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Systematic review of the efficacy of cognitive behaviour therapies for childhood and adolescent anxiety disorders

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Purpose. To review the effectiveness of cognitive behaviour therapy (CBT) as a treatment for anxiety disorders of childhood and adolescence.

Method. Studies were included if they treated young people (under 19 yrs) with diagnosed anxiety disorder (excluding trials solely treating phobia, PTSD or OCD), had a no-treatment control group, and used diagnosis as an outcome variable. A search of the literature, incorporating electronic databases, hand search and expert consultation, yielded 10 randomized controlled trials that were appropriate for inclusion.

Results. The outcome of interest was remission of anxiety disorder. Employing conservative criteria, the remission rate in the CBT groups (56.5%) was higher than that in the control groups (34.8%). The pooled odds ratio was 3.3 (CI = 1.9–5.6), suggesting that CBT has a significant effect.

Conclusions. CBT is useful for the treatment of anxiety in children over the age of 6 years. However, we still know little about the treatment of younger children or about the comparative efficacy of alternative treatments. Most of the trials were efficacy trials, and have limited generalizability. Reporting of many aspects of the trials was weak.

Anxiety disorders are the most common psychiatric disorder of childhood (Bernstein & Borchardt, 1991). Despite being less likely to present to services than other conditions, such as behaviour problems, anxiety is a serious condition that can have negative consequences in a number of domains, such as academic and interpersonal functioning

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(Pine, 1997). Moreover, anxiety during childhood and adolescence is often unremitting into adulthood (Last, Phillips, & Statfield, 1987), and is associated with other serious conditions, such as depression (Kovacs, Gatsonis, Paulauskas, & Richards, 1989) and substance misuse (Kushner, Sher, & Beitman, 1990; Lehman, Brown, & Barlow, 1998).

In light of these concerns, recent years have seen an increase in research into the treatment of anxiety disorders in children and adolescents. The first reported trial (Kendall, 1994) took its lead from the adult literature, and treated 9- to 13-year-old anxious children using cognitive behaviour therapy (CBT). The results were encouraging, and since that date, a number of studies have been published, reporting the use of varied forms of CBT with anxious young people.

These published studies, though all reporting positive results for CBT, have varied in reported effects, and in their size and complexity. It is, as a result, difficult for the reader to estimate the actual level of effectiveness of CBT for this population. In these situations, it is often useful to conduct a 'systematic review'. A systematic review has a number of strengths (e.g., Mulrow, 1995). First, the pooling of studies allows the power and precision of effect size estimates to be increased. This is particularly relevant in the field of CBT for children and adolescents, where many of the trials are small and potentially underpowered. Secondly, it allows the reviewer to re-evaluate hypotheses and methods used in the field in light of the mass of previous work. An important aspect of this review was the examination of the methods used to study the effectiveness of CBT in young populations, and one aim was to identify pitfalls in the extant research. Thirdly, a systematic review has been likened to 'a tower of statistical power that allows researchers to rise above the body of evidence, survey the landscape, and map out future directions' (Gelber & Goldhirsch; 1991; cited in Mulrow, 1995, p. 4). This review examined the overall picture that we now have of CBT for anxiety in children and adolescents and aimed to identify lacunae and future pathways for research. Finally, these quantitative syntheses of the literature allow for useful statistics such as 'numbers needed to treat' to be estimated.

Method

Definitions and inclusion criteria

The review was restricted to randomized controlled trials of CBT for childhood or adolescent anxiety disorders.

A trial was included if all participants were aged 18 years or younger and had a diagnosis of anxiety disorder. Trials that exclusively treated participants with obsessive-compulsive disorder (OCD), post-traumatic stress disorder (PTSD) or simple phobia were excluded, as the outcomes for these disorders may differ significantly from those for more typical anxiety disorders. Trials that did not specifically treat anxiety disorders (e.g., some targeted 'school refusal') were excluded unless it was clear that the target disorder of all participants was anxiety-based. Trials that treated both diagnosed and undiagnosed (e.g., subclinical) cases of anxiety were only included if they presented separate analyses for the diagnosed sample. Trials were required to use formal diagnosis as an outcome variable. Unfortunately, trials relying on self-report measures as outcome variables could not be included, as the large and highly variable range of instruments that are used for this purpose renders studies difficult to compare.

