



A systematic meta-analysis of the Stroop task in depression

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ARTICLE INFO

Article history:

Received 28 June 2011

Received in revised form 6 February 2012

Accepted 17 February 2012

Available online 28 February 2012

Keywords:

Depression

Attentional bias

Stroop

Information processing

ABSTRACT

Despite the extensive use of the Emotional Stroop task in depression, only qualitative reviews have been produced to date, and these reviews conclude that Stroop performance in depression is equivocal. The present meta-analysis addressed the need to summarize the data quantitatively. A thorough search of the literature was conducted and 47 published studies and unpublished doctoral dissertations were included in the analyses. The meta-analysis revealed large and robust depression-related Stroop effects (e.g., for clinically depressed versus control participants on negative stimuli, $g = .98$, and on positive stimuli, $g = .87$). Although the effects did not reflect a strong emotion-congruent bias, they did distinguish among levels of depressive experience, in that greater levels of depression severity were associated with larger between-groups effect sizes. Moreover, these effects have been obtained without priming procedures, or the presentation of self-relevant or disorder-congruent stimuli. These findings challenge schema-based theories of the Emotional Stroop effect and predictions based on previous qualitative reviews of the literature. The findings also suggest that further comparative behavioural research on the depression-related Stroop effect, at least among clinically depressed populations, is not necessary. Future research should address questions about underlying mechanisms and focus on a more direct measure of depression-related attentional bias.

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1. Introduction

The Stroop task (Stroop, 1935) has received considerable research attention over the last seventy years. MacLeod (1991) estimated that there were more than 700 Stroop-related articles in the literature, and the numbers have increased since that time. Continued interest in the Stroop may be attributable to the widening breadth of applications of the task, such as research on cognitive models of psychopathology. Meta-analytic reviews of Stroop performance can now be found for such diverse groups as individuals with eating disorders (Dobson & Dozois, 2004), addiction problems (Cox, Fadardi, & Pothos, 2006), difficulty with anxiety (Bar-Haim, Lamy, Pergamin, Bakermans-Kranenburg, & Ijzendoorn, 2007), attention-deficit hyperactivity disorder (Lansbergen, Kenemans, & van Engeland, 2007) and schizophrenia (Szöke et al., 2008). Despite the extensive use of the Stroop task in depression, however, only qualitative reviews have been produced to date (Gotlib, Roberts, & Gilboa, 1996; Mogg & Bradley, 2005; Williams, Mathews, & MacLeod, 1996). These reviews conclude that Stroop performance in depression is equivocal. Thus, it is timely to conduct a meta-analytic review to ascertain the state of the science.

2. The Stroop task in depression

The original, or Classic Stroop task (Stroop, 1935) involves the presentation of color words, in incongruously colored ink (e.g., the word 'red' printed in blue ink). Participants are asked to name, as quickly as possible, the ink color of each stimulus word, while attempting to ignore the meaning of the word. This attempt to suppress word meaning in order to name ink color has reliably been shown to result in longer response latencies than those that result from color naming congruent stimuli (e.g., the word 'red' printed in red ink), a phenomenon that has been referred to as the Stroop effect (MacLeod, 1991). The dependent variable in Stroop research is either response latency, which is the time in milliseconds between the onset of the stimulus and the participant's response, or an interference index usually measured as the difference in response latencies between incongruent and congruent stimuli, or between incongruent and colored non-lexical stimuli. The Stroop task is viewed as a useful tool in psychopathology research, to examine the cognitive processes of the disorder under investigation (Williams et al., 1996). Specifically, it is presumed that disordered individuals are sensitive to and preoccupied by stimuli related to their concern (Williams et al., 1996), and the Stroop task is used to determine the existence and nature of this cognitive bias.

The original Stroop task has been modified into an "Emotional" or "Modified Stroop" for research in psychopathology, by changing the content of the word stimuli from colors (e.g., red, blue, green) to affectively-laden themes related to the disorder under investigation. For example, words such as sad, down, and tired are employed in Stroop research in depression (MacLeod, 1991). The relative response latency associated with the delayed naming of disorder-related words, compared with neutral words, has been assumed to reflect an *attentional bias* for the disorder-related stimuli (Gotlib et al., 1996). Similarly, slower Stroop performance among disordered participants compared with controls has been assumed to reflect a disorder-specific attentional bias.

The importance of examining the nature of an attentional bias in depression is evident, given that a mood-congruent attentional bias has been postulated to play a role in the etiology and maintenance of the disorder (Dalgleish & Watts, 1990; Gotlib et al., 1996; Mogg & Bradley, 2005). Stroop studies assess the disruptive impact of depression on Stroop performance, as an index of the disruptive impact of attention allocation to negative thought patterns, while performing task-related activities (Segal, 1996). Such studies typically involve a comparison between a depressed and a control sample on depression-specific, negative, positive, and neutral stimuli. The results of these studies are often

interpreted in light of Beck's cognitive theory of depression (Beck, Rush, Shaw, & Emery, 1979), and Bower's (1981) network theory of emotion (for the description of a similar model, see Lang, 1979).

Beck's model proposes that early life experiences form the basis for interconnected negative schemas about the self, the world, and the future which bias information processing (Dozois & Dobson, 2001). Schemas are viewed as relatively stable and enduring cognitive templates that store, organize, integrate, and direct the processing of information (Beck et al., 1979; Dozois & Beck, 2008; Segal & Swallow, 1994). Individuals who suffer from depression tend to interpret situations in a negatively distorted manner, in line with their underlying negative schemas (Beck et al., 1979). Bower's (1981) work adds to Beck's conceptualization of depression by demonstrating the powerful association between emotion and cognition. His associative network theory states that one's current emotional state influences associative processes, the interpretation of ambiguous situations, and the salience of congruent emotional material. The theory further predicts that emotion enhances the salience of mood-congruent material for selective attention and learning. Reciprocal associations between mood and thoughts occur due to activation of one or the other, through associative linkages. Thus, individuals are predicted to actively attend to material that is consistent with their feelings, and mood-congruent stimuli should be more salient than content that is not mood-congruent (see also Clark, Beck, & Alford, 1999). Longer reaction times to negative stimuli are thought to reflect the greater effort required to suppress the meaning of those highly accessible schema-congruent stimuli (Segal & Swallow, 1994).

Although Beck's and Bower's models provide a framework for how the Emotional Stroop effect should manifest with depressed populations, they do not explain the underlying cognitive mechanisms that produce the Stroop effect. Several information-processing theories have been proposed to explain these mechanisms. The differences between the theories relate primarily to whether interference occurs at an early encoding stage or at a later response stage of information processing. While debate about the most appropriate model of the Stroop effect continues, the most accepted model to date is the Parallel Distributed Process framework (PDP; Cohen, Dunbar, & McClelland, 1990). The PDP framework offers an intermediary explanation of the Stroop effect that encompasses both early and late phases of processing.

The PDP framework posits that there is a cognitive pathway for color naming and another for lexical understanding (Cohen et al., 1990). The two pathways each consist of input units (representing color or words), intermediate units, and output units (representing response to name the color or read the word). The pathways are interconnected by these units at multiple levels, and thus the action of one may be disrupted or facilitated by the action of the other at any point, after sensory perception. According to Cohen et al. (1990), attention modulates the system, as it alters the responsiveness of the units according to whether the task demands that the color be named or the word read. Speed and accuracy of a response depend on the strength of processing of each pathway, influenced by the adjacent pathway, and modulated by task demand. Williams et al. (1996) reviewed the evidence for this model in the anxiety and depression literature, and concluded that this model can serve as a heuristic framework for modeling the attentional bias associated with emotion, and Matthews and Harley (1996) applied the paradigm toward understanding the effects of depression and anxiety on Emotional Stroop performance.

