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**Cognitive Analytic Therapy (CAT): A review of the outcome
evidence and an investigation of the effectiveness of group
CAT for female survivors of childhood sexual abuse.**

By Rachel Simmonds

**Submitted in partial fulfillment for the award of
Doctor of Clinical Psychology at
The University of Sheffield**

November 2011

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DECLARATION

I declare that this work has not been submitted for any other degree at the University of Sheffield or any other institution. I further declare that this thesis is my own original work and that all sources have been duly acknowledged.

Thesis Structure

This literature review has been prepared in accordance with the current guidance for contributors to *Psychology and Psychotherapy: Theory, Research and Practice*. The research report has been prepared in accordance with the current guidance for contributors to *The British Journal of Clinical Psychology*. Copies of the University journal approval letter and guidelines for authors are provided in Appendix i and ii, respectively.

WORD COUNT

Literature review without references	7933
Research report without references	11802
Appendices	413
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Efficacy and Effectiveness of Cognitive Analytic Therapy:

A Review of the Outcome Evidence

Despite the popularity of Cognitive Analytic Therapy (CAT), the evidence-base is limited in comparison with some other psychotherapies. No review has been previously undertaken to evaluate the nascent evidence-base and guide future research. Reviewing 21 papers revealed that the evidence-base for CAT has largely bypassed the rigours of efficacy trials. A between-studies effect size of 0.81 across nine studies indicates promising effectiveness across diagnostic conditions. However, there is a need to conduct larger-scale research, in line with established frameworks, to develop a coherent, robust and relevant knowledge-base.

Group Cognitive Analytic Therapy for Female Survivors of

Childhood Sexual Abuse: A practice-based study

A practice-based study was conducted to investigate the effectiveness of 24-session group Cognitive Analytic Therapy (GCAT) for adult female survivors of childhood sexual abuse (CSA). Validated measures of psychological functioning were administered at assessment, start and completion of GCAT. Statistically significant improvements were observed for intention-to-treat and 'completer' samples. 'Completer' analysis indicated an uncontrolled pre/post treatment effect size of 0.34. Reliable and clinically significant improvements in global functioning were achieved by 8% (n=8) of 'completers'. Given the severity of distress experienced by this sample and previous contact with mental health services, GCAT as an adjunct to secondary mental health clinical care, appears to be a promising intervention for adult female survivors of CSA. Clinical, organisational and theoretical implications are discussed and future directions for research identified.

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Table of Contents

<i>Declaration</i>	<i>ii</i>
<i>Word count</i>	<i>iii</i>
<i>Abstract</i>	<i>iv</i>
<i>Acknowledgements</i>	<i>v</i>

Chapter 1: Efficacy and Effectiveness of Cognitive Analytic Therapy:

A Review of the Outcome Evidence	1
Introduction.....	3
Method.....	7
Results.....	11
Discussion.....	38
Conclusions.....	43
References.....	45

Chapter 2: Group Cognitive Analytic Therapy for Female Survivors of

Childhood Sexual Abuse: A practice-based study	60
Introduction.....	62
Method.....	71
Results.....	85
Discussion.....	110
Conclusion.....	123
References.....	125

Appendix i: Journal approval letter.....	143
Appendix ii: Guidelines for authors.....	144
Appendix iii: Example of outcome from search term	152
Appendix iv: Quality rating checklist*.....	153
Appendix v: Rating sub-scale and total score for studies clustered according to treatment focus.....	154
Appendix vi: Letters of ethical approval.....	156
Appendix vii: Measures*	161
<i>Brief Symptom Inventory</i>	
<i>Inventory of Interpersonal Difficulties-32</i>	
<i>Rosenberg Self-Esteem Scale</i>	
<i>Hospital Anxiety and Depression Scale</i>	
<i>General Health Questionnaire-12</i>	
Appendix viii: Diagram of 'lessons learnt to survive'.....	162

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Efficacy and Effectiveness of Cognitive Analytic Therapy:

A Review of the Outcome Evidence

Purpose. Despite the popularity of Cognitive Analytic Therapy (CAT) in clinical practice, this review represents the first to evaluate the range and quality of CAT outcome studies.

Methods. A search of PsychInfo, Medline, CINAHL and Cochrane databases from 1960 to 2011 was conducted to identify efficacy and effectiveness studies reporting CAT outcomes. An existing validated research quality checklist was used to appraise the quality of CAT disorder-specific evidence, using the 'hourglass' model to inform findings.

Results. A total of 21 papers met inclusion criteria, including three randomised control trials. Whilst the CAT evidence-base is predominated by small-scale, effectiveness studies with complex and severe populations, an overall uncontrolled effect size of 0.81 across nine CAT outcome studies was found. CAT outcomes are largely equivocal to other approaches, although studies fail to take account of common factors known to influence outcome.

Conclusions. CAT appears to be a promising intervention for a range of difficulties; however, the CAT model lacks an as yet credible evidence-base, appearing to have largely bypassed the rigorous, controlled stage of the 'hourglass model'. There is a need for further research in line with established frameworks of psychotherapy development.

Practitioner Points

- CAT shows promising utility for treating complex and severe mental health difficulties.
- Co-ordinated large-scale studies should be conducted, drawing on established frameworks of evidence-base development.
- Common factors (e.g. alliance, therapist competence) should be considered in the design of future CAT studies.

Cognitive Analytic Therapy (CAT) was developed as a time-limited, integrative psychotherapy (Ryle, 1995) and has rapidly grown in popularity as a treatment for a diverse range of psychological difficulties (Denman, 2001). The approach has an emerging evidence-base (Margison, 2000) and is included in the NICE guidelines for Eating Disorders (NICE, 2004) and Borderline Personality Disorder (NICE, 2009). Yet critics have highlighted the limitations of the extant outcome evidence-base for CAT in comparison to some other psychotherapies (Llewelyn, 2003) and questioned whether the popularity of CAT in clinical practice has superceded the formation of a sufficiently credible and robust evidence-base (Marriott & Kellett, 2009). To this end, the current review aims to critique the nascent CAT outcome evidence-base, in order to appraise the range and quality of CAT evidence across diagnostic categories and suggest avenues for future research.

Developing a robust & relevant evidence-base

Effective psychological practice should be based on the “*best available research with clinical expertise in the context of patient characteristics, culture, and preferences*” (American Psychological Association, 2006, p.280). Yet determining the “*best available research*” is a complex endeavour, requiring the critical evaluation and assimilation of diverse sources of information (Barkham, Stiles, Lambert & Mellor-Clark, 2010). Numerous authors (e.g., Donnenberg, Lyons & Howard, 1999; Roth & Fonagy, 2005; Weston, Novotny & Thompson-Brenner, 2004) have commented on the tension between *efficacy* research, i.e. investigating whether “*a particular intervention has a specific, measurable effect*” (Barkham & Mellor-Clark, 2003, p.320) under controlled conditions and *effectiveness* research i.e. exploring the relevance of treatments in routine clinical practice, with

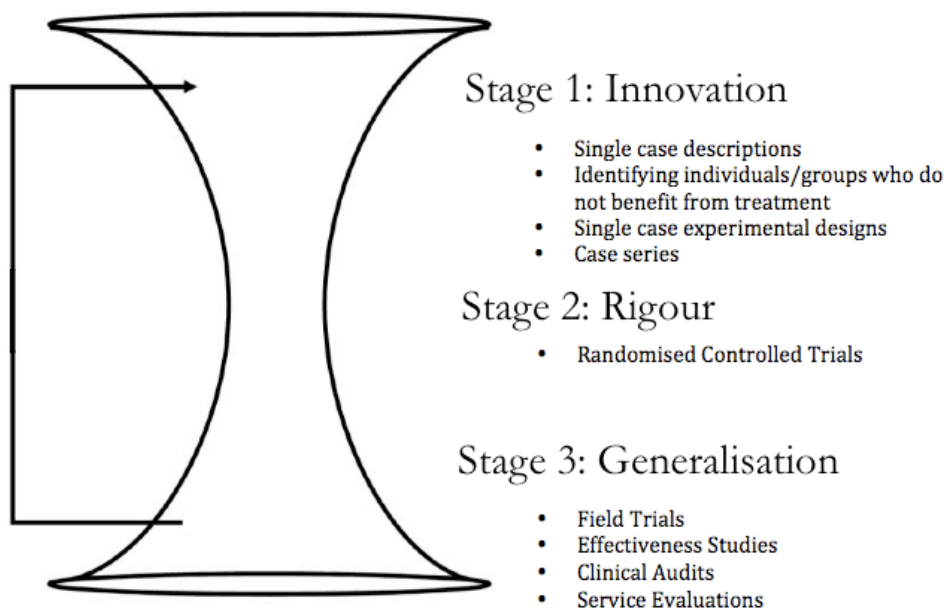
heterogeneous populations and service settings (Seligman, 1995). Traditionally, efficacy research has been associated with “*evidence-based practice*” (EBP; American Psychological Association, 2006) and effectiveness research with “*practice-based evidence*” (PBE; Barkham & Margison, 2007).

Randomised controlled trials (RCTs) prioritising internal validity and traditionally adopted in efficacy trials, have been considered the 'gold standard' for empirically supported treatments (Chambless & Ollendick, 2001), yet the appropriateness of this paradigm for psychotherapy research has been significantly challenged (e.g., Guthrie, 2000). It has been advocated that developing a robust and relevant psychotherapy knowledge-base requires both effectiveness and efficacy studies (Barkham et al., 2010; Bower & Gilbody, 2010; Gilbody & Whitty, 2002). However, coherently integrating evidence across paradigms represents a significant challenge.

Various frameworks have been presented that attempt to address this challenge (e.g. Barkham & Mellor-Clark, 2003; Parry & Richardson, 1996). A widely accepted model of linking EBP and PBE is the three-stage ‘hourglass’ model (Salkovskis, 1995; Figure 1). This model proposes a three-stage cyclical process: *stage 1*, an emerging therapy is initially tested in experimental settings on a small number of patients; *stage 2*, findings from stage 1 stimulate empirical testing in larger efficacy studies, including RCTs, with stringent criteria enhancing internal validity to isolate mechanisms of interest; *stage 3*, efficacy results are then transported into effectiveness research in clinical practice to assess the external validity of findings. The results of which stimulate further questions to be refined in small-scale experimental studies (*stage 1*); and so the process begins anew. In this way coherent research strategies are promoted and findings from efficacy and

effectiveness research complement each other to advance the development of effective psychological therapies.

Figure 1; diagrammatic representation of hourglass model adapted from Salkovskis (1995)



In addition to the tension surrounding EBP and PBE there is ongoing debate in the literature about components of psychological therapies that actually effect change. A major thrust of recent outcome research has put forward the position that outcomes are largely due to trans-theoretical “*common factors*” (Lambert, 2005), such as the therapeutic alliance (Horvath & Symonds, 1991). This is in contrast to a “*specific factors*” paradigm that maintains that targeted interventions and techniques, specific to a model, uniquely explain variance in outcome above and beyond that explained by common factors (Imel & Wampold, 2008). In line with this, different models have been developed to treat different disorders, as

reflected in published guidelines for clinical practice (e.g. NICE guidelines).

Although exploring this debate in more detail is beyond the remit of this review, it is worth bearing in mind that to date, CAT has only developed a specific targeted model to guide intervention for patients presenting with Borderline Personality Disorder; the Multiple Self-States Model (Ryle, 1997).

Cognitive Analytic Therapy: theory & practice

CAT integrates psychoanalytic and cognitive-behavioural models to offer a time-limited (usually either 16 or 24 sessions), relational approach to facilitating therapeutic change (see Kerr, 2005; Ryle & Kerr, 2002). Theoretically, CAT draws on personal construct theory (Kelly, 1956) and object relations theory (Ogden, 1983; Ryle, 1985), asserting that mental representations of self, others and the world are developmentally formed by early social, reciprocal interactions with significant others (Ryle & Kerr, 2002).

These internalised early object relations are termed '*reciprocal roles*' (RRs) and become 'psychological lenses' through which individuals anticipate and react to intra- and inter-personal relationships. Target Problem Procedures (TPPs; commonly termed 'traps', 'snags' and 'dilemmas', Ryle & Kerr, 2002) are repetitive sequences, directed towards an aim (Denman, 2001) that dictate interactions with others and patterns of self-care that confirm and perpetuate maladaptive RRs despite damaging outcomes.

In practice, CAT focuses on "3 R's": '*reformulation*', '*recognition*' and '*revision*'. Early assessment sessions prioritise active and collaborative reformulation to develop a shared understanding of the developmental origins and perpetuating factors for patterns of psychological difficulties and distress (Ryle,

1995). This is shared with the patient via a reformulation letter that is followed in subsequent sessions with a sequential diagrammatic reformulation (SDR; Ryle & Kerr, 2002). These two reformulation activities form the foundation for patient self-monitoring and 'recognition' of when they are in (or about the enter) the RR and TPPs identified in the SDR. Change in CAT arises from experiencing a non-collusive therapeutic relationship and raising self-awareness of maladaptive patterns to actively 'revise' and 'exit' TPPs and develop 'healthier' RRs. 'Exits' in CAT may be multi-modal (e.g. exposure, cognitive challenge, compassion, etc.) as indicated by patients' presenting difficulties. CAT also emphasises the therapeutic value of endings, and therapist and patient exchange 'goodbye letters' (e.g. Rayner, 2011) in the final session. Follow-up sessions are negotiated between client and therapist, but usually range from one session at three-month follow-up to four sessions offered over a six-month period.