Trials were required to randomize participants to a CBT or to an inactive control

group. The initial aim of the review had been to compare CBT to both inactive controls and to other interventions. However, upon examination of the literature, only two studies that were designed to compare CBT with another active treatment were found (Beidel, Turner, & Morris, 2000; Bernstein *et al.*, 2000). In each case the control treatment group also received CBT or elements in their treatment that could be considered cognitive behavioural, and were, therefore, inappropriate for inclusion in this review. Further comment on these studies appears in the Discussion. It was decided, therefore, to include only trials that compared CBT with no treatment or an inactive treatment.

Search methods

The search for trials comprised electronic databases; reference lists of reviews and book chapters; and hand search of journals.

Electronic databases

The following databases were searched:

- Cochrane Controlled Trials Register
- Current Controlled Trials
- Medline
- Embase, Psycinfo
- Cinahl
- NHS Economic Evaluation Database
- National Technical Information Service
- Index to Scientific and Technical Proceedings (ISI Web of Science).

Full search criteria are available on request.

Reference lists

The reference lists of recent reviews and trials were examined.

Hand search

Journals were selected for hand search as follows:

- Every journal that had published a trial identified by the previous stages.
- Journals that were known to publish trials on the treatment of anxiety disorders.

The content lists of these journals, from January 1990 to August 2003, were hand searched by a child psychiatrist and a trainee clinical psychologist (P. C and C. F.) using the search terms produced for the electronic search. The following journals were hand-searched:

- *Journal of Child Psychology and Psychiatry*
- *British Journal of Psychiatry*
- *Behavioural and Cognitive Psychotherapy*
- *British Journal of Clinical Psychology*
- *Psychological Medicine*
- *Journal of the American Academy of Child and Adolescent Psychiatry*
- *Journal of Consulting and Clinical Psychology*

- *Journal of Clinical Child and Adolescent Psychology (until July 2001 only)*
- *Journal of Abnormal Psychology*
- *Journal of Abnormal Child Psychology*
- *Cognitive Therapy and Research*
- *Behaviour Research and Therapy*
- *Behaviour Therapy*.

Other searches

Experts in the field were presented with the list of trials produced by the stages outlined above and were asked to add any further completed trials that they were aware of.

Search results

The search identified 22 randomized controlled trials in which CBT had been employed to treat young people with the anxiety disorders that were the focus of this study. Of these trials, 12 were excluded on one or more grounds.

Four trials were excluded as all analyses included some children without a formal diagnosis of anxiety disorder, or because diagnosis was not used as an outcome variable (Blagg & Yule, 1984; King *et al.*, 1998; Mendlowitz *et al.*, 1999; Warren, Smith, & Velten, 1984).

Six studies were excluded as there was no appropriate inactive control group. Last, Hansen, and Franco (1998) was excluded, as their placebo-control group received 'educational support therapy'. This consisted of a combination of 'educational presentations and supportive psychotherapy'. Some aspects of the supportive psychotherapy, however, mirrored techniques that are often used in CBT (e.g., keeping diaries of thoughts and feelings, and learning to distinguish fear and anxiety). Bernstein *et al.* (2000) was excluded as that study compared CBT with CBT plus imipramine. Cobham, Dadds, and Spence, (1998) was excluded as all participants received CBT or CBT plus a cognitive-behavioural family intervention. Beidel *et al.* (2000) was excluded because control participants received 'testbusters', which were described as 'an active but non-specific intervention'. Both Barrett and Turner (2001) and Manassis *et al.* (2002) compared group and individually delivered CBT, and had no group who received no CBT, and these two trials were thus ineligible for this review. All remaining trials compared CBT to no treatment.

Kendall, Brady, and Verduin (2001) was excluded as the authors made it clear that a large proportion of the data had been included in previously reported trials (the study was chiefly concerned with examining the effects of comorbidity on outcome of CBT).

This left ten trials. Four of these were conducted by one research group (Barrett, 1998; Barrett, Dadds, & Rapee, 1996; Dadds, Spence, Holland, Barrett, & Laurens, 1997; Shortt, Barrett, Dadds, & Fox, 2001), and three by another (Flannery-Schroeder & Kendall, 2000; Kendall, 1994; Kendall *et al.*, 1997), but in all of these instances, it appeared that the data were derived from separate samples.

The list of trials that were included in the final analysis, together with descriptive data, is reproduced in Appendix A.