3. Evidence for a depression-related Stroop effect

Beck's and Bower's models predict that depression should be associated with an attentional bias for mood-congruent stimuli. With the Stroop task, this prediction suggests that there should be mood-congruent interference effects for depressed populations, compared with controls. On the other hand, several researchers have argued

that depression is associated with biases in controlled or effortful processing in processes such as interpretation and memory, and not with early or relatively automatic processes such as attention (Frewen & Dozois, 2005; Hartlage, Alloy, Vázquez, & Dykman, 1993; Mathews & MacLeod, 1994; Williams, Watts, MacLeod, & Mathews, 1988; Williams, Watts, MacLeod, & Mathews, 1997). Such arguments derive from studies of the differences between anxious and depressed populations on Stroop and memory task performance. A common finding in this body of research is that anxious populations exhibit strong anxiety-related Stroop effects and inconsistent memory biases, whereas depressed populations exhibit inconsistent depression-related Stroop effects, and strong memory biases (e.g., Dalglish & Watts, 1990; Mathews & MacLeod, 1994; Mineka, Watson, & Clark, 1998; Mogg & Bradley, 2005).

Qualitative reviews of Stroop performance confirm inconsistent reports of depression-related Stroop effects (Gotlib et al., 1996; Mogg & Bradley, 2005; Williams et al., 1996). In light of the debate around whether depression is associated with an attentional bias, and the equivocal results found for Stroop task performance, it has been suggested that if found, depression-related Stroop effects are more likely to occur under certain conditions: a) when negative semantic primes are presented prior to stimulus presentation in order to activate negative schemas (see Mogg & Bradley, 2005; Segal, 1996; Segal & Swallow, 1994), b) when stimuli are presented for longer periods of time to allow for greater elaborative processing (see Mogg & Bradley, 2005) and/or c) when self-relevant as opposed to general emotional stimuli are presented (see Dalglish & Watts, 1990; Mathews & MacLeod, 1994; Mogg & Bradley, 2005; Williams et al., 1996).

The literature has converged on predictions about the above aspects of depression-related Stroop performance. However, the impact of population-related variables such as depression severity, age, and gender remains questionable due to a lack of consensus or evidence. First, the relationship between depression severity and Stroop performance is unclear given inconsistent findings regarding whether state or trait emotion elicits greater Stroop effects (Gotlib et al., 1996; Williams et al., 1996). Second, age-related differences on the Emotional Stroop task have not been systematically assessed, although MacLeod (1991) determined that interference effects on the Classic Stroop task begin in early childhood, peak around the age of seven or eight, decline throughout adulthood, and begin to increase again after the age of 60. Third, although there is no evidence in the literature to indicate gender differences on the Classic Stroop task (MacLeod, 1991), women are disproportionately represented in the depression literature, and thus it is worthwhile to examine whether gender moderates effect size on the Emotional Stroop task.

4. Meta-analysis as a statistical tool

Research results are known to be inconsistent (Schwarzer, 1991), at least partly due to variability in methodologies across studies. Meta-analysis is a statistical technique which aggregates the summary statistics from a number of studies, to draw overall conclusions on the data from a broad literature. The statistical value reported in meta-analysis is the effect size (ES), defined as the standardized mean difference between a criterion group and a comparison group on an outcome variable (Schwarzer, 1991), and divided by an estimate of sample variability (either the comparison group's standard deviation, or a pooled estimate of population deviation). A combined effect size, computed as the average of effect sizes from a series of studies, provides an estimate of the most representative relationship between the groups being compared (Schwarzer, 1991). Meta-analysis takes into account the sample size and the magnitude of the effect size for the comparisons in each study (Rosenthal, 1998). While narrative reviews are potentially subjective and inefficient (Schwarzer, 1991),

meta-analysis provides an estimate of overall effects that may better reflect population parameters (Kazdin, 2003).

5. Predictions and queries

Given the mixed pattern of results demonstrated using the Stroop task and debate in the literature, the objective of the current meta-analysis was to address the following predictions: 1) Overall, weak effect sizes were expected for depression-related Stroop effects, given the inconsistencies in the literature. 2) Despite uncertainty in the empirical literature, greater Stroop effects were predicted for clinically depressed groups than for dysphoric or mood induced groups. According to Beck's and Bower's models, greater depression severity is predicted to result in larger Stroop effects, due to greater accessibility of negative schemas. 3) Depressed groups were expected to demonstrate greater Stroop effects for mood-congruent stimuli, than controls, in accordance with Beck's and Bower's models. 4) It was hypothesized that Stroop effects would be greater among depressed groups for primed, self-relevant, and depression-specific words. 5) Longer stimulus presentations were predicted to result in greater depression-related effects than shorter stimulus presentations. In addition to these predictions, we examined the impact of gender and age on Emotional Stroop performance.

6. Method

6.1. Literature search

Studies were collected through a search of the online bibliographic search engines PsycINFO and PubMed, using the keywords: 'depression', 'Stroop', 'attention', 'information processing', 'attentional bias', and 'cognitive interference'. The search was restricted to English language articles, published electronically or in hard copy, up to and including 15 July 2010. Unpublished doctoral dissertations were also collected. Using a process of tracking back articles, relevant citations from obtained studies and review articles were also pursued in order to ensure a comprehensive search of the literature base.

6.2. Inclusion criteria

The following inclusion criteria were applied in the selection of studies:

1. The study employed a Classic and/or Emotional Stroop task. The primary interest of the present study was Emotional Stroop task performance. However, given the wealth of literature indicating the reliability of its effects (MacLeod, 1991), the Classic Stroop task was included to provide a benchmark for Emotional Stroop performance. Some Emotional Stroop studies included incomparable, variable stimuli, unrelated to depression (e.g., anger, mania, anxiety). In addition, some Classic Stroop studies did not include incongruent color stimuli, without which it is not possible to make statements about interference. Thus, Emotional Stroop studies were only included if they incorporated stimuli with negative valence and Classic Stroop studies were only included if they incorporated incongruent color stimuli.
2. The study contained participants who could be categorized into one of the following groups: clinically depressed, dysphoric, or negatively mood induced. Several studies included mixed depressed samples with a variety of diagnoses of depression, thus it was not possible to analyze Stroop performance by specific depression diagnosis. Depressed samples with comorbid diagnoses of anxiety were included given the high rate of comorbidity between depression and anxiety, and the chance that many depressed participants would have comorbid anxiety, whether or not it was assessed. Depressed samples with other psychiatric diagnoses

(e.g., schizophrenia) were excluded. Samples with primary health concerns were included if depression was a primary comorbid diagnosis.

3. The study included participants from any age group, and employed any stimulus presentation format, e.g.: card or computer administration, supraliminal unmasked or subliminal masked presentation, word and pictorial stimuli, and primed and unprimed stimuli.
4. The study included a participant control group for between-groups comparisons and/or a stimulus control group for within-groups comparisons.
5. The study included Stroop task performance data prior to treatment, if a treatment was administered as part of the study, because post-treatment comparisons of Stroop performance could be confounded by differing treatment outcomes across studies.
6. The study reported response latency data including sample size of the comparison groups and either means and standard deviations, *t* test, *F* test, or exact significance (*p* values).
7. The study contained participant groups and/or stimuli that met the above criteria and were comparable with at least two other participant groups or sets of stimuli, because a minimum of three comparisons were required to calculate an effect size.

