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Cognitive Behaviour Therapy for Health Anxiety: A Systematic Review and Meta-Analysis

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Background: Health anxiety (HA), or hypochondriasis, is a psychological problem characterized by a preoccupation with the belief that one is physically unwell. A 2007 Cochrane review (Thomson and Page, 2007) found cognitive behavioural therapy (CBT) to be an effective intervention for individuals with HA. Similar findings were reported in a recent meta-analysis (Olatunji et al., 2014), which did not employ a systematic search strategy. The current review aimed to investigate the efficacy of CBT for HA, and to update the existing reviews. **Method:** A systematic search was conducted following Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidance, including randomized controlled trials that compared CBT with a control condition for people with HA. Five hundred and sixty-seven studies were found in the original search, of which 14 were included in the meta-analysis. **Results:** Meta-analysis was conducted on 21 comparisons and a large effect size for CBT compared with a control condition was found at post therapy $d = 1.01$ (95% confidence interval 0.77–1.25), as well as at 6- and 12-month follow-up. **Conclusions:** This systematic review and meta-analysis provides support for the hypothesis that CBT is an effective intervention for HA when compared with a variety of control conditions, e.g. treatment-as-usual, waiting list, medication, and other psychological therapies.

Key words: Hypochondriasis, health anxiety, cognitive behavioural therapy, systematic review, meta-analysis

Introduction

Hypochondriasis in DSM-IV has been redefined in DSM-5 to Illness Anxiety Disorder (American Psychiatric Association, 2013). By both definitions, this problem is characterized by preoccupation with the belief that one has, or could acquire, a serious illness, emanating from 'anxiety about the meaning, significance or cause' of their symptoms. This is accompanied by high anxiety about health and excessive health-related behaviours or maladaptive avoidance. For some time now, these problems have been referred to as 'health anxiety' (HA), and given the recent publication of DSM-5, this is the term used here.

HA is a common mental health problem; epidemiological studies report rates of 0.26–8.5% of individuals in primary care meeting DSM (Diagnostic and Statistical Manual of Mental Disorders) or ICD (International Classification of Diseases) criteria (Creed and Barsky, 2004). Gureje et al. (1997) found that individuals with abridged, or subclinical, HA had similar levels of impairment in terms of occupational role, physical impairment and health perception to those who met the full ICD-10 criteria. Warwick and Salkovskis (1990) have suggested that HA is best thought of as a continuum, with full clinical diagnosis at the upper end. Treatment

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options exist for those experiencing significant distress as a result of their anxiety, notably those in the new category of somatic symptom disorder. HA is costly due to the over-use of medical health services by individuals with HA (Barsky et al., 2005). There is evidence that rates of HA are higher in individuals with physical health conditions (Robbins and Kirmayer, 1996) and therefore individuals with medical conditions are an important group to target for treatment.

A cognitive behavioural understanding of HA (Warwick and Salkovskis, 1990) has resulted in the development of a focused treatment (Salkovskis et al., 2003) which has been tested in single cases (Salkovskis and Warwick, 1986), case series (Warwick and Marks, 1988), and randomized controlled trials (RCT) (Clark et al., 1998). The cognitive behavioural therapy (CBT) approach to HA involves developing a shared understanding of the problem followed by belief and behaviour change through discussion, socratic questioning and 'behavioural experiments' (Salkovskis et al., 2003). CBT interventions for HA based on these treatment elements have been found to be more effective in RCTs than a stress management package (Clark et al., 1998), waiting-list control (Warwick et al., 1996), paroxetine (Greeven et al., 2007), and treatment as usual (Barsky & Ahern, 2004).

In 2007, a Cochrane review was published of psychological therapies for HA (Thomson and Page, 2007). This found that psychological therapies for HA were more effective than control conditions, with the exception of psycho-education interventions. Whilst a more recent review by Olatunji et al. (2014) noted similar findings, there were a number of methodological concerns – primarily that the search strategy was not systematic or clearly defined. The present systematic review and meta-analysis aimed to update this important field by investigating the efficacy of CBT for clinical and subclinical HA relative to control conditions, focusing on measures of health anxiety, depression and anxiety pre- and post-intervention, and assessing the quality of the RCTs. A second aim was to investigate whether CBT has equivalent effects for subclinical HA compared with clinical HA, and similarly whether effects are different for people with medical illness compared with those without.