Trial assessment

The quality of each trial report was assessed using a modified version of the form produced by the University of York, Centre for Reviews and Dissemination – a British

centre specialising in the review and dissemination of healthcare research (University of York, 2001). To this form were added a number of criteria that were felt to be of specific importance in mental health trials. For each criteria, there were a number of aspects of good trial design that would be present in an 'ideal' study. The resulting criteria, and median / range of scores on each are presented in Appendix B. Each criterion was scored on a scale of 0–3 for each trial, where 0 indicated that the trial failed (or could not be scored) on all 'ideal' aspects, and 3 indicated that the trial was judged to have been adequate or better on all aspects.

In order to examine the reliability of this scale, each trial was rated independently by two of the authors (S. C. H and P. C.). The level of convergence between the total scores generated by each rater was examined using an intra-class correlation. This indicated that inter-rater reliability was high (r = .81). The mean score and range of scores for each quality category is presented in Appendix B.

For those trials where the independent raters produced discrepant subscale scores, a final score was assigned by consensus. Possible total score ranged from 0 to 27. Total quality scores ranged from 13 to 17.5, and the mean score was 16.2.

Outcome measurement

The main outcome assessment was the number of cases who were diagnosis-free at the post-treatment assessment. We had intended to perform a separate analysis based on self-report questionnaire outcome. However, the studies used a variety of instruments for this purpose, and those using the same instrument presented the data in different ways. For example, of the seven studies employing the Revised Children's Manifest Anxiety Scale (RCMAS; Reynolds & Richmond, 1978), four used raw RCMAS scores, and three used transformed t scores, meaning that two smaller and less meaningful analyses would have been required.

Statistical analysis methods

For each study, the odds ratio of remission after treatment was estimated. The pooled odds ratio and confidence intervals were calculated using the log odds procedure. A conservative analysis using the numbers randomized to each group was computed. This analysis assumed a favourable outcome in waiting list controls who were not followed up and an adverse outcome in the treated participants who were not followed up.

In studies where more than one type of CBT was included (e.g., studies where group and individual CBT were compared), these were pooled to give an overall effect for CBT.

A test of heterogeneity was carried out (DerSimonian & Laird, 1986). As there was some evidence of heterogeneity, a random effect estimate of the pooled odds ratio has been presented. The relationship between study size and effect size was also examined using weight least square regression (Macaskill, Walter, & Irwig, 2001).

Results

Table 1 summarizes the outcomes of the studies included.

As can be seen in Table 1, all of the studies included in this review reported a positive odds ratio. A number of the studies, however, had confidence intervals spanning 1.0, indicating the possibility that the result might reflect a Type I error.

Table 1. Outcome data and odds ratios for the studies included in the systematic review

	CBT		Waiting list		Followed-up cases	Randomized cases
Study	WD	R/FU	WD	R/FU	Odds ratio (95% CI)	Odds ratio (95% CI)
Barrett *et al.* (1996)	0	37/53	3	6/23	6.12 (2.1–17.9)	4.19 (1.6–11.2)
Barrett (1998)	6	25/34	4	4/16	7.46 (2.0–27.7)	2.42 (0.8–7.1)
Dadds *et al.* (1997)	1	27/41	1	27/52	1.76 (0.8–4.1)	1.59 (0.7–3.6)
Flannery-Schroeder & Kendall (2000)	6*	17/31	0*	0/12	30.17 (1.6–556.2)	21.34 (1.2–388.2)
Hayward *et al.* (2000)	1	5/11	1	1/22	12.13 (1.6–90.5)	6.31 (1.1–35.0)
Kendall (1994)	2*	16/25*	0	1/20	22.58 (3.6–142.8)	18.65 (3.0–115.7)
Kendall *et al.* (1997)	13*	25/47*	11*	2/23*	9.75 (2.3–40.7)	1.34 (0.6–3.2)
Silverman *et al.* (1999)	12	16/25	3	2/16	10.1 (2.1–48.1)	2.02 (0.6–6.5)
Shortt *et al.* (2001)	1	33/53	5	1/12	12.53 (2.1–75.0)	2.76 (0.9–8.3)
Spence *et al.* (2000)	3*	24/33*	0	1/14	23.21 (3.7–147.0)	17.64 (2.9–109.0)
Pooled	45	225/353	28	45/210	8.13 (4.4–15.2)	3.27 (1.9–5.6)

* Data extrapolated from other information in the paper.
Notes: WD = withdrawn; R/FU = recovery/follow-up.