6.3. Data collection and coding

The following information was systematically recorded for each study, to the extent that the study provided this information: type of Stroop task (Emotional, Classic), composition of participant groups (number of participants, gender distribution, age), nature of the stimuli (valence, disorder relevance, personal relevance, if primed and nature of the primes), stimuli presentation procedures (study design and length of stimulus exposure), response latency data, depression scores (Beck Depression Inventory or Hamilton Depression Rating Scale), proportion of women per comparison, and publication status. An attempt was made to systematically record anxiety scores but there was insufficient consistency across studies in the use of anxiety measures and reporting of anxiety data, to be able to use the data for analysis. The proportion of women per comparison was calculated by adding the number of women in the two groups being compared, and dividing this number by the addition of the total number of participants in each group. Study design refers to whether the stimuli were presented randomly, such as in computer presentations where response latency is measured in milliseconds per stimulus, or in a

block design, such as in card presentations where response latency is measured in seconds as the time taken to name all of the colors in each condition. Length of stimulus exposure refers to whether stimuli were presented for long enough to be perceived consciously (i.e., supraliminal: typically 70 ms or more) or for such a short duration that they could only be perceived subconsciously (i.e., subliminal: typically less than 70 ms). Depression scores and the proportion of women per comparison were coded as continuous moderator variables. Age, stimuli relevance, publication status, length of stimulus exposure, and study design were coded as categorical moderator variables. In order to circumvent the need to eliminate studies due to inconsistencies in method of reporting age, age was coded by general category instead of as a continuous variable. Table 1 provides the coding scheme of multi-level variables.

6.4. Analyses

Comprehensive Meta-Analysis software, Version 2.002 (CMA; Biostat, Englewood, NJ) was used to perform all analyses. While estimates of ES such as Cohen's (1977) *d* provide a measure of the deviance of the criterion group from the comparison group (Dobson & Dozois, 2004), ESs that employ a weighted estimate of the population standard deviation are more highly recommended, due to their use of a more stable estimate of population variability (Kazdin & Bass, 1989). Hedges *g* is one such form of ES computation, and was utilized in the present study. The cutoff criteria proposed by Cohen (1977) for the identification of small, medium, and large ES are .20, .50, and .80, respectively, and these cutoffs can be applied to Hedges *g*. The 95% confidence intervals for each ES were reported to indicate the range within which an ES would fall if repeatedly calculated. A random effects, rather than a fixed effects model, was used for the present analyses. A random effects model assumes that the data were drawn from populations that differ from each other in ways that could affect ES, and thus accounts for both within-study error and for true between-study differences (Borenstein, Hedges, Higgins, & Rothstein, 2005).

Two sets of primary analyses were conducted. The between-groups analyses consisted of comparisons between the depressed and control groups on negative, positive, neutral, and Classic Stroop (incongruent color) stimuli. In order to minimize the number of comparisons within a study and the associated ES bias, in the case of a study with multiple depressed and control groups, each depressed group was only compared with its matched control. The within-

Table 1
Coding scheme for multi-level variables.

Variable	Coding	Description
Participant groups	Clinically depressed	Inpatients or outpatients with a current primary diagnosis of unipolar depression, or research participants who met Structured Clinical Interview for Diagnosis criteria for current unipolar depression (including major depressive disorder, major depressive episode, dysthymia, minor depression, and major depression).
	Dysphoric	Participants that were assigned to a dysphoric group based on an elevated score on a depression inventory, above a specified threshold.
	Sad Mood	Participants that underwent a negative (sad) mood induction prior to completing the Stroop task.
	Control	All non-specific control populations: labeled as non-psychiatric, non-depressed, healthy controls, less than cutoff score, etc.
	Control Neutral	Participants that underwent a neutral mood induction prior to completing the Stroop task.
Participant age	Control Positive	Participants that underwent a positive mood induction prior to completing the Stroop task.
	Adult	Ages 18 through 60
	Older adult	Age 60 and older
Publication status	Adult and older adult	Ages 18 and up (including ages over 60)
	Published	Empirical journal article
Stimuli relevance	Unpublished	Doctoral dissertation
	Nonself-relevant	Participants did not select stimuli or rate them as personally relevant
Study design	Self-relevant	Participants selected stimuli themselves or rated them as personally relevant
	Block	Stimuli presented in category blocks and response time recorded per block.
Stimulus exposure	Random	Stimuli presented individually and randomly, response time recorded per stimulus.
	Subliminal	Length of stimulus exposure identified as subliminal (typically less than 70 ms)
	Supraliminal	Length of stimulus exposure identified as supraliminal (typically more than 70 ms)

groups analyses examined differences among the types of Stroop stimuli, within each participant group.

Several secondary analyses were also conducted. A fail-safe n was computed for each ES to estimate the number of non-significant results that would be required to render a significant result non-significant (Schwarzer, 1991). Funnel plot analyses were conducted to detect the potential influence of publication bias. A funnel plot that indicates the possibility of publication bias would show a thicker clustering of studies on one side of the mean than the other, at the bottom of the graph, reflecting the fact that smaller studies are more likely to be published if they have larger than average effects (Borenstein et al., 2005). The Q -statistic was calculated to determine the dispersion among ESs, as a result of true differences in ES among studies (Borenstein et al., 2005; Schwarzer, 1991). I^2 was calculated to determine the magnitude of dispersion among ES for each finding. I^2 values of 25, 50, and 75 are considered to represent low, moderate, and high dispersion, respectively (Borenstein et al., 2005).

Moderator analyses were conducted to determine if differences in study and participant characteristics systematically influenced ES, for those comparisons that were found to have a statistically significant ES, and a statistically significant Q . Studies that did not provide moderator data were excluded from those moderator analyses that required the missing data. An analysis of variance procedure was employed for the categorical moderator variables, using a fully random effects analysis. Specifically, a random effects model was used to combine studies within each subgroup as well as to combine subgroups to yield the overall effect. As such, the study-to-study variance was assumed to be the same for all subgroups and this value was computed within subgroups and then pooled across subgroups.

For the between-groups comparisons, study was employed as the unit of analysis (as with the calculation of Hedges g) except in the instances where subgroup had to be employed as the unit of analysis. For example, subgroup was typically used as the unit of analysis when evaluating the effect of length of stimulus exposure because some studies had both supraliminal and subliminal conditions. Subgroup was used as the unit of analysis for all within-subjects comparisons (as with the calculation of Hedges g). The CMA analysis of variance procedure groups the data according to the selected categorical moderator variable and provides an estimate of ES for each group, as well as a Q -value and a p value to indicate whether the difference between groups was significant. A meta-regression procedure, using the unrestricted maximum likelihood model was employed for the continuous moderator variables. Subgroup was used as the unit of analysis for all meta-regression procedures as that was a requirement of the program.

7. Results

7.1. Description of the data

A search of PsycInfo using the previously described keywords yielded 7151 hits, and a search of PubMed yielded 5570 hits. There was significant overlap across search results. Thus, the most relevant and least overlapping combinations of keywords were further explored both in PsycInfo and PubMed: 'depression + Stroop', 'depression + cognitive interference', and 'depression + attentional bias'. This more refined search yielded a total of 930 abstracts and from those abstracts, 229 articles which potentially provided empirical evidence were obtained for review. The final database consisted of 47 studies that provided the data necessary to conduct the meta-analysis (see references with asterisks). Of these studies, 21% were dissertations, 32% exclusively examined the Classic Stroop task, 51% exclusively examined the Emotional Stroop task, and 17% examined both paradigms. All of the studies included in the analyses involved adult or older adult samples because only one child or adolescent study was found to meet the inclusion criteria for the analyses

(Neshat-Doost, Taghavi, Moradi, Yule, & Dalgleish, 1997). In addition, no priming studies were included in the analyses, as there were too few comparable studies to compute an ES. Studies were excluded from the analyses due to: reporting unusable data such as number of correct responses or interference scores, lacking critical information (e.g., number of participants, standard deviations, or exact p values), lacking comparable studies, failing to report Stroop data, reporting changes in data from pre- to post-treatment only, reporting ANOVA interaction terms without follow-up comparisons, lacking a participant or stimulus control group, lacking procedural information rendering data un-interpretable, performing elaborate transformations rendering the data unusable, and collapsing data across participant groups.