Method

Eligibility criteria

Study type. RCTs of CBT for people with HA were selected for this review. The interventions included were problem-specific CBT, cognitive therapy, or behaviour therapy, including psycho-educational approaches using CBT models and strategies delivered 1:1, in groups, or online by trained therapists. Only studies that compared CBT with a non-CBT-based control condition were included. Control conditions included wait-list, treatment as usual (TAU), medication, placebo, other psychological therapies, support groups, and non-CBT psycho-education.

Population. Participants were over the age of 18 years, with hypochondriasis diagnosed according to standardized diagnostic criteria (e.g. DSM-III, DSM-IV, IV-TR, ICD-10), or with subclinical HA measured by a valid HA psychometric measure, not including somatization disorder.

Outcome measurement. The primary outcome measure was HA symptom severity. Assessments of HA had to use valid and reliable questionnaires at pre- and post-therapy.

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Where a post-therapy measure was not available, the next available measure following the end of therapy was used in its place. Six- and 12-month follow-up measures were also extracted where available. Secondary outcome measures of depression and general anxiety for pre- and post-therapy were also included.

Information sources and study selection

Studies were identified through searching the following databases: PsycINFO, PubMed, EBSCO, Embase and Web of Knowledge. The search was conducted on 5 January 2014. A second search, using the same criteria, was conducted by one member of the research team on 6 July 2015 to check for any literature published since the initial search. No new studies meeting the inclusion criteria were identified in the second search.

Search. The search terms used were 'health anxiety' OR hypochondria∗ AND 'cognitive therapy' OR 'behaviour therapy' OR 'behavior therapy' OR 'cognitive behaviour therapy' OR 'cognitive behavior therapy'. These terms were searched in key words, title, abstract, and as MeSH subject heading terms. The search was for studies published between 1979 and 2014, to match the use of DSM-III and DSM-IV diagnostic criteria.

The reference sections of all included papers, as well as three previous reviews and meta-analyses (Thomson and Page, 2007; Bouman, 2014; Olatunji et al., 2014), were scrutinized for any overlooked papers. Emails were sent to experts in the field to search for any unpublished literature.

Study selection. After removing duplicates, two members of the research team individually assessed each of the remaining papers for inclusion eligibility. This was done in two stages, looking first at just the title and abstract, and then at the full text. An *a priori* procedure was followed to resolve any inter-rater discrepancies: in the case of a disagreement about the inclusion of a particular study, both reviewers re-assessed the paper for inclusion. If the reassessment still led to a disagreement between the reviewers, an independent third party was asked to assess the paper, and the decision would be based on the majority decision.

Data items. The following was collected from each included paper by one of the authors: details of CBT treatment delivered (e.g. length, theoretical orientation, mode of delivery, additions to therapy such as psycho-educational material); details of control or second active treatment; participant drop-out; assessment of health anxiety; presence of physical health conditions and socio-demographics of participants. The primary dependent variable was a validated measure of health anxiety, and secondary outcomes were measures of depression and general anxiety. Quality of life was also considered as a secondary outcome, but eventually not included because very few studies identified measured this.

Data extraction. A data extraction spreadsheet was designed for the purpose of this study, piloted on one of the included papers and modified to suit the review questions. This information was extracted from each study by a member of the research team. The data to be meta-analysed was checked by a second member of the team for accuracy. This included the means and standard deviations (SD) for outcome measures of HA pre- and post-treatment, as well as at 6-month and 12-month follow-ups where available, for each group. Where available, the means and SDs for measures of depression and anxiety pre- and post-treatment were also checked. An *a priori* process was followed for this: the completed table was presented to

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the second team member, who highlighted any data points that they disagreed with. The first team member then checked the alleged error, and if they agreed with the second team member, changed the error. In the case that there was still a disagreement, a third team member would be consulted and the majority decision followed.

Quality assessment

Adopting The Cochrane Collaboration's tool for assessing bias, an assessment of study quality was also conducted. Each of the eligible papers was assessed according to seven different domains, which might introduce bias: sequence generation, allocation concealment, blinding of participants and personnel, blinding of outcome assessment, incomplete outcome data, selective outcome reporting and 'other issues' (Higgins et al., 2011). This information was extracted from each included paper by one member of the research team only. An overall rating for the quality of each individual study was calculated by allotting a score of three points for each of the items rated as having a low risk of bias, two for each item rated as having an unclear risk of bias, and one for items rated as having a high risk of bias. A cut-off of the median bias scores was used, and studies that scored above the median score were rated as having an overall lower risk of bias and those below the median were rated as having a higher risk of bias.