We then examined the proportion of children randomized to each condition whose anxiety had remitted by the end of the treatment period. Children randomized to receive CBT had a 56.5% chance of remission of their anxiety (225/398). In comparison, children who were randomized to control groups had a 34.8% chance of remission (73/210).

The analysis of these data produced evidence of heterogeneity, $\chi^2(9) = 16.4$, $p = .059$, with evidence of reduced effect size for larger studies. This is demonstrated in Figure 1, which illustrates the odds ratio of recovery in relation to study size.

The odds of recovery in the treatment group as compared to the waiting list control was 3.27 (95% CI = 1.92–5.55, $p < .001$).

Numbers Needed to Treat (NNT) analysis

The NNT statistic represents the number of patients who must receive an intervention to prevent one additional adverse event (in this case, unremitted anxiety). Using the conservative analysis described above, the number needed to treat in this study was four.

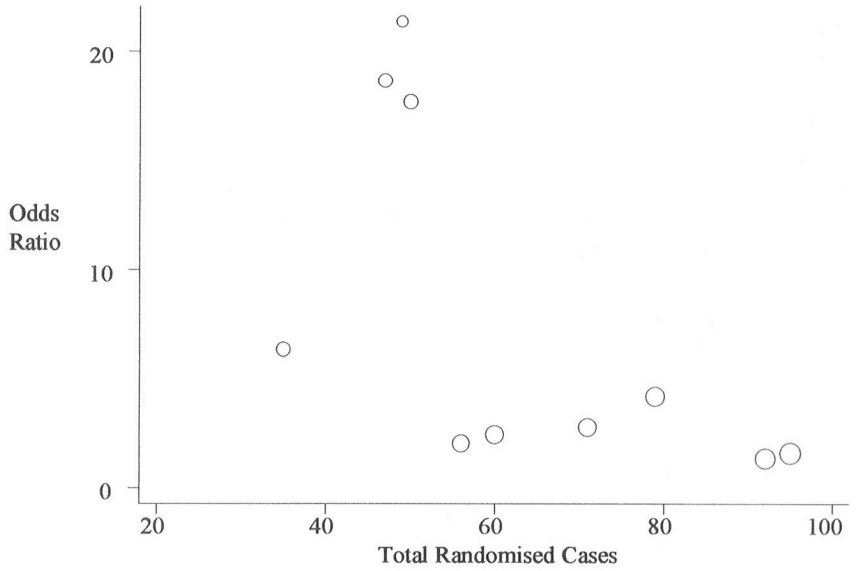

Figure 1. Odds ratio relative to study size

Analysis of follow-up cases

The above analyses were repeated using a less conservative analysis based on follow-up cases only. The odds of recovery in those who complied with treatment compared to those followed up from the control group was 8.13 (95% CI = 4.35–15.22, $p < .001$). The percentage of followed up children whose anxiety remitted after CBT was 63.74%, compared to 21.43% in children followed up from the control groups. The NNT statistic for this less conservative analysis was three.

Discussion

The results of this study indicate that CBT is an effective intervention for anxiety disorders of childhood and adolescence, when compared to a no-treatment control. All but one of the studies included in this research reported a positive effect of CBT in terms of diagnostic outcome, and all had an odds ratio of greater than one. The pooled odds ratio was 3.27, with 95% confidence intervals that did not span 1.0, indicating that when viewed as a whole, the studies reported are suggestive of a strong positive effect of CBT when compared to no treatment. The single study that did not report a statistically significant effect of CBT for clinical cases of anxiety (Dadds *et al.*, 1997) was actually a school-based early intervention and prevention programme, and although a number of children met diagnosis for an anxiety disorder, the most severe cases were referred elsewhere for treatment. As a result, the children who had the highest levels of symptoms (and, therefore, the highest potential for change) were not included in this study.

The NNT analysis reported here indicates that four children with anxiety disorders would need to be treated in order for one additional case to remit. This compares well with treatments for other psychiatric disorders (e.g., a family intervention for

schizophrenia has an NNT of seven to prevent relapse over one year; Best Evidence Reviews, 1996). Although there are a number of difficulties inherent in using this statistic (e.g., Smeeth, Haines, & Ebrahim, 1999), it seems that the economic costs of employing CBT for treatment of this group are likely to be modest. Little research has examined the health economic implications of the use of CBT with children and adolescents, and this would appear to be a fruitful avenue for future investigation.