Method

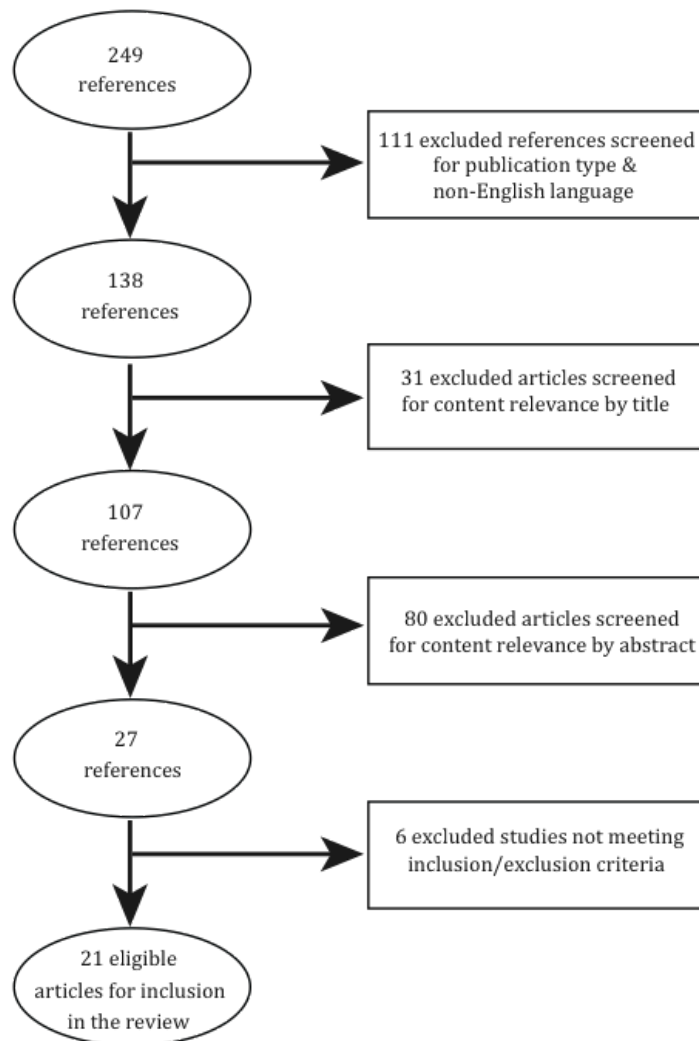
A literature search was conducted to identify CAT outcome research published between 1960 and 2011 in order to capture any outcome literature reflecting precursors to CAT in its modern form. Studies were identified by searching PsycInfo, Medline, CINAHL and the Cochrane library using the search term "*cognitive analytic**" (see Appendix iii for example output of search term).

Although the search consisted of only a single search term, this captured literature where at least one of the terms was present in the title/abstract/article (e.g. the search term captured papers containing "cognitive-analytic therapy"). In addition, active CAT researchers were contacted and asked if they were aware of any CAT research in press pertinent to the aims of this review. These searches generated 249 papers (excluding duplicates).

Inclusion/Exclusion criteria

Studies were included based on the following criteria: 1) CAT delivered as an intervention, either in individual or group format, 2) use of psychometrically sound outcome measures, 3) at least pre-post scores available, 4) written in English, 5) accepted for publication in a peer-reviewed journal and 6) reporting independent datasets (see Figure 2).

Figure 2; *Diagram of search strategy*



From the 249 papers, nine were excluded as they were not written in the English language, 93 because they were books or book reviews, four because they were unpublished theses and five Cochrane protocols. The title and abstracts of the remaining 138 papers were examined and a further 30 papers excluded from the review on the basis that CAT was not described. One paper was a correction and appended to the relevant study. The remaining 107 papers were retrieved for a more detailed evaluation. A further 80 papers were excluded on the basis that they were theoretical comments rather than outcome research, outcome was construed more broadly than quantitative change on standardised psychometric measures (e.g. engagement rates, Ougrin, Ng& Low, 2008), or studies reported process issues, or employed qualitative methodologies. Twenty-seven CAT outcome studies reported collecting pre-post outcomes on validated measures, however the authors of four papers¹ did not respond to requests for the outcome data alluded to in the paper. A paper detailing a case study included in another paper², and data from an RCT duplicated in a subsequent study³ were excluded from the review. Therefore, a total of 21 papers were eligible for inclusion.

Eligible papers were evaluated using an extant checklist (Downs & Black, 1998; see Appendix iv for copy), developed for rating the methodological quality of randomised and non-randomised healthcare outcome studies. The checklist provides an overall quality score (range 0-32) and subscale scores evaluating i) whether sufficient information was provided to allow the reader to make an unbiased assessment of the findings (*reporting subscale*; range 0-11), ii) generalisability of findings (*external validity subscale*; range 0-3), iii) what

¹ Bennett, Parry, & Ryle, 2006; Kerr, 2001; Sochos, 2005; Withers, 2008

² Bennett & Parry, 1998

³ Chanen et al. 2008

measurement or intervention biases may have confounded findings (*internal reliability subscale*; range 0-7) and iv) which selection biases could have confounded findings (*internal validity subscale*; range 0-6). One further question relates to the sample size and thus the *power* (range 0-5) that the study had to detect a 'true' result. The checklist has been shown to have good psychometric properties. Downs and Black (1998) report a mean score of 14 (SD=6.39) for controlled studies and 11.7 (SD=4.64) for non-randomised studies.

A Clinical Psychologist independent of this study rated three papers. An intraclass correlation coefficient of 0.98 (95% C.I.=0.67 to 1.00) demonstrated excellent inter-rater reliability between raters' total quality rating scores (Field, 2005).

Data Analysis

Where study data permitted, a measure of uncontrolled effect size was calculated by dividing the mean change score achieved pre/post-CAT by the mean pre-CAT standard deviation (Barkham, Gilbert, Connell, Marshall & Twigg, 2005; Westbrook & Kirk, 2005). For studies reporting multiple outcomes, the analysis utilised the Brief Symptom Inventory (BSI; Derogatis & Melisaratos, 1983) or Symptom Checklist 90 Revised (SCL-90-R; Derogatis, Richels & Rock, 1976), as this was the most consistent measure employed across studies. If a measure of global functioning was not reported, the author selected the most widely used and validated measure. It was possible to calculate effect sizes in this way for nine CAT outcome studies.

Results

The 21 studies that met the inclusion criteria for the review are summarised in Table 1, which includes total quality rating scores. Studies are grouped according to methodology consistent with each stage of the 'hourglass' model (Salkovskis, 1995; see Figure 1).

Table 1; CAT outcome studies grouped by methodological approach consistent with stages of the 'hourglass' model (Salkovskis, 1995)

Author(s)	Number of sessions (Completers sample size)	Sample	Comparison Condition (Completers sample size)	Design	Standardised outcome measures	Score ^a
STAGE 1						
Case Description						
Ryle & Beard (1993)	26-sessions (N=1)	Borderline Personality Disorder Adult		Uncontrolled (pre-post)	IIP-127 ⁴ SCL-90R ⁵ DES ⁶	10 [†]
Bennett (1994)	16-sessions (N=1)	Depression, Anxiety Adult		Uncontrolled (pre-post)	BDI-II ⁷ SCL 90R IIP-127	6 [†]
Graham & Thavasothy (1995)	5-sessions (N=1)	Dissociative psychosis Adult		Uncontrolled (pre-post)	DES	6 [†] 4 [‡]

^a Total Downs and Black (1998) quality rating score

[†] Author's rating score; [‡] Independent rater's score

⁴ Inventory of interpersonal problems-127 (Horowitz, Rosenberg, Baer, Ureno, & Villesenor, 1988)

⁵ Symptom Checklist 90 Revised (Derogatis, Richels, & Rock, 1976)

⁶ Dissociative Experiences Scale (Berstein & Putnam, 1986)

⁷ Beck Depression Inventory-II (Beck, Steer, & Brown, 1995)

Table 1 continued

Yeates et al. (2008)	11-sessions (N=1)	Acquired Brain Injury Adult	Uncontrolled (pre-post)	BAI ⁸ HADS ⁹ STAXI-2 ¹⁰ DAS ¹¹	12 [†]
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Single Case Experimental Designs (SCED)

Kellett (2005)	24-sessions (N=1)	Dissociative Identity Disorder Adult	SCED	BSI ¹² BDI-II IIP-32 ¹³ DES-II ¹⁴ SSD ¹⁵ PSQ ¹⁶	13 [†]
Kellett (2007)	24-sessions (N=1)	Histrionic Personality Disorder Adult	SCED	BSI BDI IIP-32 PSQ YSQ-SV ¹⁷	15 [†]

⁸ Beck Anxiety Inventory (Beck, Epstein, Brown, & Steer, 1988)

⁹ Hospital Anxiety and Depression Scale (Zigmond & Snaith, 1983)

¹⁰ State-Trait Anger Expression Inventory (Spielberger, 1999)

¹¹ Dyadic Adjustment Scale (Spanier, 1976)

¹² Brief Symptom Inventory (Derogatis, 1993)

¹³ Inventory of Interpersonal Problems-32 (Barkham, Hardy, & Startup, 1996)

¹⁴ Dissociative Experiences Scale (Carlson & Putnam, 1993)

¹⁵ State Scale of Dissociation (Krüger & Mace, 2002)

¹⁶ Personality Structure Questionnaire (Pollock, Broadbent, Clarke, Dorrian, & Ryle, 2001)

¹⁷ Young Schema Questionnaire-Short Version (Young, 1998)

Table 1 continued

Kellett and Totterdell (in press)	16-sessions (N=1)	Morbid Jealousy Adult	16 sessions CBT (N=1)	SCED	BSI BDI-II IIP-32 RJQ ¹⁸ PJQ ¹⁹	13 [†]
Kellett (in press)	24-sessions (N=1)	Paranoid Personality Disorder Adult		Hermeneutic SCED	BSI BDI-II IIP-32 PSQ	16 [†]

STAGE 2**Randomised Controlled Trials (RCT)**

Treasure et al. (1995)	20-sessions (N=10); Adult,	Anorexia Nervosa or Bulimia Nervosa	20 sessions Educational Behavioural Treatment (n=10)	RCT	MRS ²⁰	20 [†]
Fosbury, Bosley, Ryle, Sonksen & Judd (1997)	16-sessions (n=10); Adult,	Diabetes Adult	Diabetes Specialist Nurse Education (DSNE) 14-18 sessions (n=16)	RCT	IIP-127	23 [†]

¹⁸ Romantic Jealousy Questionnaire (Pines, 1992)¹⁹ Prestwich Jealousy Scale (Beckett, Tarrier, Intili & Beech, 1992)²⁰ Morgan and Russell Scale (Morgan & Russell, 1975)

Table 1 continued

Dare, Eisler, Russell, Treasure & Dodge (2001)	13-sessions (n=22)	Anorexia Nervosa or Anorexia Nervosa/Bulimia Nervosa Adult	25-sessions Focal Psychodynamic Psychotherapy (n=21) 14-sessions Family Therapy (n=22) 11-sessions 'Routine' treatment as usual (n=19)	RCT	MRS	20+ 21+
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STAGE 3

Quasi-Experimental study

Chanen et al. (2009)	13-sessions (n=41) Adolescents (aged 15-18);	Borderline Personality Disorder Adolescents (aged 15-18)	11-sessions, Good Clinical Care (n=37) 15-sessions Historical Treatment as Usual (n=32)	Quasi-experimental design	YSR ²¹ /YASR SOFAS ²²	24+
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Table 1 continued

Practice-based studies of effectiveness

Brockman, Poynton, Ryle & Watson (1987)	12 sessions (N=30)	Depression, Anxiety Adult	12-sessions Interpretive Therapy (n=18)	Uncontrolled	BDI ²³ GHQ-60 ²⁴ CCI ²⁵	17 [†]
Duignan & Mitzman (1994)	4 individual sessions and 12-sessions Group CAT (n=7)	Personality Disorders, Depression, Panic Disorder, Psychosis Adult	12-sessions individual CAT (n=30; Brockman et al., 1987)	Uncontrolled	BDI GHQ-60 CCI	17 [†]
Clarke & Llewelyn (1994)	16-sessions (N=6)	Female Survivors of Childhood Sexual Abuse Adult		Uncontrolled, pre/post	SCL90-R JBI ²⁶ BDI-II RSES ²⁷	15 [†]
Clarke & Pearson (2000)	16-sessions (N=4)	Male Survivors of Childhood Sexual Abuse (BPD & NPD ²⁸) Adult		Uncontrolled, pre/post	SCL90-R JBI BDI-II RSES	15 [†]

²¹ Young Adult Self-Report (Achenbach, 1997)

²² Social and Occupational Functioning Assessment Scale (Goldman, Skodol, & Lave, 1992)

²³ Beck Depression Inventory (Beck, Ward, Mendelson, Mock, & Erdaugh, 1961)

²⁴ General Health Questionnaire-60 (Goldberg, 1972)

²⁵ Crown Crisp Inventory (Crown & Crisp, 1979)

²⁶ Jehu Belief Inventory (Jehu, 1988)

²⁷ Rosenberg Self Esteem Scale (Rosenberg, 1989)

²⁸ Narcissistic Personality Disorder

Table 1 continued

Ryle & Golynkina (2000)	24-sessions (N=27)	Borderline Personality Disorder Adult		Uncontrolled, pre-post	BDI SCL90-R IIP-127 SQ ²⁹	19 [†]
Wildgoose, Clarke & Waller (2001)	16-sessions (N=5)	Borderline Personality Disorder Adult		Uncontrolled, pre-post	PSQ DIS-Q ³⁰ SCL-90-R IIP-127	18 [†]
Birtchnell, Denman & Okhai (2004)	16-sessions (N=32)	Not reported. (classified as common mental health difficulties) Adult		Uncontrolled, pre-post	PROQ2 ³¹ CORE ³²	11 [†]
Mace, Beeken & Embleton (2006)	16-sessions (N=17)	Personality Disorders, Affective Disorders Adult	Brief Psychodynamic Therapy (n=17)	Uncontrolled, pre-post	GHQ-12 ³³	13 [†] 11 [‡]
Marriott & Kellett (2009)	Short-term CAT 16-sessions (n=38) Medium-term CAT 24-sessions (n=27)	Depression, Anxiety, Phobias, Obsessive Compulsive Disorder, Post Traumatic Stress Disorder, Personality Disorders Adult	Short-term CBT 16-sessions (n=38) Medium-term CBT 20-sessions (n=27)	Uncontrolled, pre-post	BSI BDI-II IIP-32	17 [†]

²⁹ Social Questionnaire (Corney, Clare, & Fry, 1982)

³⁰ Dissociation Questionnaire (Vanderlinden, Dyck, Vandereycken, Vertommen, & Verkes, 1993)

³¹ Person's Relating to Others Questionnaire (Birtchnell & Evans, 2004)

³² Clinical Outcomes in Routine Evaluation (Evans et al., 2002)

³³ General Health Questionnaire-12 (Goldberg & Williams, 1988)

Table 1 continued

Short-term PCT
9-sessions
(n=38)

Medium-term PCT
17-sessions (n=25)

Of the 21 identified studies, the majority (95%, n=20) were conducted in the United Kingdom and were predominated by small *n*, uncontrolled PBE methodologies. There have been three RCTs, four SCED and one quasi-experimental study comparing results from a RCT with treatment as usual.