Statistical analysis

Standardized mean difference effect sizes were calculated for health anxiety, depression and anxiety outcomes where available for pre-therapy, post-therapy and control conditions. In the absence of an immediate post-therapy measure, the outcome measures taken the soonest following therapy ending were included. Effect sizes for pre-therapy and 6-month and 12-month follow-up HA outcomes were also calculated where possible.

Standardised mean difference. Effect sizes for the difference in outcome between CBT and a control condition, or CBT and a second active therapy or medication, were calculated. The pre- and post-therapy outcome measures which were used to calculate a change score (post- minus pre-therapy score) measured in Cohen's d – the mean change in outcome measure divided by the pooled SD. A Cohen's d of 1 means that the two means differed by 1SD. To aid interpretation of effect sizes, $d = 0.2$ is considered a small effect size, $d = 0.5$ a medium effect size, and $d = 0.8$ a large effect size (Cohen, 1988). Cohen's d was selected over Hedge's g to aid interpretation, as Cohen's d is more widely used.

A random-effects meta-analysis is most appropriate when the studies being combined are not direct replications of one another, and so was used here due to the heterogeneity of studies in terms of types of participants, outcome measures used, and interventions and control conditions provided. This model weights each individual effect size inversely proportionally to the sum of the variance and heterogeneity. The test of heterogeneity used was the Q test (Cochran, 1954), which is the sum of the squared deviations of each study's effect size from the overall effect size, with each included effect size being weighted by its inverse variance.

Subgroup analysis. Subgroup analysis separated studies which included and excluded participants with physical health problems, studies which required a DSM or ICD diagnosis

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of HA compared with those which did not, studies assessed as having low or high risk of bias, and studies with control conditions of either TAU, wait-list or active treatment.

The meta-analysis was conducted for each of these subgroups, and the difference considered significant if the confidence intervals of each analysis did not overlap – a conservative approach that is recommended in the Cochrane handbook (Higgins & Green, 2008).

Bias. Publication bias was assessed using a funnel plot, which plots effect size against standard error (as an index of study size), to check if there is evidence for the 'file drawer problem' – the idea that studies with non-significant results remain unpublished, meaning the literature is biased towards presenting positive results. Other things being equal, as many studies should overestimate the true effect as underestimate it, and the range of over- and underestimates should be related to the standard error of the study. Specifically, if there is no publication bias, then studies with lower error should scatter in a smaller range around the true effect size, whereas studies with higher error (and smaller sample sizes) should have a wider range of under- and overestimates, resulting in a 'funnel'-shaped graph. If the scatter is asymmetrical, this may indicate publication bias. In particular, the classic sign of bias is a relative absence of studies with low effect sizes and high standard error, as small studies that underestimate the utility of the intervention are more likely not to be published.

Results

Study selection

Five hundred and sixty-seven articles were identified through database searches, with a further two studies that were under review identified as a result of emails to experts in the field. No additional studies were identified through the reference lists of included papers. See Fig. 1 for a diagrammatic representation of the search and selection process, based on the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidance (Moher et al., 2009).

Of the 344 studies screened for inclusion, 313 were excluded based on detailed examination of their title and abstract. The Cohen's kappa value, which measures the inter-rater agreement between the two assessors at this stage of screening, was $\kappa = 0.917$ (SE = 0.041), which is a high level of agreement. Thirty-one full-text articles were assessed for their eligibility for inclusion in the meta-analysis, and 14 were included in the final study. The Cohen's kappa coefficient for this assessment was $\kappa = 0.933$ (SE = 0.065), which again is a high level of agreement. The reasons for excluding sixteen papers included: not exclusively recruiting participants with health anxiety; not being research papers; not being randomized; including repeat data or follow-up data from included studies; not having a non-CBT control condition; and not using a validated measure of HA.

Study characteristics

The 14 studies included in the final analysis had a total of 1544 participants. Seven of the studies had more than one control or experimental condition, and so 21 comparisons were included in the meta-analysis. See Table 1 for a summary of participants' demographic information, when this was available. See Table 2 for a summary of the study characteristics.