Despite the positive results of this systematic review, these results should not yet be taken to indicate that CBT is well established as a treatment for anxiety disorders in young people. Our understanding of the efficacy of CBT is still quite limited. In particular, this review did not attempt to evaluate the effectiveness of CBT in comparison to other available treatments. This was due to the fact that very few such studies are published. In the course of the literature review, only two studies designed to compare CBT with another active treatment were found. Bernstein *et al.* (2000) compared CBT plus imipramine with CBT plus placebo for anxiety-based school refusal. Those receiving both imipramine and CBT did significantly better than those just receiving CBT, in terms of their school attendance, but not in terms of their anxiety symptoms. Beidel *et al.* (2000) compared CBT with an educational support intervention, which included study skills and an element of exposure. The results suggested that the CBT intervention was significantly more effective at ameliorating the social anxiety symptoms with which their participants were referred, than the educational intervention. Although these results are interesting, more research is needed, in particular comparing CBT with widely prescribed alternative treatments for child and adolescent anxiety, such as pharmacotherapy. Kendall *et al.* (1997) point out that this is a necessity before any treatment can be considered 'well established'.

Although reviews of CBT in the adult field are now able to examine CBT in the context of specific anxiety disorders (e.g., social phobia, panic), this is not yet possible within the child and adolescent literature. The majority of trials in this field have treated anxiety as a unitary disorder. A small number of the trials included in this review conducted some examination of their results according to the primary disorder of patients, but this practice was insufficiently detailed or widespread to allow us to conduct our analysis according to specific individual anxiety disorders. It is now widely acknowledged within the adult field that different anxiety disorders benefit from different approaches, and it is likely that adopting this approach for children and adolescents will result in increased effect sizes for CBT.

None of the studies reported here attempted to treat a child younger than 6 years of age—and very few attempted to treat children that young. Moreover, the literature search described above yielded no trials that attempted to evaluate a treatment specifically for young anxious children. This lack of evaluated treatment options is concerning in light of mounting evidence that many anxiety disorders have a very early onset, and that clinical anxiety is not uncommon in this age group (Office of National Statistics, 2000).

Many of the studies included in this review made use of extensive exclusion criteria (see Appendix A). In addition, many of the studies recruited their participants from non-clinical settings. While the need to conduct early research on pure samples is understood, these exclusions reduce the generalizability of the results. As a consequence, we still know little about the effectiveness of CBT for children with very severe or comorbid disorders.

This systematic review only included results up to the end of therapy. Although a number of trials reported follow-up outcome data, this was rarely compared to a no-

treatment control group. Clearly there are often ethical reasons for not wishing to do this, but as a result we know little about the long-term effectiveness of CBT. Future trials that compare CBT with other active treatments, however, should take the opportunity to conduct controlled, long-term follow-up.

The quality of the trial reporting in this study was variable. Using well-established criteria for assessing the trial reports, each scored poorly in at least one domain. Areas that were particularly poorly reported were:

(a) randomization methodology;
(b) whether the study used intention-to-treat analysis;
(c) the power of the study; and
(d) attrition rate.

In many cases, this will have simply been a flaw in the reporting of the study—high quality procedures will have been awarded a low score on the basis that they were simply not described in the study concerned. Additionally, it may be that journal editors, in an attempt to conserve space, ask authors to remove what appears to be extraneous detail from the text. This has important consequences for assessing the quality of a trial, and may need to be given more consideration in future. It is also likely, though, that some practices that are now deemed essential in the conducting of a trial will not have been established at the time these trials were carried out. It is important, though, for the future of the field, that new trials are conducted to these high standards, and that the report of the trial reflects this.

Analyses of the data from the studies included in this review produced evidence of heterogeneity of effect size, with larger effect sizes for small studies. Such statistical heterogeneity is sometimes taken as evidence of publication bias, with smaller negative studies failing to be published. However, other explanations may also account for this distribution of data. In particular, this pattern often arises as a result of clinical heterogeneity of the studies. Clearly, studies will employ different types of therapists, different inclusion and exclusion criteria, and so on, all of which can have an impact on the effect size. In trials of psychological therapies in particular, this type of heterogeneity can also arise as a result of smaller trials providing better therapy. Smaller trials, for instance, often have smaller numbers of highly selected and well-trained therapists, supervised by highly motivated grant holders. This may obviously have an impact on the quality of therapy received.