7.2. Between-groups analyses

Each of the three depressed groups (clinically depressed, dysphoric, sad mood induction) was compared with a control group on four types of Stroop stimuli (negative, positive, neutral and color incongruent). A comparison between depression-specific and general negative words did not yield significantly different ESs for any of the participant groups, and so the data for both were combined for the remainder of the analyses. Table 2 provides the number of studies or the number of comparisons (k), number of participants (N), combined ES (g), 95% confidence intervals (CI), Q , I^2 , and fail-safe n , per between-groups comparison. The N per comparison is inflated in some instances due to multiple comparisons within some studies. There were an insufficient number of studies to calculate a combined ES for sad mood induced versus control participants, on the Classic Stroop.

The comparison of clinically depressed and control participants yielded significant large ESs across Stroop stimuli. These data were accompanied by significant Q 's, large I^2 's, and large fail-safe n 's. Results for the comparisons between dysphoric and control participants were also significant across stimuli, with the exception of the Classic Stroop. The ES's for the comparisons between dysphoric and control participants were slightly smaller (small to moderate) than for the clinically depressed versus control participants, yielded smaller fail-safe n 's, and only one significant Q , accompanied by a moderate I^2 (for negative stimuli). Only the ES for negative stimuli was significant for the comparisons between sad mood induced and control participants, and was accompanied by a small fail-safe n and a non-significant Q . Fig. 1 presents a graphic depiction of the effect sizes for the between-groups findings.

7.3. Within-groups analyses

Comparisons were made between the different types of stimuli for each of the three depressed groups and the control group. Table 3 provides the summary information for each within-groups comparison. As with the between-groups comparisons, it should be noted that the N per comparison is inflated in some instances due to multiple comparisons within some studies. There were insufficient studies to calculate a combined ES for sad mood induced participants on color incongruent versus control stimuli.

All comparisons between stimuli were significant for clinically depressed participants, except for the comparison between positive and neutral stimuli. While the two significant ES's within this group were small and were accompanied by non-significant Q values and moderate fail-safe n 's, the incongruent versus control comparison revealed a large ES, a significant Q , a large I^2 , and a large fail-safe n . The comparisons between color incongruent and control stimuli revealed the only significant ESs across the other participant groups, each also accompanied by a significant Q , a large I^2 , and a large fail-safe n .

Each between and within-groups comparison was independently entered into a funnel plot analysis, to examine the issue of publication bias. Several of the between-groups funnel plots indicated a thicker

Table 2

Effect size, homogeneity, and publication bias results for between-groups comparisons.

Comparison	<i>k</i>	<i>N</i>	<i>g</i>	95% CI	<i>Q</i> (df)	<i>I</i> ²	Fail-safe <i>n</i>
<i>Negative content</i>							
Clinically depressed vs. control	19	1078	.98***	.71–1.25	76.75 (18)***	76.55	963
Dysphoric vs. control	7	507	.55**	.23–.86	15.16 (6)*	60.43	44
Sad mood vs. control	4	450	.20*	.02–.38	3.05 (3) ^{ns}	1.76	3
<i>Positive content</i>							
Clinically depressed vs. control	14	825	.87***	.50–1.25	84.78 (13)***	84.67	399
Dysphoric vs. control	4	225	.57***	.27–.88	3.75 (3) ^{ns}	19.95	15
Sad mood vs. control	4	476	.29 ^{ns}	−.09–.66	9.06 (3)*	66.89	3
<i>Neutral content</i>							
Clinically depressed vs. control	17	1169	.81***	.43–1.18	142.88 (16)***	88.80	606
Dysphoric vs. control	5	377	.28*	.04–.52	4.68 (4) ^{ns}	14.53	6
Sad mood vs. control	5	480	.12 ^{ns}	−.06–.30	1.72 (2) ^{ns}	.00	0
<i>Classic Stroop</i>							
Clinically depressed vs. control	14	1065	.86***	.47–1.25	119.15 (14)***	88.25	502
Dysphoric vs. control	2	438	.27 ^{ns}	−.04–.58	1.12 (1) ^{ns}	10.80	–

Note: *** $p \leq .001$, ** $p \leq .01$, * $p \leq .05$, ns = non-significant. Study was used as the unit of analysis, therefore k = number of studies used in the analysis. N = number of participants, g = combined effect size, CI = confidence intervals, Q = significance of between-study dispersion, I^2 = magnitude of between-study dispersion, fail-safe n = number of additional studies with null effects required to render the results non-significant.

clustering of studies above the mean at the bottom of the graph. Such a pattern reflects a publication bias: smaller studies were more likely to be published if they had larger than average effects. Such a pattern would also serve to bias the findings towards smaller ESs as the larger studies with lower effect sizes carry more weight. Among the within-groups funnel plots only one analysis revealed such a pattern. The remainder generally showed that most studies clustered close to the mean and within the funnel perimeters, without indicating bias.

Some of the between- and within-groups funnel plots also revealed studies that fell outside but close to the funnel perimeters. Among those plots, a few had an outlier that fell far beyond the funnel perimeters in the lower right-hand quadrant, indicating a small study with a much larger than average effect size. Egger's regression intercept was calculated for those plots to determine the significance of the publication bias both with the outlier and with the outlier removed from the analysis. There was no discernable pattern in these results; in some instances Egger's regression intercept became more significant upon removal of the outlier, in others it became less significant. This pattern of results, in conjunction with a lack of theoretical rationale for removing the outliers, led us to leave the outliers in the analyses.

7.4. Moderator analyses

Moderator analyses were performed for five between-groups and three within-groups comparisons that revealed both a significant combined ES and a significant Q value. Age was a significant moderator for one of the comparisons, in that larger significant interference effects were found for older adults than younger adults amongst control groups on incongruent versus control stimuli. The moderating effect of stimuli relevance could not be calculated for seven of the eight comparisons, because all stimuli were non-self-relevant. Stimuli relevance was not a significant moderator for the one comparison in which there were both self-relevant and non-self-relevant stimuli. The moderating effect of publication status was not calculated for one comparison because all of the studies involved in the comparison were published, and for the remainder of the comparisons publication status was not significant. The moderating effect of length of stimulus exposure could not be calculated for three of the eight comparisons, because all stimulus exposure was supraliminal. Of the remaining comparisons, length of stimulus exposure was only significant for clinically depressed versus control participants on positive stimuli,

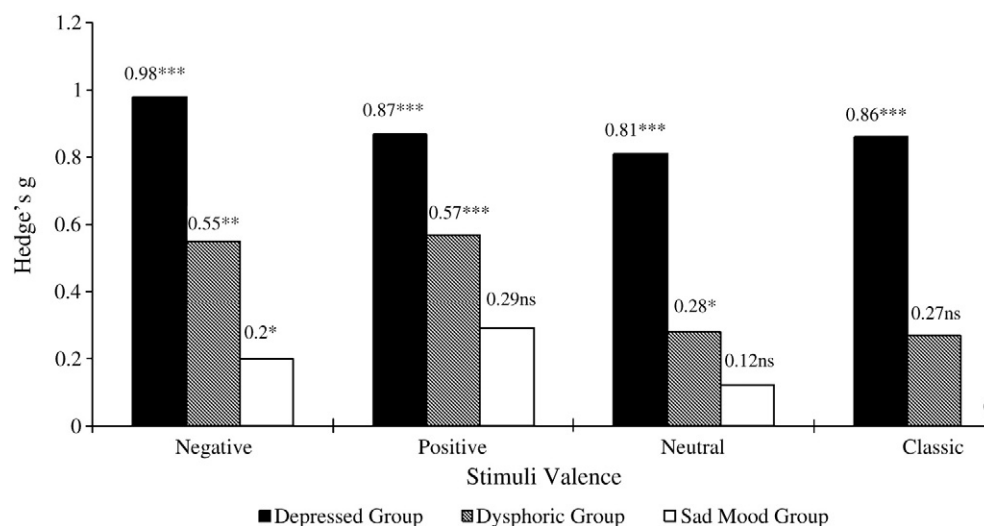


Fig. 1. Effect sizes for between-groups comparisons (all comparisons relative to control groups). *** = $p < .001$, ** = $p < .01$, * = $p < .05$, ns = non-significant.