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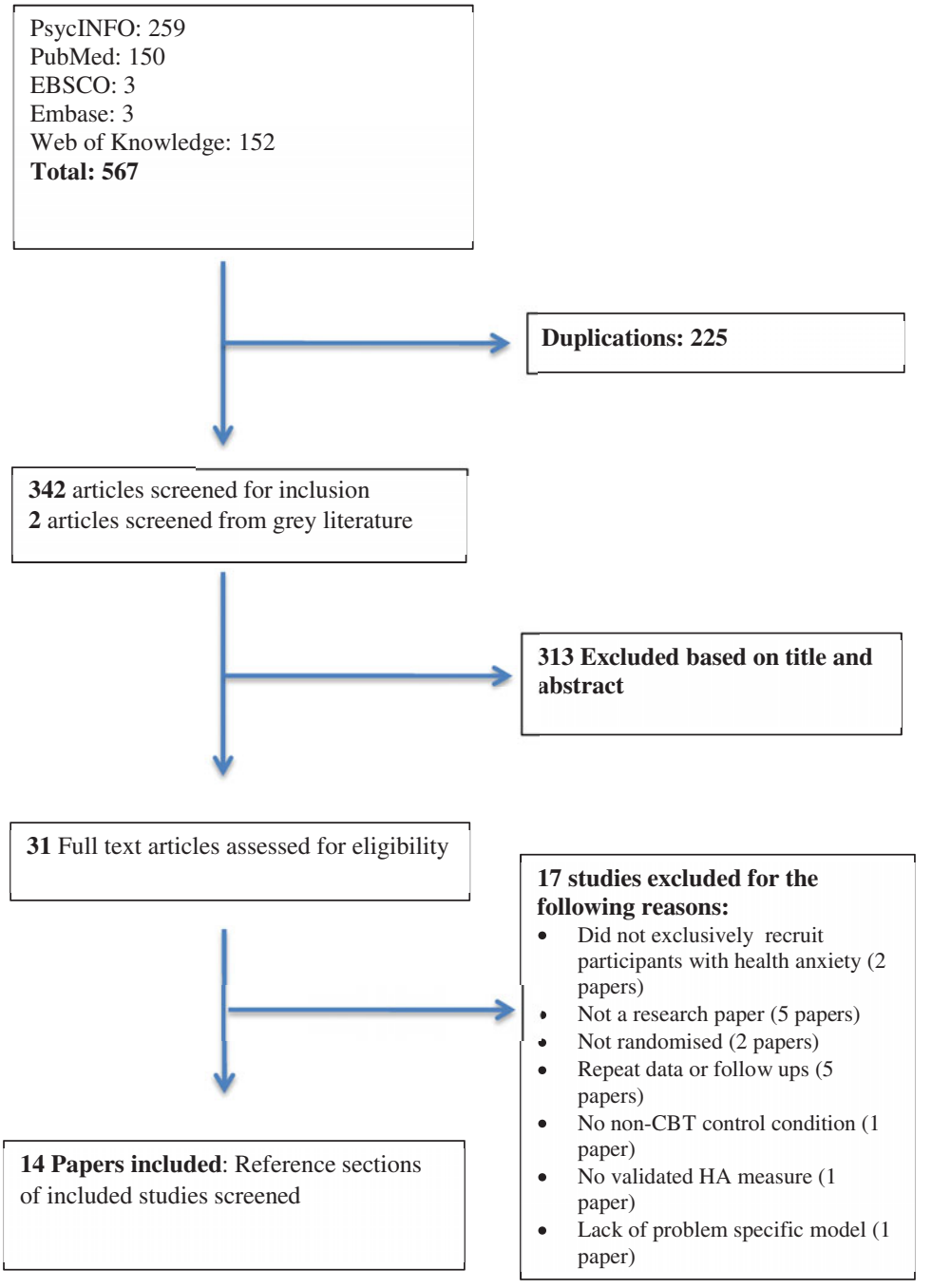

Figure 1. (Colour online) Search process flow chart.