'Personality Disorder' is by far the most common condition, being the primary recorded diagnosis in eight outcome studies (38%). The study by Birtchnell, Denman and Okhai (2004) did not record any diagnostic characteristics of the sample. As the study was conducted in a psychotherapy department in routine practice it was classified as treating common mental health difficulties.

Quality appraisal of included studies

All CAT RCTs (M=21, range 20-23) scored above the mean quality rating for controlled trials (Downs & Black, 1998; M=14, SD=6.39). On average, case descriptions (M=8, range 4-12) scored below the mean rating for non-randomised studies (Downs & Black, 1998; M=11.7, SD=4.64). All CAT SCED studies (M=14, range 12-16) and effectiveness studies reporting outcomes from routine clinical practice (M=16; range 11-19) scored above average for non-randomised designs, suggesting that overall the published CAT evidence to date is of relatively high quality.

Table 1 suggests that there has not been coherent, chronological progression of CAT outcome research through the 'hourglass' framework (Salkovskis, 1995). This review, therefore, now goes on to evaluate the quality and range of studies contributing to the cumulative evidence-base for CAT as an effective intervention

according to diagnostic groupings. Mean sub-scale and total score for studies clustered according to treatment focus are presented in Appendix v.

Personality Disorder (PD)

The range and quality of outcome evidence for CAT and PD varies considerably. Overall, a lack of controlled trials severely limits the internal validity of evidence for CAT and PD. Furthermore, it is difficult to assess the external validity of findings due to generally inadequate reporting of patient and service characteristics. The vast majority of outcome evidence for CAT comprises of studies of patients diagnosed with Borderline Personality Disorder (BPD), with two SCEDs exploring the utility of CAT with other PD presentations.

Borderline Personality Disorder (BPD)

The published data consistently suggests that CAT can produce good outcomes for patients with BPD. An early, uncontrolled case description provided preliminary evidence that 26-sessions of CAT can be effective for BPD in routine clinical practice (Ryle and Beard, 1993), and further small-scale, uncontrolled studies substantiate this finding (Duignan & Mitzman, 1994; Ryle and Golyenkina's, 2000; Wildgoose et al., 2001).

Ryle and Golyenkina's (2000) study demonstrated that following 24-sessions of CAT, 52% (n=14) of BPD patients 'improved', 22% (n=6) achieved some level of change, and 26% (n=7) did not demonstrate clinical change. However, it is difficult to attribute any change directly to CAT, as outcome measures were only completed prior to assessment and at 6-month follow-up. Furthermore, promising outcomes may be due to other factors not adequately measured, such as therapist effects (Crits-Christoph, Baranackie, Kurcias & Beck, 1991). The allocation process is

poorly detailed; more severe cases may have been allocated to the more experienced clinicians. Although the study attempted to evaluate the quality of CAT delivered, a more robust method to measure therapists' competence would have enhanced the methodology (e.g. Bennett & Parry, 2004). The study did, however, explore factors associated with outcome and found that poorer outcome in CAT was associated with severity of BPD symptoms, unemployment, alcohol misuse and self-injurious behaviour. At 18-months follow-up, two-thirds of the sample completed outcome measures, indicating some evidence of continuing gains in this selected group.

A further small-scale naturalistic study evaluating the effectiveness of 16-sessions of CAT on dissociation, personality fragmentation, global distress and interpersonal functioning (Wildgoose et al, 2001) revealed that at 9-month follow-up all participants (n=5) demonstrated a reduction in severity of BPD, to the extent that four patients were considered 'recovered'. Although three patients achieved some measure of clinically significant change (Jacobson & Truax, 1991), one patient demonstrated statistically significant deterioration on outcome measures and another's scores appeared unaffected by CAT. No clear factors were isolated to explain the differences in outcome, although Wildgoose et al. (2001) considered that the severity of patients' difficulties and brevity of the intervention limited the potential for change.

In a further uncontrolled study combining individual and group CAT, Duignan and Mitzman (1994) reported promising outcomes. Statistically significant change across a range of outcome measures was observed between start of intervention and 1-month follow-up for all patients completing CAT (n=7), although no rates of reliable and clinically significant change were reported. The authors conclude that a combination of individual and brief group CAT in routine

practice can be an effective treatment for those experiencing severe disturbances. The study has high external validity, but it is difficult to extrapolate the unique effect of either individual or group intervention from this sample. Furthermore, participants were selected on their willingness to undertake group therapy, which may have feasibly influenced clinical outcomes and certainly introduces selection bias.

To date, the highest quality evidence for CAT and BPD comes from an RCT conducted by Chanen et al. (2008). The authors compared CAT (n=41) with manualised 'good clinical care' (n=37), a specially designed intervention informed by problem-solving principles and Cognitive-Behavioural Therapy (CBT) for adolescents experiencing prodromal BPD. Patients were randomised to intervention and completed various outcome measures at baseline, 6, 12 and 24-month follow-up, with 92% (n=72) of participants completing outcomes for at least three time-points. The authors conclude that "both CAT and GCC are effective", (Chanen et al., 2008, p.477), although it is difficult to attribute change to either intervention as they were delivered as an adjunct to the comprehensive treatment package offered by a reconfigured specialist service. Therefore, Chanen et al. (2009) expanded on these findings in a quasi-experimental study, comparing outcomes for the CAT and GCC samples with adolescents who received 'Historical Treatment As Usual' (H-TAU, n=32), prior to service reconfiguration. Results at 2-year follow-up indicate that all three interventions were successful in reducing BPD symptomology. CAT had the most marked improvement across all outcomes, producing the highest improvement in externalising difficulties and parasuicidal behaviour. CAT also produced a faster rate of improvement in internalising and externalising difficulties, although the upper bound of the 95% confidence interval suggests that GCC may achieve a similar rate of change. There was no difference

between conditions in the rate of improvement observed in borderline psychopathology or parasuicidal behaviour, nor the frequency of access to service during treatment. Findings have strong external validity as minimal exclusion criteria were applied and treatments represent clinically representative interventions. Contemporaneously randomising participants to a 'treatment-as-usual' condition and randomly allocating patients to therapists would have enhanced the validity of findings.

An earlier controlled study compared CAT with brief psychodynamic therapy (BPT) delivered by clinicians in training in routine clinical practice (Mace et al., 2006). Patients (n=17) were allocated to treatment condition following independent assessment and matched on diagnosis, duration of difficulties, disability and experience of previous therapy. Again, findings suggest that both CAT and BPT produced similar statistically significant improvements in mental health. The validity of attributing change to the interventions was limited, as measures were only taken at assessment and 3 month follow-up. Clinically significant change rates were reported, suggesting that BPT appeared to produce better outcome; only 6 CAT patients achieved clinically significant improvement compared with 13 of the BPT patients. However, valid comparison between CAT and BPT are limited given that the CAT group reported significantly higher scores on the GHQ-12 at assessment. Twice as many patients allocated to CAT were diagnosed with PD and the 'dose' of therapy in each condition was not clear.

Both controlled studies attempted to control for therapist effects as therapists delivered both interventions, yet only Chanen et al. (2008) independently evaluated therapist competency, fidelity to the model and treatment integrity. Neither controlled study, however, reported rates of reliable

change (Jacobson and Truax, 1991), so findings have not taken into account inherent measurement error.

Generally, although the accumulated evidence suggests promising utility of CAT for BPD, most CAT outcome studies for BPD suffer from small sample sizes, thus limiting the generalisability of findings. Papers are characterised by insufficient detail of patient or service setting characteristics, making it difficult to determine the representativeness of the samples or treatment, and therefore the external validity of findings. Only two studies have compared CAT with another active treatment, which both reported a lack of superiority of CAT for BPD, although the study quality makes it difficult to draw valid conclusions between treatments. Clearly, further controlled trials ensuring valid baseline comparisons and randomisation of patients to treatment group and therapist are required, without which it is difficult to say with any certainty just how effective CAT is for BPD. It is of note that the progression of empirical research regarding CAT for BPD appears mutually exclusive, with no crossover between studies, comprehensively failing to follow the hourglass model (Salkovskis, 1995), previously outlined. This review suggests that interdependence between future efficacy and effectiveness studies for CAT and BPD should be encouraged (Barkham & Mellor-Clark, 2003) and outcome evidence should continue to explore for which BPD patients CAT works, and why.

Histrionic Personality Disorder (HPD)

Kellett (2007) utilised SCED methodology with a patient diagnosed with HPD, who completed psychometric outcomes at assessment, end of CAT and follow-up. Idiosyncratic experimental variables, constructed in line with DSM-IV diagnostic criteria (American Psychological Association, 2000) for HPD, were continually

measured during baseline (21 days), treatment (182 days) and follow-up (154 days). Change in experimental measures demonstrated that CAT had a significant improvement on all variables, compared with baseline scores. The results suggest more than a 40% reduction in the majority of histrionic tendencies during intervention. Positive identity formation was maintained over follow-up, although significant deterioration was observed in the patient's focus on physical appearance at the point of termination. Clinically significant improvement was observed on the BDI, BSI and the PSQ on pre-post comparisons. Longer-term follow-up would have more effectively evaluated the stability of apparent change over time.

Paranoid Personality Disorder (PPD)

Kellett (in press) reported the efficacy of CAT in a hermeneutic SCED (HSCED) with a patient diagnosed with PPD and depression. The patient completed daily ratings of six target variables throughout baseline (42 days), intervention (161 days) and 4-session follow-up (140 days). Summed together, these experimental variables yielded a general paranoia score. Standardised psychometric outcome measures were administered at assessment, termination and final follow-up, although no standardised measure of paranoia was administered. Results indicate a significant reduction in target ratings of "suspiciousness" and "anxiety", and a significant increase in problem-solving ability. Reliable improvement was observed across the majority of measures, but no reliable change was observed in interpersonal functioning. Change was maintained over follow-up, and qualitative evidence for change being attributed to CAT came from the 'Change Interview' (Elliott, 2002) conducted by an independent researcher following the final follow-up session.

These SCED studies suggest that CAT may be an effective intervention for treating less prevalent PD presentations. However, a criticism of both studies is that the baseline was the assessment/reformulation phase and therefore did not constitute a technically neutral baseline. Unique therapist and patient characteristics in such studies also limit the extent to which findings can be generalised. Further evidence is required to build a robust and relevant evidence-base for CAT with other PD presentations.

Anxiety and Depression

There appears to be a paucity of evidence for CAT with common mental health problems compared with other psychological approaches (e.g. Cognitive Behavioural Therapy; NICE, 2004), despite the impetus to evaluate and deliver non-CBT therapies for anxiety and depression (Care Services Improvement Partnership Choice & Access Team, 2008; NICE, 2009; DoH, 2011). Overall, CAT outcome studies for affective disorders displayed poor internal validity, reflecting the paucity of controlled studies with this clinical population. The external validity of findings was comprised by the generally inadequate standards of reporting patient and therapist characteristics.

Bennett's (1994) case description indicated that 16-session CAT had a positive impact on depression scores, global functioning and interpersonal difficulties, with change maintained at 3-month follow-up. Yet, case descriptions suffer from inherent methodological limitations, e.g., a lack of controlled conditions limit the explanatory relationships that can be inferred, and n=1 sample size severely restrict the generalisability of findings.

Better evidence accrues from Brockman et al.'s (1987) study, comparing CAT with 'interpretative therapy' (INT; Mann & Goldman, 1982) in routine clinical

practice. Both therapies were delivered in an outpatient setting by 15 trainee therapists from varying professional backgrounds, none of whom had received formal training in either approach. A total of 48 participants were randomly allocated to each condition (CAT, n=30; INT, n=18) and completed therapy, with more complex cases being allocated to the more experienced clinicians. CAT and INT both produced improvement in depression and general mental health scores, with CAT patients also reporting significant improvement in positive self-attitude. There was no difference between conditions in patients' subjective ratings of change experienced during therapy. Of those returning measures at approximately 1-year follow-up (62%), change appears to have been maintained. It is difficult to make comparisons between treatments since the CAT condition demonstrated a higher level of distress at baseline, and this was not taken into account in the subsequent analysis. No measure of adherence to each model was employed and as the majority of therapists delivered both interventions, it is possible that there was cross-contamination between conditions. Furthermore, bias was introduced as an experienced CAT practitioner provided weekly group supervision for all therapists in both models. Neither therapy was manualised, which restricts internal validity and the extent to which valid attributions of change can be inferred.