Table 3
Effect size, homogeneity, and publication bias results for within-groups comparisons.

Comparison	<i>k</i>	<i>N</i>	<i>g</i>	95% CI	<i>Q</i> (df)	<i>I</i> ²	Fail-safe <i>n</i>
<i>Clinically depressed</i>							
Negative vs. neutral	28	519	.25***	.12–.38	29.22 (27) ^{ns}	7.59	80
Negative vs. positive	21	420	.21**	.08–.35	5.92 (20) ^{ns}	.000	30
Positive vs. neutral	16	318	.06	–.10–.21	6.47 (15) ^{ns}	.000	0
Incongruent vs. control	19	638	1.67***	1.18–2.15	237.92 (18)***	92.43	2424
<i>Dysphoric</i>							
Negative vs. neutral	7	196	.16 ^{ns}	–.04–.36	1.02 (6) ^{ns}	.000	0
Negative vs. positive	6	97	.01 ^{ns}	–.27–.29	.95 (5) ^{ns}	.000	0
Positive vs. neutral	3	38	.06 ^{ns}	–.39–.51	.12 (2) ^{ns}	.000	0
Incongruent vs. control	3	56	2.75***	1.50–4.00	10.68 (2)**	81.28	97
<i>Sad mood induction</i>							
Negative vs. neutral	16	365	.09 ^{ns}	–.06–.23	3.97 (15) ^{ns}	.000	0
Negative vs. positive	16	370	.10 ^{ns}	–.05–.24	3.35 (15) ^{ns}	.000	0
Positive vs. neutral	16	380	–.06 ^{ns}	–.20–.09	1.54 (15) ^{ns}	.000	0
<i>Control</i>							
Negative vs. neutral	44	837	.01 ^{ns}	–.08–.12	15.85 (43) ^{ns}	.000	0
Negative vs. positive	35	642	–.03 ^{ns}	–.14–.08	6.24 (34) ^{ns}	.000	0
Positive vs. neutral	33	592	.02 ^{ns}	–.10–.13	3.44 (32) ^{ns}	.000	0
Incongruent vs. control	16	894	1.83***	1.31–2.36	299.22 (16)***	94.65	3414

Note: *** $p \leq .001$, ** $p \leq .01$, ns = non-significant. Subgroup was used as the unit of analysis, therefore *k* = number of comparisons used in the analysis and may be greater than the true number of studies. *N* = number of participants, *g* = combined effect size, *CI* = confidence intervals, *Q* = significance of between-study dispersion, *I*² = magnitude of between-study dispersion, fail-safe *n* = number of additional studies with null effects required to render the results non-significant.

$Q'(1) = 7.75$, $p < .05$. Supraliminal stimulus exposure produced larger effects, $g = .97$ ($p < .001$) than subliminal exposure, $g = -.09$ ($p > .05$).

Study design revealed significant moderating effects on four of the eight ESs. First, among comparisons of clinically depressed versus control participants, study design revealed a significant effect for negative stimuli, $Q'(1) = 8.04$, $p < .01$; studies with a block design yielded larger effects, $g = 1.35$ ($p < .001$), than did studies with a random design, $g = .68$ ($p < .001$). Second, with the same participant comparison, study design revealed a significant effect for positive stimuli, $Q'(1) = 8.14$, $p < .01$; studies with a block design yielded larger effects, $g = 1.30$ ($p < .001$), than studies with a random design, $g = .44$ ($p < .05$). Third, for clinically depressed participants, study design revealed a significant effect for incongruent versus control stimuli, $Q'(1) = 6.52$, $p < .05$; studies with a block design yielded larger effects, $g = 2.12$ ($p < .001$), than studies with a random design, $g = .91$ ($p < .05$). Last, for control participants, study design revealed a significant effect for incongruent versus control stimuli, $Q'(1) = 19.53$, $p < .001$; studies with a block design yielded larger effects, $g = 2.45$ ($p < .001$), than studies with a random design, $g = .87$ ($p < .01$).

For four of the eight comparisons, there was insufficient data to examine the moderating effects of depression severity, as it is not possible to conduct meta-regression with less than three comparisons per analysis. Three of the remaining four comparisons yielded significant moderating effects of HAM-D scores. Specifically, HAM-D scores increased, as *g* increased for clinically depressed versus control participants on negative stimuli, $z = 2.02$, $p < .05$. Similarly, HAM-D scores increased, as *g* increased for clinically depressed versus control participants on incongruent (Classic Stroop) stimuli, $z = 3.95$, $p < .001$. However, for clinically depressed participants on incongruent versus control stimuli, *g* decreased as HAM-D increased, $z = -3.50$, $p < .001$. For one of the eight comparisons, there was insufficient data to examine the moderating effects of Beck Depression Inventory (BDI) scores. Amongst the remainder, only one comparison yielded significant moderating effects of BDI scores; as BDI scores increased, *g* decreased, $z = -2.15$, $p < .05$, for the comparison between clinically depressed and control participants on neutral stimuli.

The proportion of women per comparison (gender) was a significant moderator for one of the eight ES: a) as proportion of women increased, *g* decreased, $z = -2.66$, $p < .01$ for the comparison between

clinically depressed and control participants on neutral stimuli ($g = .64$, $p < .001$). There were an insufficient number of studies to conduct meta-regression for one of the comparisons and gender was found to be non-significant for the remaining comparisons.

8. Discussion

The mixed pattern of results that have been identified for the Stroop task in depression, combined with debate in the literature, motivated the current meta-analysis in order to quantitatively examine these relationships. The results of the current meta-analysis demonstrated large effect sizes for both the Emotional Stroop task and the Classic Stroop task, when clinically depressed individuals were compared to control groups (see Tables 2 and 3 for a summary of the results). Between-groups analyses were affected by both depression severity and to some extent emotion congruence: Stroop performance for the comparisons between clinically depressed individuals and controls resulted in the largest effect sizes, followed by small to medium ESs between dysphoric individuals and controls, and small ESs for those between sad mood induced individuals and controls. The largest between-groups effect sizes for clinically depressed individuals were found for negative stimuli, followed by positive, Classic Stroop, and neutral stimuli. The order of between-groups effect sizes was somewhat different for the dysphoric and sad mood induced groups; positive stimuli demonstrated the largest effects, followed by negative, neutral, and Classic Stroop stimuli.

The measures of robustness for the above results revealed that the largest fail-safe numbers were found for the comparisons between clinically depressed individuals and controls on negative, followed by neutral, Classic Stroop, and positive stimuli. Specifically, the number of studies with null effects required to render the effect size for the comparison insignificant was more than 9 times greater than Rosenthal's (1991) tolerance level for robustness, $5k + 10$ (*k* = the number of studies included) for negative stimuli, more than 6 times greater for both neutral and Classic Stroop stimuli, and more than 4 times greater for positive stimuli. The measures of robustness associated with the results for both dysphoric and sad mood induced individuals demonstrated a different pattern again, with smaller numbers.

The within-groups analyses demonstrated the largest effect sizes for the comparisons between incongruent and control stimuli (Classic Stroop), across groups. Indeed, these interference effects were highly significant and robust (all comparisons exceeded Rosenthal's tolerance level). The only other significant interference effects were for the comparisons between negative and neutral stimuli, and negative and positive stimuli, among clinically depressed participants. These interference effects were small and highly significant, with moderate fail-safe numbers that did not meet Rosenthal's (1991) tolerance level for robustness.