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Table 1. Demographic information for each included study

Study	Duration of HA (years)	Age of onset (years)	Gender ratio (% of female)	Mean age of participant (years)	Marital status (% married)	Employment (% employed)
Barsky and Ahern (2004)	10.8	31.5	76	42.2		65.7
Bouman and Visser (1998)						
Bourgault-Fagnou and Hadjistavropoulos (2013)			77.2	68.7	43.9	
Buwulda et al. (2007)						
Clark et al. (1998)			67	34		
Greeven et al. (2007)	10		58	41.3	68.5	
Hedman et al. (2014)				45.5	66.5	54.5
Hedman et al. (2011)	21	22.6	74	39.1		85
Jones (2002)		50.3				
Seivewright et al. (2008)			47			
Sorensen et al. (2011)			63	37	63	74
Tyrer et al. (2014)			53.5	48.7		
Visser and Bouman (2001)			50	35.6	59	63
Weck et al. (2014)			58	39		

Table 2. Study characteristics

Study	Diagnostic method	Physical health conditions included?	Trial location	Risk of bias rating
Barsky and Ahern (2004)	50% met DSM-IV	No	University	Low
Bouman and Visser (1998)	DSM-IV	No	Not known	Low
Bourgault-Fagnou and Hadjistavropoulos (2013)	None	Yes	University	High
Buwulda et al. (2007)	DSM-IV	No	Not known	Low
Clark et al. (1998)	DSM-III-R	No	Community	Low
Greeven et al. (2007)	DSM-IV	No	Hospital	High
Hedman et al. (2014)	DSM-IV	No	University	Low
Hedman et al. (2011)	DSM-IV	No	Hospital	Low
Jones (2002)	None	Yes, 50% of participants	Community	High
Seivewright et al. (2008)	HAI score	Yes	Hospital/ Community	High
Sorensen et al. (2011)	ICD-10	No	Hospital	Low
Tyrer et al. (2014)	DSM-IV	Yes	Hospital	High
Visser and Bouman (2001)	DSM-IV	No	Community and University	Low
Weck et al. (2014)	DSM-IV	No	University	High

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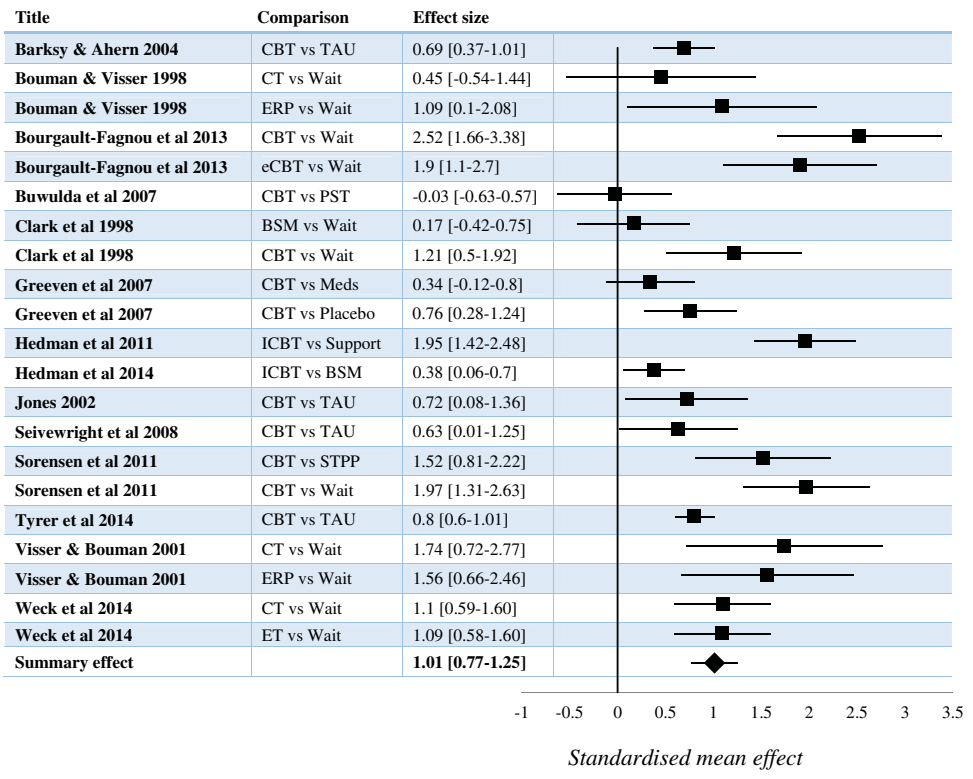

Figure 2. (Colour online) Forest plot of individual and pooled effect sizes of CBT for HA. The vertical line represents the null hypothesis position of no effect from CBT. The centre of the diamond represents the mean effect size across studies. The error bars on each study's effect size estimate represent the 95% CI.