Marriott and Kellett (2009) provide the strongest evidence for CAT with common mental health difficulties. The study benchmarked short-term (median sessions=16; n=38) and medium-term (median sessions=24, n=27) CAT against short and medium-term CBT and 'person centred therapy' (PCT) in routine clinical practice. Despite more distressed patients being allocated to CAT, participants were matched across modalities on a score of global distress at intake and number of sessions of therapy completed. Results indicate that all modalities were effective in improving psychological wellbeing across a range of measures, with comparable

effect sizes to those demonstrated in efficacy studies. The findings suggest that in short-term therapy, the rates of clients achieving recovery on a measure of global distress were significantly higher in the CBT service. However, in medium-term therapy, no differences in rates of clinically significant change were observed between CAT, CBT and PCT. In the CAT service, those patients receiving more than 16 sessions were more likely to achieve recovery than those receiving short-term CAT.

Marriott & Kellett's (2009) study, however, suffers from methodological limitations inherent within practice-based evidence; no randomisation to modality or therapist, reliance on self-report measures, and no measure of therapist competence, diagnostic validity or adherence to the model. Arguably, however, methodological requirements such as these are necessities derived from efficacy trials and have less value in PBE where the key aim is to evaluate the utility of psychotherapy in clinical reality (Stiles, Barkham, Mellor-Clark & Connell, 2008; Stiles, Barkham, Twigg, Mellor-Clark & Cooper, 2006).

In another PBE study, 16-session CAT was shown to have a significant improvement on two measures of interpersonal functioning (Birtchnell et al., 2004). Change was maintained at 3-month follow-up. However, the quality of the paper was poor, failing to report patient characteristics and validity of the primary outcome measure, making it impossible to draw plausible and reliable conclusions.

The lack of evidence for CAT with affective disorders may not be surprising given that CAT practitioners commonly treat those patients experiencing more complex and severe difficulties (Denman, 2001). Indeed, it is of note that three studies included in the review found that people presenting with co-morbid complex difficulties were more likely to be independently allocated to CAT (Brockman et al., 1987; Mace et al., 2006; Marriott & Kellett, 2009). When

controlling for this, 24 sessions of CAT appears to be as effective as CBT for depression and anxiety (Marriott & Kellett, 2009), although short-term CBT was shown to be superior to short-term CAT. The evidence to date suggests that CAT shows promise for treating affective disorders, especially with co-morbid presentations. It therefore appears timely for efficacy trials to build on findings from initial PBE, in line with the hourglass model (Salkovskis, 1995). This would hopefully elucidate the CAT-specific mechanisms of change to guide future outcomes research and develop effective treatment strategies.

Eating Disorders (ED)

The two papers for CAT with adult patients with Anorexia Nervosa (AN) provide the highest quality evidence for a diagnostic condition (see Appendix v). The quality rating scores reflect the relatively high level of internal validity in each paper. As yet no effectiveness studies have been conducted exploring outcomes of CAT in routine practice with ED populations.

Treasure et al. (1995) compared manualised CAT (n=14) with 'educational behavioural treatment' (EBT; n=16). Trained therapists with several years of clinical experience of treating ED delivered both interventions. No significant differences in outcomes were observed between the treatments at 1-year follow-up; both CAT and EBT resulted in an average weight gain of 6.8kg with 30% of those completing therapy maintaining weight gain. Poorer outcome was predicted by greater proportion of pre-treatment weight loss. Treasure et al., (1995) concluded that both outpatient CAT and EBT can be effective for adult onset AN, although outcomes may be explained by other factors not measured in the study, e.g. therapist effects, alliance.

Dare, Eisler, Russell, Treasure and Dodge (2001) conducted a RCT and found that 'focal psychoanalytic psychotherapy' (FPP) and 'family therapy' (FT) were significantly superior to routine treatment as usual (TAU). CAT also demonstrated benefits although the difference did not reach statistical significance. Randomisation controlled for potential confounding variables; age of onset, duration of difficulties, co-morbid bulimic symptoms and marital status. Of the original sample (n=84), 64% completed treatment, with significantly more non-completers in the TAU condition. No significant differences in engagement rates were found between conditions. Results at 1-year follow-up showed modest overall improvement across a range of measures, but specific outcomes are not reported between treatment conditions. Controlling for initial weight, active treatment appeared to significantly improve weight gain, as approximately a third of participants no longer met diagnostic criteria at 1-year follow-up, compared with 5% of the TAU sample. Intention-to-treat analysis revealed that 32% (n=7) of the CAT participants achieved a good outcome, compared with 52% (n=11) of those in FPP and 41% (n=9) in FT. Unlike Treasure et al. (1995), therapy was delivered by therapists with varying experiences, which may explain some of the difference between treatment outcomes (Morris, 2001). Four therapists supervised by an experienced CAT clinician delivered CAT, whereas experienced therapists delivered FT and FPP. Bell (2001) also notes that the duration of treatment was not consistent between conditions. This may explain some variance in outcome between CAT, FPP and FT as some research indicates that dose of therapy influences outcome (Hansen, Lambert & Forman, 2002).

CAT appears a promising intervention for adults with AN, being cited in the NICE guidelines for eating disorders (NICE, 2004). Findings suggest that CAT is superior to TAU and achieves comparable results with EBT at 1-year follow-up, yet

FPP and FT appear to achieve better outcomes. These results, however, should be interpreted with caution given the studies limitations. Assessing therapists' competence and fidelity to the CAT model would have enhanced the quality of both studies. Furthermore, the studies suffer from small sample sizes, which may have restricted the power to differentiate between treatments. Longitudinal follow-up is indicated and future studies of CAT with ED populations would benefit from using standardised outcome measures, and reporting rates of reliable and clinically significant change, to enable benchmarking with the wider extant CAT literature.

Survivors of Childhood Sexual Abuse (CSA)

Two small scale uncontrolled effectiveness studies report outcomes of 16-session CAT for female (Clarke & Llewelyn, 1994; n=6) and male survivors (Clarke & Pearson, 2000; n=4) of CSA. Both studies are of comparable quality, suffering from low internal validity, given that they report routine clinical outcomes. However, the studies had the highest internal reliability with outcomes collected at the same time-points and clearly reported attrition rates. The studies also had the highest external validity subscale score (M=2, 67%; Appendix v) across conditions, providing important preliminary evidence that CAT may be effective in routine practice for survivors of CSA demonstrating severe levels of disturbance.

Clarke and Llewelyn (1994) collected self-reported outcomes at pre- and post-therapy, and at 3-month follow-up. Following CAT, five patients (83%) demonstrated reliable improvement in global functioning, although only two scored below the clinical cut-off at termination. Scores also indicated improvements in depression, self-esteem and reduced self-blaming beliefs and self-harming behaviour. Broadly, change appeared maintained over 3-month

follow-up, although there was some indication of mood relapse. Whilst symptomatic relief was observed, repertory grid methodology demonstrated little change in how the female sample construed relationships with men. As Ryle (1999) notes, the use of complementary methodology mitigated over-estimation of the effectiveness of CAT observed on the standardised outcome measures and could be a strategy implemented in future CAT research.

Clarke and Pearson (2000) replicated the study with male survivors of CSA. Participants all reported difficulties consistent with a PD diagnosis. Similarly to female survivors, all male survivors demonstrated a reduction in self-blaming beliefs about their early abuse experiences and reduced depression scores. However, one participant reported deterioration in self-esteem following CAT. Overall, levels of global distress improved, but two participants with BPD reported increased psychological distress following termination of therapy. The authors conclude that the results corroborate previous findings that 16-session CAT is too brief to effect change for patients with highly complex psychological difficulties (Mace et al., 2006; Wildgoose et al., 2001).

Taken together, the studies give some weight to an argument that CAT may be a useful intervention for CSA survivors, although both studies had very small sample sizes; larger scale, controlled studies are indicated. The evidence-base for CAT with CSA survivors would benefit from use of clear and established diagnostic procedures, use of randomisation, longer-term follow-up and benchmarking findings against outcomes achieved by other psychotherapeutic approaches for survivors.

Long-term physical health conditions

The long-term physical health studies had the highest mean reporting sub-scale score (M=7.50, 68%; Appendix v). However, it is difficult to draw robust conclusions on the overall quality of the outcome evidence for CAT with difficulties associated with physical health conditions given the disparate populations and heterogeneous methodologies; one study is an uncontrolled case description with poor internal validity and a total quality score of 12, whereas the other is a RCT, with a higher quality rating of 23, but with a small sample size and hence limited generalisability.

Acquired Brain Injury (ABI)

CAT, as an adjunct to concurrent cognitive rehabilitation, appeared to be effective in reliably improving a patient's levels of anxiety and anger approximately 2-years post injury, and improving relationship satisfaction (Yeates et al., 2008). No longer-term follow-up data was presented and therapist competence and model adherence was not measured. As this was an uncontrolled single case description no wider valid inferences can be drawn on the utility of CAT for patients with neurological impairment.

Diabetes

Fosbury et al. (1997) randomly allocated 32 participants with consistently poorly controlled Type I diabetes to CAT (n=15) or a 'diabetes specialist nurse education' programme (DSNE; n=17) aiming to improve participants' diabetes management. Of the original sample, 81% (n=26) completed measures at 9-month follow-up, with a greater rate of attrition in the CAT condition (33%; n=5). Those who did not complete the study were found to score significantly lower on the diabetes

knowledge questionnaire at intake. Fosbury et al. (1997) attributed the higher CAT dropout rate to the pre-contracted appointment schedule. At termination, CAT produced significant change in participants' knowledge of diabetes, whereas DSNE was shown to be effective in reducing blood glucose levels. At 9-month follow-up both conditions achieved significant improvement in glucose levels and diabetes knowledge. CAT appeared to produce longer lasting change, as those in the DSNE condition demonstrated a relapsing trend following intervention. CAT also produced significant change in interpersonal functioning at 9-month follow-up, with scores in the DSNE condition also approaching significance.

The study had a small sample size, and 18 potential participants refused to take part, thus limiting the representativeness of the research sample. The lack of a no-treatment control group means that change cannot be attributed directly to either intervention. Furthermore, controlling for the frequency of treatment between conditions would have enhanced the quality of the study. Given these limitations, there is yet to be convincing evidence that CAT is superior to an education programme in improving self-care in diabetes self-management.

Overall, CAT may be a useful intervention in physical health settings. Clearly further, replicative, controlled studies are indicated, which ideally would precede investigating the relevance of CAT in routine practice for difficulties associated with long-term physical health conditions.

Dissociative Psychosis

A case description of 5-session CAT in an inpatient setting noted a reduction in the frequency and severity of dissociative experiences associated with dissociative psychosis (Graham & Thavasothy, 1995). Scores remained within the non-clinical

range throughout 2-year follow-up. Although the descriptive data reported in this paper indicates that CAT may be a promising intervention, the treatment was poorly described and arguably comprised part of a wider inpatient treatment package. It is impossible to conclusively attribute observable change to CAT. This study achieved the lowest overall quality rating score of those included in the review (M=5, 16%; Appendix v).

Dissociative Identity Disorder (DID)

Kellett (2005) reports outcomes from a SCED with a patient diagnosed with DID. Seven idiosyncratic experimental measures rated daily throughout baseline (35 days), intervention (175 days) and follow-up (168 days) demonstrated a reduction in the intensity of state dissociation and increasing awareness of identity shifts during treatment, compared to baseline. Change was maintained over follow-up, with continued reduction in depersonalisation. The patient also completed a battery of outcome measures at assessment, end of CAT and six-month follow-up. Results indicated reliable and clinically significant improvement in global functioning, depression and personality integration between assessment and termination, with a trend indicating continuing integration of personality on the PSQ (Pollock, Broadbent, Clarke, Dorrian, & Ryle, 2001) during follow-up. However, the external reliability of findings cannot be inferred from data reported in the paper and despite the intensity of measurement of the effect of CAT, the paper achieved a low score on the internal validity subscale of the Downs and Black (1998) checklist (Appendix v), given the insufficient attention given to possible confounding variables.