Our moderator analyses yielded largely non-significant and inconsistent findings. Thus, interpretation of these findings is tenuous. Many studies did not measure and/or report the required variables, and therefore had to be removed from the moderator analyses. Thus, the results are even less interpretable because they do not reflect moderation of all of the data that contributed to the original effect sizes. That said, some of the moderator variables and/or analyses deserve comment. First, many studies did not indicate which version of the Beck Depression Inventory they used and so it is possible that the depression severity results were affected by the use of different versions of the measure. Second, age may not have been a significant moderator for most comparisons due to a lack of age variability in the samples. There were insufficient child or adolescent studies to be analyzed, and studies with older adults included samples typically aged 60 or 65 through 85. MacLeod (1991) deduced that performance on the Stroop decreases after the age of 60 and thus with a mixed age sample, the performance of 'younger' older adults would attenuate any age effects that may be present at older ages. Third, similar to stimuli relevance, exposure length only had sufficient studies with both subliminal and supraliminal exposure times to calculate a moderating effect for one comparison. Thus, a thorough assessment of the moderating effects of exposure length was not possible. Last, none of the studies included in the meta-analysis incorporated priming procedures.

The variable that both had sufficient data to calculate its moderating effect, and moderated the greatest number of comparisons was study design. In four of the eight analyses, block design resulted in significantly larger effects than random designs. Other studies have also found that block design yields a significantly larger effect size than random design (Bar-Haim et al., 2007; Williams et al., 1996). While studies that used block design yielded a larger number for average response time per participant group or stimulus condition than studies that reported response times for individual stimuli, the difference between groups or stimuli should have remained constant given an equal number of stimuli per card. The fact that block design produces larger effects is something to consider when designing and evaluating Stroop research.

In sum, the number of significant effects demonstrated in the present meta-analysis settles the debate as to whether there is an Emotional Stroop effect among depressed populations, and the magnitude of the effects contradicts our first prediction of weak effect sizes. Depressed samples do, on average, show longer latencies on the Emotional Stroop task. In line with our second prediction, the hierarchical nature of the effect sizes demonstrates that the Emotional Stroop distinguishes among levels of depressive experience; greater severity is associated with larger effect sizes. Our third prediction was also found to be accurate: depressed groups demonstrated greater Stroop effects for mood-congruent (negative) stimuli than controls. In further support of this prediction, within-groups analyses revealed significant, though small, effect sizes for clinically depressed individuals when comparing negative and neutral stimuli. In contrast, significant within-groups effect sizes were not found for negative versus neutral stimuli among controls.

We were largely unable to address our fourth and fifth hypotheses due to a lack of studies that included the relevant information related to priming, self-relevant stimuli, exact stimulus presentation times,

and subliminal exposures. With regard to our fourth hypothesis, no significant differences in effect size were found between depression-specific and general negative stimuli, and there was not a consistent pattern in terms of which of the two yielded larger effect sizes. However, it should be noted that one study (Pérez, Rivera, Fuster, & Rodríguez, 1999) was an outlier for two of the between-groups comparisons (i.e., clinically depressed versus control on negative stimuli and clinically depressed versus control on positive stimuli) and it contained depression-specific stimuli. With this study removed, g changed from .98 to .86 on the former comparison, and from .87 to .71 on the latter comparison. With regard to our fifth hypothesis, only one comparison yielded a significant moderating effect of stimulus exposure length, where supraliminal exposure times yielded a larger effect size than subliminal exposure times. We did not find a consistent pattern of significant moderating effects for age and gender. Finally, the present meta-analysis appears to demonstrate that the Classic Stroop serves as a good benchmark for comparison with Emotional Stroop performance.

The results of the present study raise some questions about our understanding of how the Emotional Stroop task operates among depressed populations. To start, our findings suggest that the attentional bias in depression is not disorder- or necessarily emotion-congruent. First, no significant differences in interference effects were found between depression-specific and general negative stimuli, and there was not a consistent pattern in terms of which of the two yielded larger effect sizes. Second, between-groups analyses revealed similarly large and significant effect sizes for comparisons between depressed and control groups for both negative and positive stimuli, though the negative stimuli generated slightly larger effect sizes.

Third, non-significant or small effect sizes were found for within-groups comparisons between negative-neutral and negative-positive stimuli among depressed groups. Only the clinically depressed group demonstrated significant within-groups effects which favored negative stimuli over positive and neutral stimuli, and the effect sizes were small. This finding suggests a qualitative difference between clinically depressed and dysphoric or mood induced groups, on the Emotional Stroop. It has been postulated that significant between-groups effects juxtaposed with non-significant within-groups effects, as in the present study for the other groups, indicates that the control participants have a bias away from negative stimuli rather than that depressed participants have an attentional bias toward negative stimuli (Bar-Haim et al., 2007). This postulation would be well-supported if we had also found significant negative effect sizes for the control group on the within-subjects comparisons, but we did not. Last, we were not able to assess whether self-relevance moderated the majority of comparisons because only one comparison included studies that had both self-relevant and non-self-relevant stimuli and it did not yield a significant difference between the two. It should also be noted that although the negative stimuli used in some studies were characterized as interpersonal (Dozois & Dobson, 2001; Hamilton, 2003), autonomous, or sociotropic (Gupta-Rogers, 1999; Hamilton, 2003; Kinderman, 1994; Nunn, Mathews, & Trower, 1997; Shapiro, 2002), which may have increased the relevance of the stimuli, large effects favouring negative stimuli were still not found.

Overall, our results suggest that depressed individuals experience an attentional bias for both negative and positive stimuli, which suggests a general emotional bias on the Stroop, rather than an emotion-congruent, disorder-specific and self-relevant attentional bias. As such, our results challenge traditional schema-based theories of depression. Unless underlying depressogenic schemas (as per Beck's model; Beck et al., 1979) can be interpreted to have become so broad as to be activated by seemingly irrelevant stimuli (i.e., positive and neutral stimuli). The results are more difficult to interpret in light of Bower's (1981) associative linkages model, which predicts longer response times in response to mood-congruent stimuli. Presumably, dramatic fluctuations in mood would not be expected over the course

of completing the Emotional Stroop task, to elicit similar focus on both positive and negative stimuli.

Recent research has attempted to elucidate the cognitive (with neuropsychological tests) and cerebral (with neurophysiological tests) mechanisms that underlie the Emotional Stroop effect and may shed further light on this finding. *Algom, Chajut, and Lev (2004)* conducted a series of experiments with a general undergraduate sample. Their results lead them to the conclusion that the Emotional Stroop effect reflects a generic slowdown of activity in the presence of threat, rather than a selective attention mechanism. In other words, a general-purpose defense mechanism is automatically activated by threat. By this argument then, perhaps all emotion words are perceived as threatening and activate this generic slowdown of activity. Whether or not “attentional bias” is the accurate cognitive mechanism in action during the Emotional Stroop is further discussed below.

Dalgleish (2005) systematically questioned the various aspects of *Algom et al.'s (2004)* framework and emphasized the need for research in cognitive neuroscience to elucidate the cognitive mechanisms underlying proposed theoretical conceptualizations. To date, studies examining the neural basis for response to different emotional words have found discrepant results. In their event-related potential study, *McNeely, Lau, Christensen, and Alain (2008)* found that a sample of individuals with major depressive disorder demonstrated an equivalent neurophysiological response (larger N450 amplitudes over parietal sites) to both negative and positive words compared with neutral words. The authors concluded that this finding reflects deficits in cognitive inhibition rather than selective attention for negative words. On the other hand, in their event-related potential study, *Dai and Feng (2010)* concluded that individuals with major depressive disorder have deficient behavioural and neuropsychological indices of attentional inhibition for negative material only.

Another question raised by the present study is whether general cognitive deficits or a general cognitive slowing among clinically depressed individuals can account for the findings. This question is apparent because of the similarly large interference effects found across types of stimuli for this group compared with controls and the small within-groups effect sizes across stimuli. An alternative explanation for our findings is that there are cognitive deficits among depressed groups, in conjunction with a general emotional bias, which affected their performance across stimuli. This explanation is supported by the fact that clinically depressed individuals, compared with controls, demonstrated large effects for emotional stimuli, followed closely by Classic Stroop and then neutral stimuli. In contrast, while dysphoric individuals demonstrated moderate group effects for the emotional stimuli, they demonstrated small group effects for the Classic Stroop and neutral stimuli. In addition, we found small but significant within-group effects for clinically depressed participants favoring negative stimuli, whereas the dysphoric participants yielded non-significant within-groups effects. This finding suggests that the general emotional bias is stronger among clinically depressed individuals and there is some content-specificity favoring negative stimuli. Thus, our findings suggest a qualitative difference between participant groups; there is a general emotional bias among dysphoric and depressed individuals (stronger among the latter), and in addition there is a cognitive deficit among more severely depressed individuals.