Overall meta-analysis results

A meta-analysis of the overall effect of CBT on health anxiety outcome scores, compared with all control conditions (21 comparisons: active therapy, wait-list, TAU, medication and placebo medication) was conducted, resulting in a large mean effect size of $d = 1.01$ (95% CI (0.77–1.25); see Fig. 2.

The heterogeneity analysis was significant ($Q = 89.45$, $P < 0.0001$, $I^2 = 75.15$), indicating substantial heterogeneity between studies. The funnel plot (see Fig. 3), which plots standard error against effect size, was symmetrical and so did not indicate publication bias. A large range at the top of the funnel was identified, indicating that studies with lower standard error found a wide range of effect sizes, which was not predicted as an increase in precision is expected as standard error decreases.

At 6-month follow-up (seven comparisons), a large effect size was again found ($d = 0.91$, 95% CI 0.39–1.44). The heterogeneity analysis was significant ($Q = 35.34$, $P < 0.0001$, $I^2 = 77.16\%$), again showing substantial heterogeneity between studies. The sensitivity

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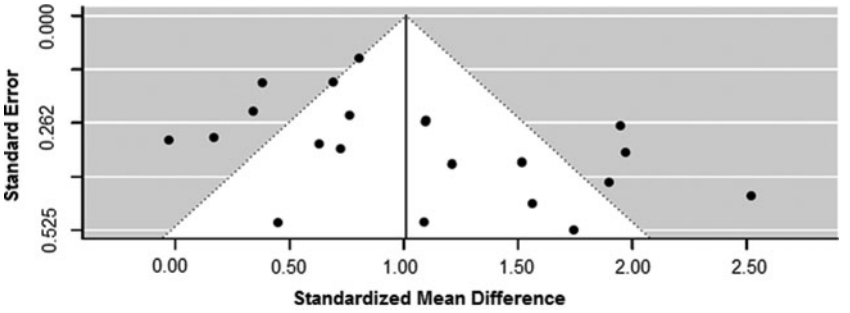

Figure 3. Funnel plot of each study's standardized effect size against standard error.

analysis revealed that removing each study in turn did change the *P* value, but never to the point of non-significance (*P* ranged from 0.0001 to 0.01).

At 12-month follow-up (six comparisons), a large effect size was still found ($d = 1.06$, 95% CI 0.48–1.63). The heterogeneity analysis was still significant ($P < 0.0001$, $Q = 31.1$, $I^2 = 76.27\%$), showing the studies still exhibited substantial heterogeneity. The sensitivity analysis revealed that removing each study in turn did change the *P* value, but not to the point of non-significance (*P* ranged from 0.0001 to 0.001).

General anxiety outcome measures at pre- and post-therapy revealed a small effect size ($d = 0.42$, 95% CI 0.26–0.58). The heterogeneity analysis was significant ($Q = 29.57$, $P = 0.01$, $I^2 = 43.6\%$), representing moderate heterogeneity between studies. The sensitivity analysis revealed that removing each study in turn did not change the *P* value.

Depression outcome measures at pre- and post-therapy were also analysed, resulting in a small effect size ($d = 0.45$, 95% CI 0.31–0.58). The heterogeneity analysis was not significant ($Q = 25.43$, $P = 0.11$, $I^2 = 23.52\%$), indicating that the studies produced comparable estimates of this effect. The sensitivity analysis revealed that removing each study in turn did not change the *P* value.

Subgroup analysis

The meta-analysis was conducted several times to analyse the effect of including subgroups. Health anxiety outcome measures were compared for studies which included participants with physical health problems, and those which excluded physical health problems. In the studies including participants with health conditions ($\kappa = 5$), a large effect size was found ($d = 1.16$, 95% CI 0.74–1.58). A comparably large effect size was also found for studies that excluded participants with health conditions ($\kappa = 16$, $d = 0.96$, 95% CI 0.67–1.24). The similarity of these two effect sizes, and the overlap of their confidence intervals, suggests that the inclusion of participants with physical health conditions did not account for the heterogeneity between studies, and also does not significantly impact the efficacy of CBT for HA.

Health anxiety outcomes were compared for studies that required participants to be assessed against DSM or ICD criteria before inclusion, versus those that did not. Those that included participants without a validated diagnosis ($\kappa = 5$) found a large effect size ($d = 1.19$, 95% CI

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0.66–1.71). Studies that required a validated diagnosis ($\kappa = 16$) also had a large effect size ($d = 0.96$, 95% CI 0.70–1.22). The comparable effect size measures suggest that CBT is an effective intervention for both subclinical and clinical levels of HA.