Morbid Jealousy

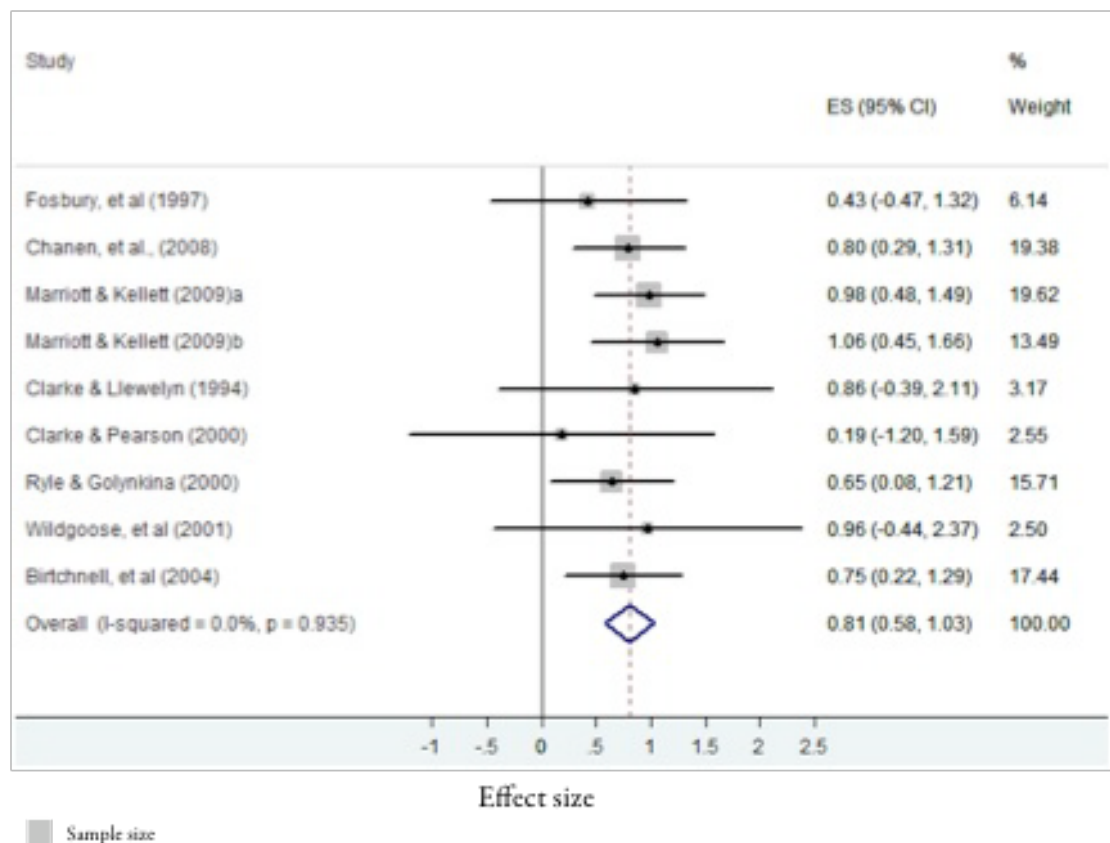
Kellett and Totterdell (in press) employed a matched SCED to treat morbid jealousy with CAT (n=1) and CBT (n=1). Patients completed five daily experimental measures of jealousy symptoms throughout baseline (CAT=35 days; CBT=44 days), intervention phase (CAT=98 days; CBT=51 days) and follow-up phases (84 days for both). Patients' partners also returned contemporaneous daily ratings of two target difficulties (level of being controlled and level of perceived jealousy). Reliable and clinically significant pre-post improvements were demonstrated in levels of global functioning, depression and interpersonal difficulties for the CAT patient. Change was maintained in the CAT condition at 3-month follow-up, compared with evidence of mood relapse in the CBT patient. CAT produced a greater change in severity of jealousy on the standardised measure, although the CAT patient had lower jealousy scores at assessment. CAT was also associated with a significant improvement in idiosyncratic measures of jealousy, hypervigilance, anxiety and self-esteem. However there was a trend for improvement during baseline, reducing the confidence with which results can be attributed to CAT. The partner of the CAT patient did not report any significant improvements in the patient's jealous feelings or controlling behaviours.

Although the 'dose' of therapy was controlled for, the frequency of sessions was not. Whilst the same therapist delivered both interventions, thus controlling for therapist effects, no measure of adherence to each model was employed and cross-contamination may have occurred. Such limitations impact on the internal validity of findings but represent the reality of conducting practice-based SCED research.

Benchmarking

Due to the large number of $n=1$ studies and poor quality reporting of raw outcomes, uncontrolled effect sizes could only be calculated for nine studies (41%) across a range of diagnostic conditions. Figure 3 demonstrates that overall, CAT generated a large uncontrolled effect size of 0.81 (Cohen, 1988), suggesting that CAT is an effective intervention. However, the confidence intervals suggest that in four outcome studies, CAT produced detrimental effects. These four studies all had very small samples (contributing between 2.50 and 6.14% to the overall uncontrolled effect size), which result in broader confidence intervals (Lueger & Barkham, 2011).

Figure 3; forest plot demonstrating uncontrolled effect sizes for CAT outcome studies³⁴



³⁴ ES=Effect size and 95% confidence interval; % weight=sample size determines the weighting of each study towards the overall effect size

Discussion

CAT was initially conceptualised as a researchable therapy, relevant to the public sector (Ryle, 1995), and has increasingly been taken-up in clinical practice (Margison, 2000). Despite the growing evidence-base (Denman, 2001), critics have highlighted the paucity of CAT outcome research in comparison with other psychotherapies (Llewelyn, 2003; Marriott & Kellett, 2009). Evidence is slowly accumulating regarding the promising utility of CAT across diverse presenting difficulties, with CAT being cited in the NICE guidelines for Eating Disorders (NICE, 2004) and BPD (NICE, 2009). This review found that despite a promisingly large uncontrolled effect size of 0.81 attained across outcome studies and diagnostic categories, in no sole diagnostic group is the quality of evidence for CAT either compellingly or convincing. The nascent evidence-base is plagued by mutually exclusive, heterogeneous methodologies that lack sufficient rigour and are of largely unknown generalisability, thus making it difficult to say with any certainty just how effective CAT is.

Overall, the review found insufficient controlled empirical investigations and an associated general lack of internal validity. The predomination of practice-based study designs makes it very difficult to infer any more than tentative causal relationships between CAT and reported change in outcomes. The strongest evidence to date comes from a study comparing CAT with 'good clinical care' for emerging BPD in adolescence (Chanen et al., 2008; 2009). The study incorporated randomisation and manualised interventions, and attempted to control for therapist effects, whilst delivering interventions to a clinically representative population. Even so, the study design makes it difficult to establish the unique

effect of CAT above and beyond general therapeutic contact with a contemporaneous specialist service.

The few controlled trials to date comparing CAT with other treatments demonstrated a lack of superiority for CAT. The evidence suggests that CAT is as effective as ‘good clinical care’ for prodromal BPD (Chanen et al., 2008); short-term CAT is inferior to CBT for common mental health difficulties, but 24-sessions of CBT or CAT produce equivocal outcomes (Marriott & Kellett, 2009). Family therapy and focal psychoanalytic psychotherapy were shown to produce better outcomes than CAT for adults suffering from AN (Dare et al., 2001), and CAT produced similar outcomes to a psychoeducation programme for AN (Treasure et al., 1995) and diabetes (Fosbury et al., 1997). These results must, however, be interpreted with caution given that studies often failed to establish an adequate baseline or control for differences between treatments delivered to patients.

Furthermore, evidence is accruing that suggests that varying modalities do achieve broadly similar outcomes (e.g. Roth & Parry, 1997) due to “*common factors*” (Lambert, 2005). This review found insufficient recognition within the CAT outcome literature regarding common factors known to influence outcomes (e.g. therapist effects, Crits-Christoph, Baranackie, Kurcias & Beck, 1991; therapeutic alliance, Horvath & Symonds, 1991), despite common factors forming an aspect of the CCAT measure of model fidelity (Bennett & Parry, 1994). CAT outcome research would do well to make use of such established tools to incorporate more consideration of common factors in future studies.

CAT was specifically developed to be appropriate for a range of conditions (Margison, 2000), yet authors have commented that CAT is often used to treat patients presenting with complex and severe difficulties (e.g. Denman, 2001). The current review substantiate this finding in two additional ways; a) CAT outcome

studies to date have been completed with patients with arguably more severe difficulties, and b) various studies report that patients with PD were more likely to be allocated to CAT by independent assessors (Brockman et al., 1987; Mace et al., 2006; Marriott & Kellett, 2009). This review also highlights the dearth of evidence of CAT for common mental health difficulties. The current evidence does suggest that CAT can be useful for treating affective disorders (e.g. Marriott & Kellett, 2009). It therefore appears timely for efficacy trials to build on findings from CAT PBE, in line with the hourglass model (Salkovskis, 1995), to focus future CAT research and develop effective treatment strategies for this clinical population.

Moreover, Margison (2000) commented that, "*CAT is unusual in being increasingly widely practised without following the full three-stage model of development [the hourglass model as advocated by Salkovskis, 1995]*" (p.146). A decade on, there does not appear to have been any further coherent progression of the CAT evidence base. The development of diagnosis specific CAT evidence appears disorganised, somewhat disparate and in no sense chronological, whilst largely bypassing rigorous evaluation. The current state of the CAT evidence base is too dependent on small-scale and, uncontrolled PBE style studies. Although clinically representative, the external validity of studies is limited by the general lack of reporting of patient characteristics and therapist/service characteristics, making it difficult to assess for whom CAT may be effective and under which conditions.

It is vital, however, that this lack of an as yet substantive evidence-base should not be equated with ineffectiveness of CAT (NICE, 2009). Rather, CAT outcome research should be viewed as a nascent endeavour. Developing a coherent and credible evidence-base for CAT requires interdependent EBP and PBE paradigms prior to integration via meta-analysis (Barkham & Mellor-Clark,

2003). High quality, co-ordinated large-scale efficacy and effectiveness studies need to be conducted, which are mapped out according to established frameworks of evidence-base development (e.g. Salkovskis, 1995), to guide clinical practice.

Theoretical Implications

The original CAT model was based around a broad concept of ‘neurosis’ (Kerr, 2005), offering a general model of development and psychopathology (Ryle & Kerr, 2002). At present, CAT has evolved a model of BPD (Multiple Self-States Model; Ryle, 1997), based on the theory of early trauma prompting dissociation of reciprocal roles into separate self-states. Efforts to develop a CAT model for self-harm (Sheard, Evans, Cash, Hicks, King, Morgan et al., 2000; Ougrin, Ng & Low, 2008 ;Ougrin, Zundel, Ng, Banarsee, Bottle & Taylor, 2011) and psychosis (Kerr, Birkett & Chanen, 2003; Ryle & Kerr, 2002) have been made, but these are yet to be subject to rigorous outcome evaluation. There is a stark lack of CAT conceptual frameworks for guiding treatment with patients suffering from common mental health problems. Developing theoretically consistent, disorder-specific models, to drive future outcome studies that incorporate assessment of “*common factors*”, would clearly benefit the CAT evidence-base.

Clinical Implications

CAT shows promising utility for people presenting with a range of personality disorders (Ryle & Golyukina, 2000; Wildgoose et al., 2001; Chanen et al., 2009; Kellett, 2007; Kellett, in press), eating disorders (Dare et al., 2001; Treasure et al., 1995), long-term health conditions (Fosbury et al., 1997; Walsh, Hagan & Gamsu, 2000; Yeates et al., 2008), difficulties associated with CSA (Clarke & Llewelyn, 1994; Clarke & Pearson, 2000), common mental health problems (Brockman et al.,

1987; Marriott & Kellett, 2009) and psychotic experiences (Kerr, 2003; Ryle & Kerr, 2002). However, CAT has not demonstrated superiority to other psychotherapy approaches in any area. Research suggests that CAT for PD should be longer than 16-sessions (Mace et al., 2006; Wildgoose et al., 2001), although no studies have explicitly explored the dose-response effect for CAT.

Future Research

Developing the CAT evidence-base requires coherence between efficacy, effectiveness and practice-base studies (Salkovskis, 1995; Barkham & Mellor-Clark, 2003). Concerted research efforts concerning CAT are indicated, as a lack of compelling research evidence may preclude future investment in a promising intervention. Future large-scale pragmatic trials (Goodyer et al., 2011) may offer a methodology, that fits with the aspiration of CAT, to evaluate outcomes in clinical practice incorporating longer-term follow-up and benchmarking against other modalities (Lueger & Barkham, 2010). Studies should routinely report rates of reliable and clinically significant change. Future research should consider routinely utilising a measure of clinical competence specifically developed for CAT (e.g., CCAT; Bennett & Parry, 2004) and consider exploring the optimal 'dose' of CAT for differing diagnoses and patient profiles. Studies need to explore why some patients drop out or deteriorate during CAT. Future research may use methodology associated with stage 1 of the hourglass model (Salkovskis, 1995) to explore factors associated with outcome to refine the theory and practice of CAT that can be tested in efficacy (stage 2) and effectiveness (stage 3) studies. Given the popularity of the approach, CAT practice research networks are a distinct possibility.

There is a need to isolate the mechanisms of change in CAT. The outcome data suggests that some CAT-specific tools appear to be associated with sudden gains (Yeates et al, 2008; Kellett, in press). Although a review of CAT process research is beyond the scope of this paper, there is debate within the literature on the role of the reformulation letter (Hamill, 2008; Shine, 2010; Rayner, 2011) and SDR (Kellett, 2005) in facilitating change. Building the evidence-base for CAT will require further exploration of the impact of such CAT-specific tools on outcomes.

Limitations of the review

There is a need to be circumspect about the current findings, due to the small number of papers included in the present review. Furthermore, the findings reported may be susceptible to researcher allegiance; two CAT practitioners' authored/co-authored nine papers. Although the quality-rating checklist employed was developed to be appropriate across the hierarchy of evidence, it is grounded within a medical paradigm. Therefore, it is inappropriate to apply some quality ratings to psychotherapy outcome studies (e.g. participants were 'blind' to the intervention). It is acknowledged that the checklist represented a heuristic tool, highlighting the need to develop a similar tool to capture the quality of evidence across diverse paradigms in psychotherapy research. The current state of the CAT evidence did not permit a meta-analysis due to the small number of published studies.