Recent neuropsychological and neurophysiological may shed light on this hypothesized qualitative difference. The research indicates that depressed individuals have specific impairments in executive function, with regard to conflict monitoring (e.g., *Holmes & Pizzagalli, 2008; Holmes & Pizzagalli, 2010*), in other words tasks that involve competing response options such as on the Classic Stroop. Among depressed individuals, these studies have found evidence of structural and functional abnormalities in brain regions hypothesized to be required to successfully complete tasks that involve conflict monitoring. For example, *Holmes and Pizzagalli's (2008)* event-related potential study found

that some individuals with Major Depressive Disorder showed reduced activation in the dorsal anterior cingulate cortex and left dorsolateral prefrontal cortex on incongruent relative to congruent trials on the Classic Stroop task. In other words, perhaps the hypothesized cognitive deficit mentioned above is a deficit in conflict monitoring.

Other research has found evidence for specific patterns of neural activation associated with the Emotional Stroop task in depressed individuals. *Mitterschiffthaler et al.'s (2008)* functional magnetic resonance imaging data revealed a significant engagement of the left rostral anterior cingulate cortex and right precuneus during sad words in individuals with major depressive disorder relative to healthy controls. In fact, compared with patients with major depressive disorder, healthy controls did not have any regions of increased activation. *Herrington et al. (2010)* found that reaction times were similarly longer for pleasant than for unpleasant words for both depressed and control participants. However, functional magnetic resonance imaging (fMRI) data demonstrated increased activation in a region of the dorsolateral prefrontal cortex for pleasant words compared with unpleasant words, and decreased activation for unpleasant words compared with neutral words. These effects were found for both depressed and non-depressed participants. Based on this body of literature, the qualitative difference associated with depression suggested by our results could potentially be explained by impairments in conflict monitoring, in conjunction with a cognitive deficit in emotional processing. Some researchers have argued that this deficit in emotional processing is a deficit of cognitive inhibition (e.g., *McNeely et al., 2008*). Further research is needed to clarify the differential activation of neural regions for emotional stimuli, to further elucidate the generalized nature of the emotional processing deficit as suggested by our findings, and with different levels of depressive severity.

De Raedt and Koster (2010) recently developed a framework for understanding increases in vulnerability to depression that integrates cognitive and neurobiological findings, which also potentially serves to explain the above findings. They proposed that the serotonin metabolism, controlled by the hypothalamic-pituitary-adrenal (HPA) axis, mediates decreased activity in prefrontal areas of the brain, which are associated with an impaired attenuation of subcortical regions. Sustained negative affect thus results, due to an inability to exert attentional inhibitory control over negative elaborative processes such as rumination, which stems from an interaction of reduced prefrontal control and depressogenic schemas. With regard to Emotional Stroop task performance; this model suggests that larger interaction effects are found for negative stimuli due to impaired attentional disengagement from negative information.

For clinically depressed individuals with a dysregulated HPA feedback loop due to chronic negative sustained affect, in combination with increasingly broad depressogenic networks, attentional control would be hindered even when dealing with seemingly less relevant stimuli (i.e., positive and neutral stimuli). Further, considering that the Classic Stroop task is sometimes employed as a stressor in neuropsychological research, and considering the large effects we found on the Classic Stroop task even for controls, perhaps such an emotionally unrelated task was sufficient to activate the HPA axis and hinder attentional control resulting in the large within- and between-groups effects (i.e., cognitive slowing) we found. Among dysphoric individuals, depressogenic schemas would be less elaborate and the HPA feedback loop would be less, if even, dysregulated, accounting for the differences in effects between these individuals and the clinically depressed group.

The difficulty in promoting this model as an effective explanation for our findings, is that the Emotional Stroop has been widely criticized for its interpretational difficulties (e.g., *Algom et al., 2004; de Ruiter & Brosschot, 1994; Isaacowitz, 2007; MacLeod, Mathews, & Tata, 1986*). In fact, *De Raedt and Koster (2010)* did not provide evidence for an attentional bias in depression from Stroop research to support their model, citing its interpretational difficulties. The

argument is that delayed response latencies observed on the Stroop task may be unrelated to attention or early information-processing processes, but rather to late processes (de Ruiter & Brosschot, 1994; MacLeod et al., 1986) such as cognitive avoidance or response inhibition (de Ruiter & Brosschot, 1994). The Parallel Distributed Processing (PDP) model (MacLeod, 1991; Williams et al., 1996), argues that the Stroop task involves both attentional and response processes, and that Stroop effects do not represent a purely attentional bias. Bar-Haim et al. (2007) presented a similar model for their anxiety-related Stroop findings, which they argued “suggest that strong claims that bias in only one stage of processing accounts for the attentional bias in anxiety should be toned down” (p. 17). Their model posits that Stroop effects may be related to preattentive, attentional, and postattentive processes. The present meta-analysis does not indicate whether the obtained depression-related Stroop effects represent an attentional bias or a response bias to emotional stimuli. In order to accurately parse the underlying cognitive mechanisms to specifically determine if there is in fact a depression-related attentional bias, it would be necessary to examine depression-related results obtained from a ‘pure’ measure of attentional bias, as discussed in the future directions section below.

8.1. Study strengths and limitations

The present meta-analysis has several significant strengths. To start, it is the first quantitative review of the depression-related Stroop literature. Previous qualitative reviews came to the conclusion that the depression-related Stroop literature was inconsistent, thus indicating the need to conduct an objective quantitative review such as the present study. The results of the meta-analysis provide strong evidence on the state of the science, and are able to speak to many questions in the field. The scope of the literature review was sufficiently broad to represent the discipline, and the stringent inclusion criteria for the analyses ensured quality of the data. Data collection and entry was carefully conducted, and more conservative analyses were selected, thereby ensuring confidence in the results. Further, a range of outcomes associated with contemporary meta-analysis were considered, to permit the most comprehensive consideration of the results.

Despite the above strengths, potential limitations of the present meta-analysis need discussion. Meta-analysis as a statistical tool has been criticized on several grounds. First, it has been argued that meta-analysis exacerbates the “file drawer problem” due to its frequent reliance on published research. This problem may lead to an overestimation of effect size, due to an over-representation of published studies, which more often have significant effects than unpublished studies (Schwarzer, 1991). The present meta-analysis utilized three methods to address this issue: unpublished dissertations were included, funnel plot analyses were conducted, and the potential moderating effects of publication status were assessed. The funnel plots indicated that publication bias was present for some of the primary analyses. However, when publication status was evaluated as a moderator variable it was not found to significantly moderate any of the comparisons. Review of the effect sizes separated by publication status revealed an inconsistent pattern, where at times published studies revealed larger effect sizes and at others unpublished studies revealed larger effect sizes.

Second, because meta-analysis involves the combination of a large pool of data from varied research, an effect size may be misleading because some of the contributing data may be faulty or weak (Schwarzer, 1991). Similarly, due to the scope required to derive meaningful conclusions, comparisons may be made among studies that differ appreciably in design and method (Schwarzer, 1991). The inclusion and exclusion criteria and the moderator analyses employed in the present meta-analysis represented an effort to balance this potential bias. Most of the significant effect sizes were tempered by

significant Q -values and high I^2 -values. These numbers reveal that there is a significant amount of true variation in effect sizes between studies, which cannot be accounted for by random error. Previous qualitative reviews have commented on the inconsistent findings in the field (Gotlib et al., 1996; Mogg & Bradley, 2005; Williams et al., 1996). The results of the current study reflect the methodological variability in the literature base. Studies varied as to whether and how they transformed their data, in terms of what stimuli they used (which differed in content, number, color, match of conditions, presentation method, and response format), and with respect to their samples (which differed on variables such as severity of depression, diagnosis, level of symptoms, mood induction procedure, age, and gender).