Health anxiety outcomes were compared between studies, which were identified as having a low and high risk of bias. Both low and high risk studies were found to have a large effect size and significant heterogeneity. High risk studies ($\kappa = 10$) had a mean d of 0.85 (95% CI 0.52–1.18), and significant heterogeneity ($Q = 22.83$, $P < 0.01$, $I^2 = 55.5\%$). Low-risk studies ($n = 11$) had a large effect size ($d = 1.13$, 95% CI 0.81–1.45) and significant heterogeneity ($P < 0.0001$, $Q = 66.40$, $I^2 = 81.7\%$). Again, the overlapping ranges suggest the two sets of studies were comparable.

Subgroup analysis was conducted on the different types of control groups that CBT was compared with: treatment as usual (TAU), wait-list, and an active control (psychological therapy, psychosocial support, medication and placebo). A large effect size was found when CBT was compared with wait-list ($\kappa = 10$; $d = 1.45$, 95% CI 1.13–1.77), with significant heterogeneity ($Q = 18.4$, $P < 0.05$, $I^2 = 44.7\%$). A significantly smaller effect size was found when CBT was compared with TAU ($\kappa = 4$; $d = 0.76$, 95% CI 0.6–0.92), and the heterogeneity analysis was not significant ($Q = 0.55$, $P = 0.91$, $I^2=0.0\%$). A medium effect size, not reliably different from the wait-list comparison, was found when CBT was compared with other active treatments ($\kappa = 7$; $d = 0.71$, 95% CI 0.26–1.16), and the test for heterogeneity was significant ($Q = 41.13$, $P <0.0001$, $I^2 = 82.25\%$).

Discussion

Summary of results

The analysis suggested the effect of CBT on health anxiety, compared with the full range of control conditions, was positive and substantial, with a mean change in symptoms of 1.01 standard deviations. There was, however, significant heterogeneity between the included studies, which was expected given the range of participants, study protocols and outcome measures employed between the RCTs. There was no evidence of publication bias that might affect our estimate of the overall effect, and subgroup analysis revealed no effect of medical condition, formal diagnosis, or study quality on the effect size estimate. Subgroup analysis found that CBT performed better when compared with wait-list than when compared with TAU, which is perhaps unsurprising given that wait-list is the least active of all the control conditions in the included RCTs. While there was no significant difference found between CBT compared with wait-list and CBT compared with active treatments, the former had a large effect size and the latter a medium effect size.

Comparison with previous reviews

This study updates the results of the previous reviews conducted by Thomson and Page (2007) and Olatunji et al. (2014). Although the more recent review by Olatunji et al. (2014) similarly found a large effect of CBT for HA immediately post-therapy, the methodology of that study leaves room for doubt. The present review excluded RCTs that recruited people with medically unexplained symptoms, who represent a distinct diagnostic category, but these were included in the Olatunju et al. (2014) review. The systematic search strategy and more

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recent search date meant that the current review included five RCTs which were not included in the Olatunji et al. (2014) paper (Clark et al., 1998; Jones, 2002; Hedman et al., 2014; Tyrer et al., 2014; Weck et al., 2014). It is important that the inclusion criteria were narrowed and that recent RCTs were included because this increases the validity of results. This review therefore provides robust evidence that CBT is an effective intervention for HA, and that it also has a small effect on secondary outcomes such as depression and anxiety.

Our finding a large effect size for CBT immediately following therapy adds to the results of the Cochrane review conducted by Thomson and Page (2007), who found that CBT approached – but fell short of – a significant effect size for HA outcomes immediately after therapy. The difference between the two reviews could be due to the smaller number of studies available to meta-analyse at the time of their search in 2005. Alternatively, it could be that the quality of RCTs has improved, with the disambiguation of the difference between medically unexplained symptoms and HA in the DSM-5, and increased research and understanding of the maintaining factors for HA.

In terms of secondary outcome measures, the present study found small effect sizes for the effect of CBT for HA on depression and generalized anxiety at pre- and post-therapy. This is consistent with both Olatunji et al. (2014) and Thomson and Page (2007). These changes in secondary outcomes could indicate the generalized, non-specific benefits of a problem-specific intervention, which are perhaps less influenced by factors such as the conflation of medically unexplained symptoms and HA (as was apparent in the Olatunji meta-analysis), and the size and quality of the RCT.