An examination of the studies included in this review does indicate a degree of heterogeneity in terms of the type of therapy given. In particular, there was variation in the degree to which parents were involved. It is generally felt that inclusion of parents in therapy is a positive feature, but there is currently little evidence to support this assumption. A number of trials have now compared the efficacy of simple CBT to CBT that includes a family component; although it was not possible to compare these approaches statistically in this study, it is worth noting that in two of the three studies included in this review, the CBT plus family intervention was more successful than CBT alone (see Appendix A for details). Two further studies that could not be included in the systematic review also testify to the additional effectiveness accrued by including a substantial parent-focused component to the intervention (Cobham *et al.*, 1998; Mendlowitz *et al.*, 1999).

Conclusion

In summary, the evidence suggests that CBT is a promising intervention for childhood and adolescent anxiety. This is especially true when the evidence for the efficacy of CBT is compared to that which is available for alternative approaches. For example, despite the fact that children with anxiety disorders are often referred for family therapy or psychodynamic therapy, the authors could find no randomized controlled evaluations of these approaches.

Despite the comparative strength of the evidence for CBT, we do not yet know whether CBT is effective in the long term, and we do not know whether it is as effective as other treatments. It is also likely that different children will respond to different approaches, or indeed to different types of CBT. For some groups, particularly very young children, it is likely that traditional CBT will never be appropriate.

Moreover, the results of this study indicate that while many children benefited from receiving CBT, over a third maintained an anxiety diagnosis at the end of treatment. There is clearly room for considerable improvement in the understanding and treatment of anxiety in this age group. The results of this systematic review indicate that we have made a promising start in the treatment of anxiety disorders in young people. However, the main conclusion is that considerably more high quality research is now needed.

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Appendix A: Randomized outcome studies of cognitive behaviour therapy for child and adolescent anxiety disorders

Study details	Therapy type/ duration	Comparison condition	Outcome measure	Exclusion criteria	Outcome[a]
Hayward *et al.* (2000), USA; Sample: SP[b] Age: 15.8 (±1.6) yrs	GCBT: 16 × 1.5 hrs	No treatment (WL)	ADIS	Current major depression. Current or previous panic, agoraphobia, substance abuse, psychotic disorder. Using psychotropic medication.	GBCT > WL
Barrett *et al.* (1996), Australia; Sample: OAD, SAD, SP Age: 7–14 yrs	ICBT: 12 × 60–80 min ICBT + FCBT: 12 × 60–80 min	Wait list (WL)	ADIS-C ADIS-P	'Principal diagnosis of simple phobia or other (non-anxiety) diagnoses.' Intellectual or physical disabilities. 'Anti-anxiety or depression medication.' Parents 'involved in acute marital breakdown'.	FCBT > ICBT > WL
Flannery-Schroeder & Kendall (2000), USA; Sample: GAD, SAD, SP Age: 8–14 yrs	GCBT* 18 × 90 min ICBT*: 18 × 50–60 min *Some parental advice given	Wait list (WL)	ADIS-IV-C AIDS-IV-P	'Disabling physical condition.' Psychotic symptoms. 'Current use of anti-anxiety of anti-depresant medication.'	ICBT = GCBT > WL
Kendall *et al.* (1997), USA; Sample: OAD/GAD, AD/SP, SAD Age: 9–13 yrs	ICBT: *M* = 18 × 60 min	Wait list (WL)	ADIS	Psychotic symptoms. Anti-anxiety medication	ICBT > WL

Study details	Therapy type/ duration	Comparison condition	Outcome measure	Exclusion criteria	Outcome[a]
Kendall (1994), USA; Sample: OAD, SAD, AD Age: 9–13 yrs	ICBT: 17 × 50–60 min	Wait list (WL)	ADIS-P	IQ below 80. 'Disabling physical condition.' Psychotic symptoms. Current 'anti-anxiety medications'.	ICBT > WL
Silverman *et al.* (1999), USA; Sample: OAD, GAD, SP Age: 6–16 yrs	FGCBT: Children: 12 × 55 min Parents: 12 × 55 min	Wait list (WL)	ADIS-C ADIS-P	Pervasive development disorder. Psychotic symptoms. Current treatment.	FGCBT > WL
Dadds *et al.* (1997), Australia; Sample: Any DSM-IV anxiety diagnosis[c] Age: 7–14 yrs	FGCBT[d]: 10 × 1–2 hrs 3 parents' sessions	Control schools (control)	ADIS-P	'Disruptive behaviour problems.' 'Development problems or disabilities.' English not spoken at home. Clinical anxiety severity rating of higher than 5 on an 8-point scale.	FGCBT = control
Barratt (1998), Australia; Sample: OAD, SAD, SP Age: 7–14 yrs	GCBT: 12 × 2 hrs FGCBT: 12 × 2 hrs	Wait list (WL)	ADIS	Intellectual or physical disabilities. Current 'anti-anxiety or anti-depression medication.' Parents 'involved in acute marital breakdown'.	GCBT = FGCBT > WL

