally, the prospective cognition results support the hypothesis generated from the previous literature of a reliable difference across anxious and depressed participants in terms of self-reference effects even with a combined generally and PTSD anxiety group, with secondary analyses revealing no differences between GAD and PTSD participants.

Before considering the pattern of results as a whole, there are a number of methodological issues regarding this study that merit some consideration. The first concerns statistical power. For instance, a more powerful omnibus analysis of the data in which all of the tasks as well as all of the participant groups were combined has not been possible due to problems of statistical power. Such an analysis would have allowed stronger conclusions regarding the relation of relative profiles on one task as compared to another task. However, even those comparisons that have been performed here are likely to suffer from problems of power due to the numbers of factors involved in the analyses and the relatively small sample sizes of the groups, combined with the somewhat subtle nature of some of the cognitive effects under consideration. For instance, the observed power for the overall interaction term in the analysis of the modified Stroop data was only .15 (with an alpha of .05). This power issue means that although the Stroop and memory data can be taken as offering no support for the experimental hypothesis, they should in no way be interpreted as offering support for a null hypothesis of no difference across groups.

The second methodological issue concerns differential diagnosis across groups. Although care has been taken to exclude participants with a mixed diagnosis of depression–anxiety, it remains the case that the majority of the anxious participants are high on measures of self-reported depression and that most of the depressed participants are high on measures of self-reported anxiety. This blurring of the boundaries between the different disorders, at least in terms of symptoms as assessed by self-report questionnaires, has two implications regarding the potential interpretation of the data. The first is that the absence of any group effects for a given task may not mean that "purer" forms of anxiety and depression would not have revealed such group differences. In other words, differences may not have been found in some of these analyses due to symptom overlap between the groups. However, offset against this is the fact that any differences that have been found are therefore in spite of the likelihood that symptom overlap would have diluted any effects. This might therefore provide one with greater confidence in those positive findings that are present. In many ways, such discussions are academic in that presentations of depression in the absence of anxious mood and vice versa are unusual and certainly not representative of the clinical conditions that we are trying to understand. A final rider is that previous research has shown that, in the case of attentional bias at least, the presence of clinically significant depression, even when comorbid with clinical anxiety, still results in no evidence of attentional bias effects, perhaps suggesting that the issue of symptom overlap is not as worrisome as it might first seem (Taghavi et al., 1999).

A third methodological issue concerns the distinction between the different types of stimulus material. Considerable care has been taken to include separable depression-related and threat-related words by asking large groups of children and adolescents to rate the material accordingly. However, these ratings were performed by healthy children in a large group study (Neshat-Doost et al., 1999b), and it may be that the words are conceptualized differently by other children, particularly those suffering from anxiety and depression. Consequently, findings indicating that threat-related and depression-related material do not appear to be processed differently must be interpreted with some caution, as it may be that the material is not categorized in this way by the participants in the study and that material that mapped more closely to their idiosyncratic notions of threat and depression might show more differential effects.

Overall, the data from this study seem to provide reasonable support for the argument that attentional bias for emotional material as measured by the dot-probe task in children and adolescents is (a) specific to generalized anxiety and is not found in depression and (b) is greater with respect to threat-related material relative to depressogenic material. This finding was not found in another attention-related task (the modified Stroop paradigm). Furthermore, there were no significant group effects on a non–self-referent memory recall task. These data also show reliable differences between anxious and depressed participants on a test of prospective cognition.

What does all this suggest for the future of research in this area? The data highlight an advantage of direct statistical comparison across groups in that there was evidence of a stimulus-specific and group-specific processing bias on the dot-probe task. However, the results also indicate some of the perils of this type of analysis. For example, the version of the modified Stroop paradigm employed here has been shown to be relatively insensitive to processing bias effects compared to blocked versions of the task. Consequently, the lack of any group effects in these data may be due to methodological aspects of the paradigm. Conflating across many studies using various versions of the Stroop task would have allowed this problem to be more easily circumvented. It seems that the most pragmatic policy is to continue research on both fronts—direct statistical comparison as well as careful review of the stronger themes in the extant literature (e.g., Vasey & MacLeod, 2001). At this stage, it would also seem premature to reject the dominant adult theories of cognition–emotion processes in this area (e.g., Williams et al., 1997) until more data are available. In particular, data examining the developmental trends of these effects have so far been lacking and are therefore something of a priority for future research.

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