The present review found large effect sizes at 6- and 12-month follow-up, as well as immediately after treatment. This contrasts with the results of Olatunji et al. (2014), who found only a small effect size at follow-up. This difference could be due to the well-defined follow-up measure in the present meta-analysis, with the separation of 6- and 12-month follow-up measures. Our finding of sustained benefits up to 12 months is important, because the trajectory of effect size over time is an indication of the long-term effects of the intervention, and also because HA has a low natural recovery rate (olde Hartman et al., 2009). This meta-analysis therefore provides support for the long-term positive effects of CBT for HA.

Clinical implications

Another important finding of the present meta-analysis was that there was no effect of including participants with medical illness on the positive outcome of CBT intervention. This group is more likely to experience realistic negative automatic thoughts due to the presence of a physical health condition, which in turn might be expected to have negative implications for treatment outcome. However, this meta-analysis provides evidence that realistic automatic thoughts are not a treatment barrier, and so this group should be offered CBT treatment for their HA. Finding that CBT is still an effective intervention for people with physical health problems alongside their health-related anxiety is important given the higher prevalence of HA, and the greater impact on functioning, in this group (Robbins & Kirmayer, 1996).

Another finding from the subgroup analysis was that CBT for HA was effective for people with and without a formal diagnosis of HA. This is important given the high percentage of people with subclinical HA (Gureje et al., 1997), for whom CBT is revealed here to be a helpful intervention. This highlights the importance of providing treatment to individuals who

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may not meet all diagnostic criteria, but who are still experiencing significant distress and decreased quality of life, as they are likely to respond to treatment.

A final point of interest is that one RCT included in this review specifically recruited older adults with HA (Bourgault-Fagnou & Hadjistavropoulos, 2013). This trial found very large effect sizes, larger than any other included RCT, suggesting that CBT is a highly effective intervention for older adults with HA.

Strengths and limitations

A limitation of this review is the lack of inter-rating for the risk of bias assessment. This left the quality assessment open to human error, potentially reducing its accuracy. The sub-group analysis revealed that there was less heterogeneity between the results of studies marked as being at high risk of bias compared with those at low risk; this highlights the challenges of assessing quality in papers based on a quantitative system drawing on information presented by the original authors in their method sections. This also highlights that quality is a difficult construct to assess between different papers, and that it may vary depending on the methodology – a one-size-fits-all approach to quality assessment may not lead to robust results. A decision was made to give a score of quality, in order to be able to analyse high and low quality papers separately and see if quality impacted on effect size, as quality could account for some of the heterogeneity between studies. This quality scoring system did not appear to be valid, as there was no significant difference in effect sizes between studies with high and low bias scores.

Another limitation is that there was significant heterogeneity between included studies, but this heterogeneity was not accounted for by the differences in study design as explored in the *a priori* subgroup analyses. This is likely to be due to multiple factors in the papers which make finding differences between subgroups difficult, e.g. heterogeneity in study design and outcome measuring, and the similarity of effects for different therapeutic interventions. In some ways this has provided helpful clinical information, e.g. the presence of a physical health condition or subclinical HA is not a treatment barrier. At the same time, it does not point to RCT features that affect treatment outcome, which would be helpful information in the design of future trials.

A strength of this review is the separation of the diagnostic categories of HA and medically unexplained symptoms, with only studies that included participants with HA included. This increases the validity of the results for people with HA, as medically unexplained symptoms is a different psychological problem, which requires targeted treatment considerations. As such, the efficacy of CBT for these conditions should be reviewed separately. The clear definition of traditional CBT approaches has also helped to maximize treatment homogeneity across the included RCTs, increasing the validity of the results. The inter-rating of all data in the meta-analysis and the *a priori* decisions about how to deal with inter-rater discrepancies increased the rigour of the review process and therefore lowered the risk of bias in the meta-analysis results.

Conclusions

This systematic review and meta-analysis provides clear evidence supporting CBT treatment of HA, in people with and without medical problems, and in people with subclinical as well as

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clinical levels of HA. The use of CBT for HA now requires further exploration, to delineate the active treatment elements. For example, further investigation is needed into the role of cognitive restructuring compared with purely behavioural approaches, and on the role of attentional processes within HA and whether these are a key treatment target. Such work would continue to refine and develop the problem-specific model of CBT for HA.

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