The control groups used in the available research likely influenced effect size estimates (Bar-Haim et al., 2007). Specifically, clinical groups were often matched with non-clinical or ‘healthy’ controls. In contrast, dysphoric individuals were typically matched with comparable but non-dysphoric individuals who did not meet the cutoff criteria on the same measure. Sad mood induced participants were typically matched with similar non-mood induced or neutral mood induced counterparts. The magnitude of any effect size is partially determined by the degree of difference between the two samples employed in the study. Thus, the differences in effect sizes observed here may be due in part to sample differences.

The most common criticism of meta-analysis is that it amalgamates non-independent data, and thereby introduces bias into effect size calculations (Schwarzer, 1991). Non-independent data derive from at least three sources: multiple outcomes from the same studies, comparisons among more than one criterion group and a single control group (Schwarzer, 1991), and multiple publications by the same author with parts of the same data set. The amalgamation of non-independent data inflates effect sizes because sample size is artificially increased in these studies, which are then assigned more weight. Moreover, when multiple studies by the same author are included, potential biases of the author may be transferred into the analyses, through their data, which tend to support the author's theoretical stance (i.e., researcher allegiance). Although the problem of non-independence may be negligible (Schwarzer, 1991), it was addressed here by the use of more conservative statistics (i.e., Hedge's g , random effects and fully random effects models). Furthermore, the number of comparisons per study was reduced by only including matched criterion and control group pairs. Last, data from unpublished studies was not included in the same comparison with data from its published counterpart, if a published counterpart was found to exist.

8.2. Future directions

The current meta-analysis provides sufficiently large and robust effect sizes to argue that if any further comparative research examining behavioural data on the Stroop task is pursued, it need not address questions regarding Stroop performance with emotional stimuli, among clinically depressed populations. The utility of further examining behavioural data for Stroop performance among dysphoric or mood induced populations is also questionable, given moderate group effect sizes. If there is interest in further behavioural research on the Stroop task in depression, it should address remaining gaps in the literature base. For example, the depression-related Stroop performance in child and adolescent populations remains unclear. In addition, future behavioural research could focus on the variables that may moderate depression-related Stroop performance, such as comorbid diagnoses. In particular, while depression and anxiety are highly comorbid, few studies have compared an anxious group with a depressed group on Stroop performance. Moreover, the variability in anxiety measures used in Emotional Stroop research to date precludes the assessment of self-reported anxiety as a potential moderating variable of effect size.

Behavioural research examining Emotional Stroop task performance in individuals who have remitted or recovered from depression also remains unclear. While it has been argued that in the absence of priming, there is no support for differences between remitted depressed individuals and non-depressed controls on measures of depressive cognition (Segal & Ingram, 1994), more recent research has provided some evidence to the contrary. Based on the results of their event-related potential study, Vanderhasselt and De Raedt (2009) concluded that deficits in cognitive control persist for individuals who have remitted from depression and are stronger in those with a greater number of previous episodes. That said, they utilized the Classic Stroop task and only found differences in neural activation between currently depressed, remitted depressed, and never-depressed individuals; there were no differences between groups for the behavioural data. Fritzsche et al. (2010) also found evidence of cognitive bias in remitted depressed individuals. However, although they used the Emotional Stroop task and examined behavioural data, they did not find this evidence based on Stroop task performance. Rather, they found evidence of cognitive bias based on performance data from the self-referential encoding and incidental recall task and the emotion face dot probe task. Based on our review we have established the state of the science with regard to behavioural performance by sad mood induced, dysphoric and clinically depressed individuals. Future research that clarifies Emotional Stroop task performance in remitted depressed individuals could provide data for the full spectrum of depressive experience, which could further clarify the predictive utility of Stroop task performance.

While the present study provides clear and robust behavioural evidence of the existence of Emotional and Classic Stroop interference effects among depressed populations, it does not clarify what cognitive or brain mechanisms generate the effects. As discussed above, it has been proposed that the underlying cognitive deficit measured by the Classic Stroop task involves an executive functioning deficit in conflict monitoring. There is a growing body of literature examining the neural mechanisms that underlie this deficit. In order to better ascertain whether the cognitive deficit that generates performance on the Emotional Stroop task is a generalized deficit, a specific deficit in conflict monitoring or a specific deficit in cognitive inhibition, a task-matching design as per Chapman and Chapman (1973) would be helpful, utilizing either a direct comparison between the Classic and Emotional Stroop paradigms or the Emotional Stroop and another task such as the dot probe. In addition, the data have not yet converged on the brain mechanisms involved and thus further examination of event-related potential data and functional magnetic resonance imaging would be helpful.

However, if the field's interest is in the specific examination of attentional bias rather than Stroop performance *per se*, the dot probe task and eye-tracking methods may provide a more accurate assessment of attentional biases than the Stroop task. In the dot probe task, two words are simultaneously presented at two different locations on a visual display (MacLeod et al., 1986). Immediately following stimulus presentation, a visual probe replaces one of the words. Detection latency is presumed to vary as a function of stimuli relevance and location, and indicates whether visual attention was directed to or away from the stimuli of interest. The dot probe task has received support in the literature as a more accurate measure of attentional bias than the Stroop task (Bar-Haim et al., 2007; de Ruiter & Brosschot, 1994; Mathews & MacLeod, 1994; Mineka & Sutton, 1992; Mineka et al., 1998).

Due to concerns that the dot probe task is also an indirect measure of attentional bias (Eizenman et al., 2003; Isaacowitz, 2007), Eizenman et al. (2003) developed an eye-tracking technology to continuously monitor point-of-gaze. They postulated that attentional bias among depressed populations would be typified by more time spent visually fixating on negative stimuli, and by difficulties in shifting visual gaze away from such stimuli, as compared with normal controls. They found that although depressed individuals spent significantly

more time gazing at dysphoric stimuli, they did not scan dysphoric stimuli more frequently than normal controls. Eizenman et al. (2003) interpreted these findings to mean that depression primarily influences later stages of processing, as opposed to early attentional processes. While the method and associated findings of this study are compelling, more research is required before conclusive statements can be made about the paradigm, or the implications of the resulting data.

9. Conclusion

The present meta-analysis demonstrated that there are large and robust depression-related Stroop interference effects. Although these effects do not reflect an emotion-congruent bias, they do distinguish among levels of depressive experience, where greater severity is associated with larger effect sizes. Moreover, these effects do not require priming procedures, longer stimulus exposure, or the presentation of self-relevant or disorder-congruent stimuli, to be obtained. Heterogeneity within the findings could not be fully accounted for by the moderating effects of gender, age, publication status, study design, or self-reported depression. This finding reflects both inconsistent measurement of these moderators, and heterogeneity among results in the original studies. The meta-analysis also demonstrated that the Classic Stroop serves as a good benchmark for comparison with Emotional Stroop performance. Given these findings, further comparative behavioural research on the Stroop task in depression is not warranted. However, questions remain about the underlying mechanisms of Stroop performance, particularly Emotional Stroop performance, which could be addressed through further neuropsychological and neurophysiological studies and task-matching designs. To investigate depression-related attentional bias, studies with a more direct measure of depression-related attentional bias are recommended.

Sources of Funding

The first author was supported in her doctoral studies by a Doctoral Canada Graduate Scholarship (Social Sciences and Humanities Research Council of Canada) and an Alberta Innovates—Health Solutions Studentship for Mental Health Research. These funding bodies were not involved in any aspect of the study design, in the collection, analysis, and interpretation of data, in the writing of the report or in the decision to submit the paper for publication.

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