Conclusion

CAT has been criticised for being taken up in practice prior to the development of a sufficiently robust and relevant knowledge-base (Llewelyn, 2003; Marriott & Kellett, 2009). CAT lacks an as yet credible evidence-base, largely due to bypassing

the rigours of efficacy trials and poor reporting of patient/service characteristics. However, an insubstantial and nascent evidence-base should not be equated with ineffectiveness. The outcome evidence to date does suggest that CAT may to be a promising intervention, with an uncontrolled effect size of 0.81 found across nine studies reporting outcomes from a range of diagnostic groups. CAT has not been shown to produce superior outcomes to other approaches, although confidence in these findings are restricted by methodological flaws.

The review highlighted a trend for patients presenting with severe and complex difficulties in routine practice to be allocated to CAT by independent assessors (Brockman et al., 1987; Mace et al., 2006; Marriott & Kellett, 2009). Denman (2001) notes that CAT practitioners specialise in treating more severe difficulties, with a parallel process reflected in the range of empirical studies conducted to date. More attention should be focused the needs of patients with common mental health problems. The majority of CAT evidence has accrued from studies of BPD populations, perhaps reflecting a self-fulfilling prophecy since CAT has only developed a clearly delineated model of treatment for BPD (Ryle, 1997). There are some indications that 16-session CAT is not sufficient to effect change for patients with severe and complex difficulties, although the optimal dose of CAT remains to be elucidated.

Concerted research efforts should centre on developing co-coordinated larger-scale CAT efficacy and effectiveness studies, reporting rates of reliable and clinically significant change, and incorporating consideration of common factors known to influence outcome and longer term follow-up, with clear foundations in existing frameworks that guide evidence-base development.

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Group Cognitive Analytic Therapy for Female Survivors of Childhood Sexual Abuse: A Practice-Based Study

Objectives. To investigate the effectiveness of 24-sessions of group Cognitive Analytic Therapy (GCAT) for female adult survivors of childhood sexual abuse (CSA), using a practice-based dataset.

Design. A longitudinal cohort design.

Methods. Patients offered 24-sessions of GCAT contributed outcomes to a practice-based dataset (N=157). Validated measures of psychological functioning were administered at intake, pre and post-GCAT. 'Intention-to-treat' and 'completer' analysis of pre/post scores and rates of reliable and clinically significant change were calculated. Change scores on the Brief Symptom Inventory were benchmarked against other practice-based studies reporting group psychotherapy outcomes for survivors.

Results. High levels of psychological distress were observed at intake. Statistically significant improvements in psychological functioning were achieved in both the intention-to-treat and completer samples. GCAT 'completers' had an uncontrolled pre/post treatment effect size of 0.34; 8% (n=8) demonstrated a reliable and clinically significant improvement in global functioning and a further 13% (n=13) achieved reliable improvement.

Conclusions. GCAT, as an adjunct to secondary mental health clinical care, appears a promising intervention for adult female survivors of CSA considering pre-

treatment levels of psychological distress and service setting. Clinical, organisational and theoretical implications are discussed and future directions for CSA outcome research identified.

Practitioner points.

- GCAT is a promising intervention for female CSA survivors experiencing severe levels of distress.
- The evidence-base for effective interventions for survivors of CSA requires further development.
- For clinical services to be able to meaningfully contribute to the CSA treatment evidence-base, adherence to minimum data quality standards is required.

Since the early 1980s there has been growing awareness of the prevalence of childhood sexual abuse (CSA) and its associated psychological toxicity (Smart, 2000). With this awareness has come an increasing need for mental health services to appropriately respond to patients' distress and ask how best to effectively ameliorate the harmful effects of being sexually abused as a child. This empirical study aims to contribute to the current evidence-base, exploring the real world effectiveness of group Cognitive Analytic Therapy in routine clinical practice for adults sexually abused during childhood.

Whilst there are acknowledged difficulties in estimating the 'true' prevalence and incidence rates of CSA (Putnam, 2003), a recent cross-cultural study reported that 20% of women and 8% of men had been sexually abused before the age of 18 (Pereda, Guilera, Forns, & Gomez-Benito, 2009). In the UK, similar rates of CSA have been reported, with 21% of females and 11% of males disclosing histories of CSA (Cawson, Wattam, Brooker, & Kelly, 2000). However, operationalising CSA is fraught with difficulties; it is a complex concept and many definitions exist. The following is taken from the World Health Organisation (1999):

“Child sexual abuse is the involvement of a child in sexual activity that he or she does not fully comprehend, is unable to give informed consent to, or for which the child is not developmentally prepared and cannot give consent, or that violate the laws or social taboos of society. Child sexual abuse is evidenced by this activity between a child and an adult or another child who by age or development is in a relationship of responsibility, trust or power, the activity being intended to gratify or satisfy the needs of the other person.”

Throughout this study the term *survivor* is used to describe an individual who was sexually abused during childhood. It is acknowledged, however, that this term may not adequately represent all individuals' experiences of CSA.

Psychological toxicity of childhood sexual abuse

Adult survivors constitute a heterogeneous population, with diverse experiences and subsequent outcomes (Rutter, 2007). Although there is growing literature on resilience and CSA (Hyman & Williams, 2001), a wealth of evidence confirms the multiple, complex and often long-term psychological difficulties associated with CSA. Survivors appear at increased risk of anxiety, depression and guilt (Clarke & Llewelyn, 1994), dissociation (Rodriguez-Srednicki, 2001), interpersonal difficulties (DiLillo, 2001), personality disorder (Zlotnick, Mattia, & Zimmerman, 2001), substance misuse (Molnar, Buka, & Kessler, 2001), low self-esteem (Freshwater, Leach, & Aldridge, 2001; Lalor & McElvaney, 2010), eating disorders (Harper, Richter, & Gorey, 2009), post-traumatic stress disorder (Rodriguez, Vande-Kemp, & Foy, 1998), sexual promiscuity and social withdrawal (Beitchman et al., 1992), self-injurious behaviour and suicidality (Briere & Runtz, 1987).

Although various models have been proposed to understand the heterogeneous, and often extensive and pervasive negative impact of CSA (see Freeman & Morris, 2001 for a review) there is, as yet, no widely supported conceptualisation to guide treatment (Llewelyn, 2002). One model put forward has been the '*traumagenic dynamics*' model (Browne & Finkelhor, 1985; Finkelhor & Browne, 1986), which conceptualises problematic behaviours and psychological distress as complex trauma reactions arising from the meaning survivors attribute to the experience of CSA.

The Traumagenic Dynamics model

The traumagenic dynamics model (Browne & Finkelhor, 1985; Finkelhor & Browne, 1986) understands the cognitive, affective and behavioural consequences of CSA as the intrinsic misuse of power. The model comprises of four key factors, termed '*traumagenic dynamics*': 1) *Traumatic sexualisation* is associated with sexualised behaviour, either sexual dysfunction or a premature preoccupation with sexual activity; 2) *Stigmatisation* refers to the impact of abuse on a child's developing sense of self-worth that gives rise to feelings of guilt and shame, often compounded by cultural and religious taboos surrounding CSA; 3) *Powerlessness* distorts a child's sense of self-efficacy, and 4) *Betrayal* is fundamentally associated with mistrust, whereby a child learns that someone who they depend on for protection is dangerous and has either directly or indirectly caused them harm. The model provides a theoretical framework for formulating survivors' difficulties within a Cognitive Analytic Therapy approach (Hagan & Gregory, 2001).

Cognitive Analytic Therapy (CAT)

CAT integrates psychoanalytic and cognitive-behavioural models to offer a time-limited, relational approach to facilitating therapeutic change (Ryle & Kerr, 2002). Theoretically, CAT draws on personal construct theory (Kelly, 1956) and object relations theory (Ryle, 1985; Ogden, 1983), asserting that mental representations of self, others and the world are developmentally formed by early social, reciprocal interactions with significant others (Kerr, 2005). Such internalised early object relations are termed '*reciprocal roles*' (Ryle & Kerr, 2002) and become lenses through which individuals anticipate and react to interpersonal relationships.

Target Problem Procedures (TPPs) are aim-directed, repetitive sequences that dictate interactions with others and patterns of self-care (Ryle & Kerr, 2002;

Denman, 2001). The CAT model explains how difficulties are maintained despite damaging outcomes, describing three general TPPs (traps, snags and dilemmas; Ryle & Kerr, 2002) that confirm and perpetuate maladaptive reciprocal roles. When formulating CSA survivors' difficulties, CAT suggests that survivors have learnt a repertoire of reciprocal roles and TPPs to 'survive' the trauma and adversity of CSA (Hagan & Gregory, 2001; Llewelyn, 1997; Clarke & Llewelyn, 1994).

CAT therefore understands survivors' psychological development as being contextualised by the meaning they attribute to the abuse (Finkelhor & Browne, 1986), and broadly formulates traumagenic dynamics as reciprocal roles and TPPs. For example, in CAT a '*powerless, abused*' child being sexually abused by a '*powerful abuser*' will internalise both poles of this relationship template (Llewelyn, 2002). Not only will the child know what it is to feel 'powerless', 'worthless' and 'abused', they will also have an insight into what it is to be 'powerful' and 'abusing'. Llewelyn (2002) suggests that the experience of being '*powerless*' and '*abused*' may manifest in survivors as depression, whilst the '*powerful abuser*' may be demonstrated as self-harm and possible abuse of others.

Effective change in CAT is considered to arise from the process of early, collaborative narrative 'reformulation' that develops a shared understanding to explain the developmental origins of difficulties (Ryle, 1995). A sequential diagrammatic reformulation (SDR) is constructed to illustrate predominant reciprocal roles and repetitive patterns that maintain patients' difficulties and limit change (Ryle, 1997). The SDR is used to facilitate recognition of damaging patterns, both external to therapy and within the therapeutic relationship. Exits are identified to actively revise maladaptive procedures, with the therapist offering a containing, non-collusive experience (Ryle & Kerr, 2002). CAT is time-

limited, emphasising the importance of therapeutic endings. As such, at termination patients and therapists exchange good-bye letters, reflecting on changes achieved and potential future goals and obstacles to change.

Critics have, however, highlighted the limited extant outcome evidence-base for Cognitive Analytic Therapy (CAT) in general, compared with other psychotherapeutic approaches (Llewelyn, 2003; Marriott & Kellett, 2009). The preceding literature review concluded that CAT shows promise as an intervention for a range of clinical populations, including survivors (Clarke & Llewelyn, 1994; Clarke & Pearson, 2000), but continued efforts are required to contribute to a cumulative evidence-base.

Psychotherapy for survivors of childhood sexual abuse

Despite the vast amount of literature on the diverse and complex psychological difficulties associated with CSA, there is a stark paucity of empirical knowledge regarding effective interventions for distressed adult survivors (for reviews see Llewelyn, 1997; Martsolf & Draucker, 2005; Price, Hilsenroth, Petretic-Jackson, & Bonge, 2001; Taylor & Harvey, 2010). Although various psychotherapeutic modalities have been evaluated in individual and group settings (e.g. Martsolf & Draucker, 2005; Gorey, Richter, & Snider, 2001) the impetus remains to develop a robust and relevant evidence-base that guides mental health services in appropriately responding to survivors' distress (Beutler, 1993; Westbury & Tutty, 1999).

Group Psychotherapy for survivors of CSA

Several studies (e.g., Alexander, Neimeyer, Follette, Moore, & Harter, 1989; Hazzard, Rogers, & Angert, 1993; Westbury & Tutty, 1999) and reviews (Kessler,

White, & Nelson, 2003; Taylor & Harvey, 2010) have indicated that group psychotherapy can be an effective treatment modality for CSA survivors. Hagan and Gregory (2001) advocated that groups may offer therapeutic benefits to survivors above and beyond those achieved in individual psychotherapy. Groups can challenge survivors' conception that they are alone and somehow different from others, thereby addressing stigmatisation and improving self-esteem (Llewelyn, 1997). Groups can also facilitate trusting relationships that can reduce the sense of guilt, self-blame and betrayal that often prevail for survivors (Hagan & Gregory, 2001).

Group Cognitive Analytic Therapy

Whilst group Cognitive Analytic Therapy (GCAT; Maple & Simpson, 1995) has been advocated as a promising treatment approach for CSA (Hagan & Gregory, 2001), the evidence-base for GCAT is remarkably absent (John & Darongkamas, 2009). Only two studies to date have published outcomes from GCAT. An early study describing 12-week GCAT delivered in routine practice (Duignan & Mitzman, 1994; n=7) reported significant improvements in depression and wellbeing. More recently, Ryan, Nitsun, Gilbert, and Mason (2005) reported outcomes for 22 female survivors completing a psycho-educationally based group intervention informed by Cognitive-Behavioural and Cognitive-Analytic principles. The study indicated a highly statistically and clinically significant effect of the group on wellbeing, although findings from both studies are limited to small sample sizes.

Given the scant evidence for effective treatments for survivors In general (e.g. Llewelyn, 1997; Martsof & Draucker, 2005; Price, Hilsenroth, Petretic-Jackson, & Bonge, 2001; Taylor & Harvey, 2010) and the lack of an evidence-base for GCAT (John & Darongkamas, 2009), it appeared timely to conduct a larger-scale

study interrogating outcome data collected in routine clinical practice, to evaluate the effectiveness of GCAT for adult survivors of CSA in a 'real-world' setting.

Evaluating effectiveness: Practice-based evidence

Numerous authors have commented on the tension between *efficacy* studies of psychotherapy demonstrated in clinical research trials and *effectiveness* studies completed in routine clinical practice (Donnenberg et al., 1999; Roth & Fonagy, 2005; Weston, Novotny, & Thompson-Brenner, 2004). The external validity of trials has been challenged (Cartwright, 2007; Rothwell, 2005) and a *practice-based evidence* paradigm (Cahill, Barkham & Stiles, 2010) has been advocated to make use of routinely available 'real world' datasets (Marriott & Kellett, 2009; Holloway, 2002) that complement and contextualise efficacy evidence (Bower & Gilbody, 2010; Gilbody & Whitty, 2002).