Study details	Therapy type/ duration	Comparison condition	Outcome measure	Exclusion criteria	Outcome[a]
Spence *et al.* (2000), Australia; Sample: social anxiety Age: 7–14 yrs	GCBT: 14 × 90 min FGCBT: Children: 14 × 90 min Parents: 14 × 90 min	Wait list (WL)	ADIS-C-P	Severe learning difficulties. Medication for a psychological disorder. Severe 'emotional and behavioural disorders' other than anxiety.	FGCBT > GCBT > WL
Shortt *et al.* (2001), Australia; Sample: GAD, SAD, SP Age: 6.5–10 yrs	FGCBT: Children 12 × 50–60 min Parents: 6 hrs.	Wait list (WL)	DISCAP	'Intellectual or severe physical impairment.' Currently receiving other treatment	FGCBT > WL

Notes: [a]Outcome judged by number of remitted cases at end of trial. [b]Females only. [c]Subclinical sample also reported, but not included here. [d]All studies were clinic based except for Dadds *et al.* (1997), which was school based.
Abbreviations for therapy types: GCBT = group cognitive-behavioural therapy; ICBT = individual cognitive-behavioural therapy; FGCBT = group cognitive-behavioural therapy with significant family component.
Abbreviations for sample: SP = social phobia; OAD = over-anxious disorder; SAD = social anxiety disorder; GAD = generalized anxiety disorder; AD = avoidant disorder. 'Sample' refers to primary diagnosis in all of the above circumstances.
Other abbreviations: WL = waiting list control; ADIS = Anxiety Disorders Interview Schedule; ADIS-C = Anxiety Disorders Interview Schedule-Children; ADIS-P = Anxiety Disorders Interview Schedule-Parents; DISCAP = Diagnostic Interview Schedule for Children, Adoloscents and Parents.

Appendix B: Criteria used to assess the quality of the trials included in the systematic review. (N = 10)

Criterion	Median*	Range*
Randomization: An ideal study has: Use of remote site that was expert in randomization. Randomization by computer or other totally bias-free method. Separation of allocator from executor of assignment.	1	1–1.5
Recruitment method: An idea study has: Sampling from clinical settings. Non-use of convenience methods. Inclusion criteria specified. Exclusion criteria specified.	2	1–3
Group comparability at baseline: An ideal study has: Randomization on basis of key variables. A check that groups were equivalent at baseline on key variables. Covariation of key variables in analysis (if any group differences were found).	2	1–3
Study blindness: An ideal study has: Steps taken to ensure that outcome assessors were blind to treatment allocation throughout the study. Assessor blindness evaluated. Employment of some measures for which blindness could be established (i.e., not all self-report measures).	2	1–2.5
Therapeutic Integrity: An ideal study has: Co-interventions reported. Co-interventions taken into account in statistical analysis. Use of a manual. Reporting of therapist compliance. Methods for dealing with therapist failure to comply. Reporting of participant compliance.	2	1–3
Intention to treat analysis: An ideal study has: Use of intention to treat analysis. Minimal participants 'lost to follow up'. Minimal missing data.	1	0.5–1.5
Outcome measures: An idea study has: Well-validated measures. Measures suited to the construct being assessed. Use of multiple informants.	3	3–3
Power: An ideal study has: Adequate power calculation. Pre-setting of the number to be recruited. No analysis until all of the data was in (excluding follow-ups) Adequate study power (at least 80% to detect a clinically meaningful change).	1.5	1–2.5
Attrition rate: An ideal study has: Minimal attrition from each group?	2	0–3

*A higher score is indicative of a higher quality rating.