Practice-based studies can offer an ethical methodology for investigating the effectiveness of treatments (Stiles, Barkham, Mellor-Clark, & Connell, 2008; Stiles, Barkham, Twigg, Mellor-Clark, & Cooper, 2006) to ultimately improve the quality of patient care (Barkham, Hardy, & Mellor-Clark, 2010). However, estimating the effectiveness of an intervention in routine practice is intrinsically dependent upon the empirical index chosen to represent a 'good' outcome (Barkham, Stiles, Connell, & Mellor-Clark, 2011). Rates of '*reliable and clinically significant change*' (Jacobson & Truax, 1991) have been advocated as a measure of effectiveness (i.e. number of patients who start treatment in a clinical population and finish treatment within a 'non-clinical' population, having achieved change on an outcome measure that exceeds any inherent measurement error) (e.g. Evans, Margison, & Barkham, 1998). Yet for patients with severe and enduring difficulties, achieving clinically significant change may be unrealistic and '*reliable improvement*

only' may be a more appropriate clinical goal. Furthermore, acceptability of treatment represents an important aspect of effectiveness (Cavanagh et al., 2009). Engagement and regular attendance in routine clinical practice are important issues for patients with severe and complex difficulties (DoH, 2007) and approximately 40% of CSA survivors dropout of group treatment (Fisher, Winne, & Ley, 1993; Ryan et al., 2005). Therefore, evaluating the effectiveness of GCAT for survivors must take into account the uptake and completion rates in routine practice.

Benchmarking

GCAT outcomes should be evidenced against other group psychotherapy outcomes for adult survivors. This entails locating GCAT outcomes against a standard - a benchmark - as an indicator of effectiveness (Barkham, Stiles, Lambert, & Mellor-Clark, 2010). A recent meta-analysis by Taylor and Harvey (2010) identified 25 repeated measures studies, from which benchmarking data was drawn from five papers, reporting outcomes from seven group interventions for adult survivors of CSA (Lau & Kristensen, 2007; Lubin, Loris, Burt, & Johnson, 1998; Lundqvist & Ojehagen, 2001; Saxe & Johnson, 1999; Talbot et al., 1999).

To summarise, a wealth of evidence confirms the negative, long-term, psychological impact of CSA (e.g. DiLillo, 2001; Freshwater et al., 2001; Rutter, 2007), yet outcome research for CSA survivors is in its empirical infancy (Llewelyn, 1997; Martsolf & Draucker, 2005; Price et al., 2001; Taylor & Harvey, 2010). GCAT is one approach offered to survivors (Hagan & Gregory, 2001) but is currently unsupported by a robust evidence-base (John & Darongkamas, 2009).

Therefore, it appeared timely to investigate the effectiveness of GCAT for this population in a 'real-world' setting.

Aims of the present study

The present practice-based study aimed to use a dataset collected in routine clinical practice to investigate the effectiveness of GCAT for female adults sexually abused during childhood. The study utilised a within-subject wait-list control condition. Investigating the difference between change in outcome scores during treatment and wait-list supports inferences that observed differences between the groups can be attributed to the introduction of treatment (Cisler, Barnes, Farnsworth, & Sifers, 2007). This study also employed benchmarking methods (Lueger & Barkham, 2010) to contextualise GCAT outcomes with other practice-based group psychotherapy outcome studies for female adult CSA survivors.

Hypotheses

1. Statistically significant improvements in psychological wellbeing and interpersonal functioning will be achieved during GCAT, compared with change achieved during wait-time.
2. Rates of reliable and clinically significant improvement (Jacobson & Truax, 1991) in psychological wellbeing and interpersonal functioning during GCAT will be greater than rates of reliable and clinically significant change achieved during wait-time.

Method

This study employed a longitudinal, cohort design.

Setting

GCAT was delivered in a tertiary mental health service for survivors of CSA. The service offered GCAT and individual therapy to adults of working age, referred by secondary or tertiary level mental health services. All patients, by virtue of being accepted into the service would have been concurrently receiving a wide variety of input from secondary mental health care teams. The dataset did not detail information regarding current prescribed medication or concurrent service input. Consequently, patients referred to the service presented with chronic, enduring and complex mental health difficulties, requiring comprehensive care packages.

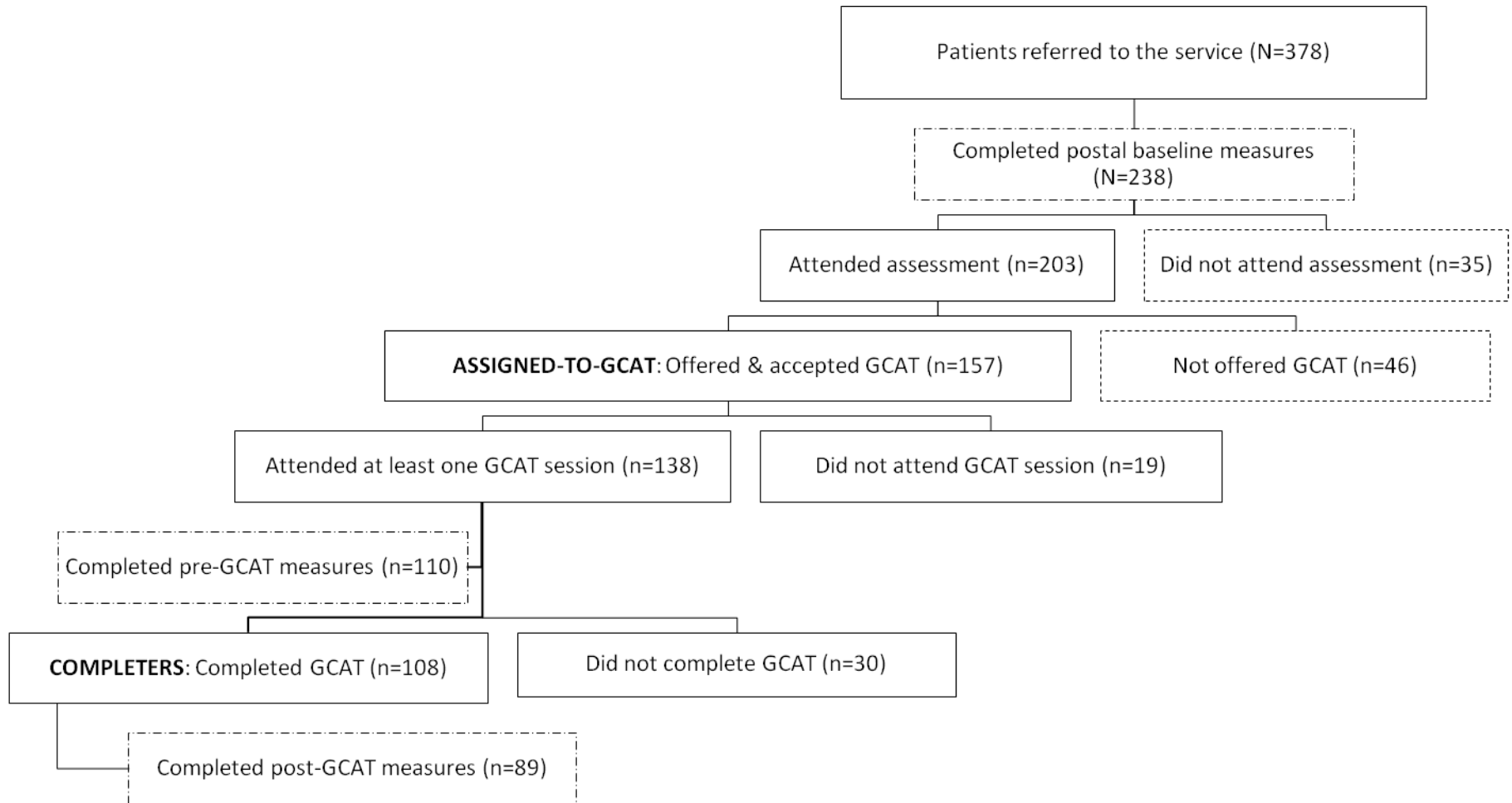
The service ethos aimed to reduce survivors' distress and increase coping rather than offering a 'cure', consistent with the '*recovery model*' (National Institute of Mental Health, 2005). Strict inclusion and exclusion criteria to the service were not applied and patients therefore represented a clinically representative sample (Shadish, Matt, Navarro, Siegle, Crit-Christoph, & Hazelrigg et al., 1997).

Sample

The patient data included in the study were extracted from an existing anonymised dataset (N=378). The dataset represented all patients referred into the service since its inception in 1995. Potential patients were excluded from subsequent analysis if outcome measures at intake were missing from the original dataset. *Post-hoc* eligibility criteria were applied to derive the research sample, consisting of all patients that had i) completed intake measures, ii) attended assessment, and iii) been offered and accepted GCAT, regardless of whether or how much of GCAT

they attended. Therefore this study reports outcomes for a) an 'assigned-to-GCAT' sample (n=157), and also b) a 'completer sample' (n=108) who were offered, accepted and completed GCAT (see Figure 1 for patient flow chart through the stages of the study).

Figure 1; diagram to illustrate patient flow through GCAT service pathway



Starting with each patient's first referral to the service, 238 patients (63%) completed measures at intake and of these 85% (n=202) attended their initial face-to-face assessment. Following assessment, 157 of the 'attenders' (77%) consented to GCAT and comprise the 'assigned-to-GCAT' sample. Of these, 12% (n=19) did not attend any GCAT sessions and disengaged from the service and 19% (n=30) did not complete the entire GCAT program. One hundred and eight patients (69%) completed GCAT and comprise the 'completer' sample in the current study. Outcome measures were completed for 57% (n=89) of the assigned-to-GCAT sample (n=157) at intake, pre- and post-GCAT.

Procedure

Once a referral to the service has been deemed appropriate, the service responds to the referral by sending a letter to the patient inviting them to an assessment session. Included with the letter is a booklet containing a battery of five outcome measures (see Appendix vii). If patients opt-in to the service at this point, their mental health needs are assessed during an initial face-to-face assessment, typically lasting approximately 60 minutes where a variety of options are discussed; GCAT being one of these. No standardised diagnostic procedures are employed. Allocation thus reflects routine clinical practice decisions considering clinical need, rather than adhering to the randomisation procedures that define efficacy studies (Buckley, Newman, Kellett, & Beail, 2006). Patients who opt-in to GCAT are placed on a waiting list and invited to join the next group. At this point, patients are offered tailored resource materials to enable them to prepare for the group work (e.g. psychoeducation regarding CSA taken from Ainscough & Toon, 2000).

Measures

Patients are asked to complete the battery of self-report outcome measures (Appendix vii) at intake, pre- and post-GCAT. The interval between assessment and pre-GCAT therefore represents a naturally occurring waitlist control condition.

Given the diverse nature of difficulties that survivors' experience, a symptom specific measure was considered unlikely to capture the extent of patients' distress. Therefore the Brief Symptom Inventory (BSI; Derogatis & Melisaratos, 1983) was chosen as the primary outcome measure for this study, being a valid and reliable measure of broad psychological difficulties. The BSI is also the most widely employed outcome measure in the group therapy outcome studies and so enables the meaningful comparison of outcomes from group psychotherapy for CSA survivors.

Brief Symptom Inventory (BSI; Derogatis & Melisaratos, 1983)

The BSI is a validated short version (53 items) of the SCL-R-90 (Derogatis, Lipman, & Covi, 1973), rated on a 5-point Likert scale (item scores=0,1,2,3,4). Higher scores indicate higher levels of general distress. The scale measures nine primary symptom dimensions: Somatisation, Obsessive-compulsive Symptoms, Interpersonal Sensitivity, Depression, Anxiety, Hostility, Phobic Anxiety, Paranoid Ideation and Psychoticism (Derogatis, 1993). In addition, three global indices can be calculated: Global Severity Index (BSI-GSI), Positive Symptom Total (PST) and Positive Symptom Distress Index (PSDI). The BSI-GSI is a mean score, combining information about the number and intensity of distressing symptoms representing a summary score of psychological distress. The test-retest reliability coefficient for the BSI-GSI is 0.90 (Derogatis & Melisaratos, 1983), indicating that the BSI-GSI is

stable over time. Cronbach's α co-efficients have been reported ranging from 0.71 to 0.85 (Boulet & Boss, 1991; Kellett, Beail, Newman, & Frankish, 2003). The current Cronbach's α was 0.97, demonstrating high internal consistency. A clinical threshold has been reported of a t-score of 63 or higher as suggesting clinical 'caseness' (Derogatis, 1993), which equates with a threshold score of 0.78.

Inventory of Interpersonal Problems-32 (IIP-32; Barkham, Hardy, & Startup, 1996)

This measure is designed to identify interpersonal difficulties. The IIP-32 is a validated and shortened version of the original 127-item scale (Horowitz et al., 1988). Items are scored on a 5-point scale (item scores=0,1,2,3,4). A higher mean score indicates more interpersonal difficulties. The IIP-32 has eight subscales that form four bipolar factors (Barkham, Hardy, & Startup, 1996): hard to be assertive vs. too aggressive; hard to be sociable vs. too open; hard to be supportive vs. too caring and hard to be involved vs. too dependent. The IIP-32 has a test-rest co-efficient of 0.70 and Cronbach's α of 0.87, slightly lower than the current assigned-to-GCAT sample internal consistency coefficient of 0.89. Whiffen, Thompson, and Aube (2000) reported a co-efficient of 0.81 in their study of adult female survivors. Given that there are no published clinical thresholds for the IIP-32, a cut-off score of 1.39 was derived according to criteria outlined by Evans et al. (1998) utilising normative data reported in Barkham, Hardy and Startup (1996) to enable rates of clinically significant change to be calculated.

Rosenberg Self-Esteem Scale (RSES; Rosenberg, 1989)

The RSES is a 10-item scale used to measure perceptions of global self-worthiness (Rosenberg, 1989). Item responses range from 0 (strongly disagree) to 3 (strongly agree). To obtain a total score, appropriate items are reversed and item scores

summed, so that a higher score indicates better self-esteem (range=0-30). Silber and Tippett (1965) report a test-retest coefficient of 0.85, and Liem and Boudewyn (1999) an internal consistency co-efficient of 0.88 with survivors of CSA. The current study found a lower, but still acceptable Cronbach's α of 0.81. No comparative data exists for an adult clinical or non-clinical population. Therefore a clinical threshold of 25.22 was determined as 2 standard deviations above the assigned-to-GCAT sample's pre-GCAT mean score (Evans, et al., 1998) to enable rates of clinically significant change to be calculated.

Hospital Anxiety and Depression Scale (HADS; Zigmond & Snaith, 1983)

The HADS is a 14-item questionnaire, consisting of two subscales measuring severity of anxiety (HAD-A) and depression (HAD-D). Responses to each item are scored 0-3 (range for each scale is 0-21); higher scores indicate increasingly severe levels of anxiety/depression. Crawford, Henry, Crombie, and Taylor (2001) suggest a clinical threshold value of 11. The scale has been found to have sound psychometric properties (Savard, Laberge, Gauthier, Ivers, & Bergeron, 1998). No studies have published test-retest reliability coefficients, however for the anxiety and depression subscales Cronbach's α coefficients are reported as 0.89 and 0.92 respectively (Zigmond & Snaith, 1983). Within the current study, α was 0.83 for both subscales.

General Health Questionnaire-28 (GHQ-28; Goldberg & Hillier, 1979).

The GHQ-28 identifies current non-psychotic, mental health difficulties. The GHQ yields a total score and four sub-scales: somatic symptoms, anxiety and insomnia, social dysfunction and severe depression. There are two scoring methods for the GHQ-28; Likert scoring and GHQ scaled scores (item scores=0,0,1,1; maximum

score=28). The latter was employed in this study. Higher scores indicate more severe psychological distress. A World Health Organisation study of 5,438 participants from 15 different locations around the world, derived a UK clinical threshold of less than or equal to 7 (Goldberg et al., 1997). Robinson and Price (1982) report a test-retest correlation of 0.90 for the GHQ-28. The Cronbach's α of the pre-therapy scores in the current assigned-to-GCAT sample was 0.95, demonstrating high internal consistency. Goldberg and Williams (1988) report high internal consistency co-efficients ranging from 0.82 to 0.93.

Missing data

One hundred and ten patients (70% of the assigned-to-GCAT sample) completed pre-GCAT outcome measures and of these, 89 (57% of the assigned-to-GCAT sample) completed post-GCAT measures. For those patients not completing outcomes following intake (did not complete pre-GCAT measures n=47; did not complete post-GCAT measures, n=21), last observation carried forward (LOCF) was employed, as advocated in 'intention-to-treat analyses' (Montori & Guyatt, 2001; Barkham, 2011). This is recommended as a way of ensuring that subsequent analysis is not 'weighted' in favour for, or against, the intervention in question. Analysis in such a way provides a highly conservative estimate of change, as this sub-sample would show no change during GCAT.

If patients provided insufficient outcome data on a measure at any time-point (according to the scoring procedure for each measure), no score for that measure was calculated. For example, scoring procedures recommend that a single missing item from a subscale on the HADS is inferred by using the mean of the remaining six items. If more than one item is missing, however, then the subscale

should be judged as invalid (Zigmond & Snaith, 1983). Therefore, the total n for each measure varies in subsequent analysis.

Intervention: Group Cognitive Analytic Therapy

No standardised model of GCAT exists. GCAT in this study comprised of 24 weekly sessions, each lasting approximately 90 minutes, and one individual follow-up session. GCAT offers psycho-educational elements on understanding early trauma and based around CAT principles. The group was broadly structured around relationship building, 'reformulation', raising self-awareness of maladaptive reciprocal roles and target problem procedures, and identifying and practicing exits (Hagan & Gregory, 2001). GCAT utilised a judicious approach to developing 'exits' in response to the needs of the members of each group and drawing on a range of modalities, for example, cognitive (e.g., challenging unhelpful thinking), behavioural (e.g., reducing avoidance) or interpersonal (e.g., increasing assertiveness). GCAT was not manualised and specific interventions within the groups have varied over time.

Early sessions aimed to facilitate a safe group therapeutic space and come to an understanding of the nature of the difficulties that survivors experience as a result of CSA (Hagan & Gregory, 2001). 'Reformulation' aimed to account for the origin of survivors' difficulties and understand problematic procedures as coping strategies. A previous evaluation of GCAT constructed individual reformulations that were shared with the group members (Duignan & Mitzman, 1994), however, the current format facilitated a group reformulation process, in addition to individual reformulations, using a conceptual tool developed by the service; 'Lessons learned to survive' (see Appendix viii; Hagan & Gregory, 2001). Group

members identified 'exits' (e.g. techniques to safely express anger) and therapeutic exercises were conducted in the group setting.

Facilitators

Two female staff co-facilitated the group at any one time. A total of 14 female staff have facilitated at least fifteen groups; seven Clinical Psychologists, two Mental Health Nurses and five Trainee Clinical Psychologists, with varying experience of CAT. Experienced ACAT accredited practitioners supervised all facilitators. All staff had access to a resource file, which contained broad outlines for each group session whilst allowing flexibility for tailoring the content to the needs of patients.

Data analysis strategy

For those patients for whom demographic characteristics were known, Mann Whitney and chi-square analyses investigated the demographic characteristics of i) those assigned-to-GCAT (n=157) and those not offered to GCAT (n=46), and ii) 'non-completers' (n=49) and 'completers' (n=108). Independent sample t-tests explored differences between the assigned/not-assigned sample scores at intake, and completers/non-completers' scores at pre-GCAT.

Statistically significant change

Statistically significant change during wait-list and GCAT was evaluated for each outcome measure, with one-tailed significance level set at $p < 0.05$, using 2 (Group; completers/non-completers) x 3 (Time; assessment/pre-GCAT/post-GCAT) ANOVAS³⁵.

³⁵ Assumptions for these tests (normality, independence and equality of variance) were not met in all cases (GHQ-28 and HADS-Anxiety sub-scale). However, ANOVA is a robust procedure, even when sample sizes are unequal (O'Connor, Simmons and Cooper, 2003). Additionally, analysis repeated with non-parametric methods obtained similar findings.

Although the wait-list condition in this study provides some degree of control, it is not a 'pure' no treatment condition, as all patients received concurrent treatment from secondary mental health services. Furthermore, duration of time spent on the wait-list was not recorded in the dataset. Therefore, although change achieved during GCAT is compared with that achieved during wait-list, results should be interpreted with caution.

Benchmarking uncontrolled effect sizes

The study benchmarked outcomes against other evidence of effectiveness of group therapy for female survivors. The comparative studies used the BSI-GSI (Derogatis & Melisaratos, 1983) or SCL-90-R (Derogatis, Lipman, & Covi, 1973) as an outcome measure. An uncontrolled effect size (Cohen's d) was calculated using the pre-post change score during group intervention divided by the pre-group standard deviation (Barkham, Gilbert, Connell, Marshall, & Twigg, 2005; Westbrook & Kirk, 2005). Cohen (1988) defines three values for d : small (0.20), medium (0.50) and large (0.80) effect sizes for between-group studies. However, in line with previous within-subject research (Conway, Audin, Barkham, Mellor-Clark, & Russell, 2003), the study adopted an uncontrolled effect size of 0.2 as representing moderate improvement and 0.5 and above as an index of substantial improvement, due to the increased variance inherent within a practice-based sample.

Reliable and clinically significant change (RCSC)

Calculating *reliable change* entails calculating a reliable change index (RCI) for each measure, which denotes the extent to which change in a participant's score between two time-points is considered beyond that which would be attributable to measurement error (Jacobson & Truax, 1991). Firstly, the standard error of

difference is calculated; a function of the initial standard deviation of a sample and the test-retest reliability of the measure (Evans et al., 1998; see Table 1).

Multiplying this standard error of difference by 1.96 gives the RCI - a boundary beyond which change in a participant's score is unlikely to occur by chance more than 5% of the time (Jacobson & Truax, 1991). However, change may be reliable but not clinically significant.

Calculating clinically significant change requires a score representing the cut-off between clinical and non-clinical populations (see Table 1). Employing clinical change alone as a measure of 'caseness' has the potential to inflate recovery rates as someone may move from a clinical to a non-clinical population within the boundaries of inherent measurement error. Therefore, taken together, RCI and clinical change represent *reliable and clinically significant change* (RCSC)³⁶. As previously outlined, however, aiming for RCSC may be unrealistic given the severe and enduring nature of this samples' difficulties. Therefore, using the BSI-GSI (Derogatis & Melisaratos, 1983) as the primary outcome measure, two indices of effectiveness are reported in this study:

- 1) *reliable improvement only* (thereby taking into account patients whose levels of distress at intake make clinically significant change an unrealistic outcome), and
- 2) *reliable and clinically significant change*, (i.e. recovery)

³⁶ This study uses the most stringent index of RCSC, calculated using the published test-retest co-efficient and published clinical thresholds where available. However, where these were not available (as in the case of the IIP-32 and RSES) a clinical cut-off was derived, set at the most stringent level of a return to a non-clinical population.

Table 1; Summary of Reliable Change Index and clinical cut-off points

Measure	Pre-GCAT SD	Test-retest reliability co-efficient	Reliable Change Index	Clinical cut-off
BSI-GSI ¹	0.90	0.90	0.79	0.78
IIP-32 ²	0.65	0.78	0.84	1.39
RSES ³	5.46	0.85	5.86	25.22
HADS ⁴				
<i>Anxiety</i>	4.65	0.89*	4.27	11
<i>Depression</i>	4.34	0.92*	3.40	11
GHQ ⁵	8.75	0.90	7.67	7

¹Brief Symptom Inventory – Global Severity Index ²Inventory of Interpersonal Problems-32
³Rosenberg Self-Esteem Scale ⁴Hospital Anxiety and Depression Scale ⁵General Health Questionnaire

* No test-retest reliability co-efficients have been published for the HADS. Instead published Cronbach α (Zigmond & Snaith, 1983) are used to derive the RCI.

The rates of 'recovery' (i.e. RCSC) are dependent upon the number of patients scoring within the clinical range pre-intervention. For those not scoring above the clinical threshold it is Inherently impossible to achieve clinically significant change to a non-clinical population. Consequently, the rate of RCSC is calculated as the number of patients demonstrating reliable and clinically significant change during GCAT as a proportion of those scoring within the clinical range at intake (Barkham et al., 2011).

Table 2 presents the percentages of people classified as clinical and non-clinical at intake across each of the measures.

Table 2; *proportion of assigned-to-GCAT sample (n=157) completing each measure and scoring within the clinical and non-clinical range at intake.*

<i>Measure</i>	<i>Intake completed measure (n)</i>	<i>Clinical population % (n)</i>	<i>Non-clinical population % (n)</i>
BSI-GSI ¹	150	91% (137)	9% (13)
IIP-32 ²	147	78% (115)	22% (32)
RSES ³	147	95% (139)	5% (8)
HADS ⁴			
<i>Anxiety</i>	152	43% (65)	57% (87)
<i>Depression</i>	151	72% (108)	28% (43)
GHQ ⁵	150	81% (121)	19% (29)

Note. Pre-GCAT n ranged from 147 to 152 due to missing data on some measures

¹Brief Symptom Inventory – Global Severity Index ²Inventory of Interpersonal Problems-32

³Rosenberg Self-Esteem Scale ⁴Hospital Anxiety and Depression Scale ⁵General Health Questionnaire

McNemar tests were conducted to compare the rates of survivors achieving reliable improvement during the no treatment condition (i.e. waitlist), with those reliably improving during GCAT.

Data were analysed using SPSS 16 (SPSS Inc., 2007) and benchmarking carried out using STATA Statistical Software (StataCorp. 2001).

Power calculation

Assuming a p -value equal to or less than 0.05 used as criterion for statistical significance, an effect size of 0.5 and a sample size of 108, post-hoc power analysis (G*Power; Faul, Erdfelder, Lang, & Buchner, 2007) revealed that 99% power was achieved for a two-tailed hypothesis detecting a within-group difference in BSI-GSI scores.

Ethics

Ethical approval for this study was granted by South Yorkshire and Humber REC and registered with the participating NHS Trust's Governance department. The researcher did not have access to any identifiable patient information and only accessed an anonymised dataset. Therefore the study complied with data protection procedures for the use of a routine clinical outcome dataset.

Results

Sample characteristics

Overall, the age of all patients assessed ranged from 17 to 87 years (mean age=35 years, SD=11); 80% identified themselves as 'White British', 43% reported currently being in a relationship and 22% in paid employment. Table 3 shows the demographic characteristics for the not assigned to GCAT and assigned-to-GCAT sample with Mann-Whitney and chi-square analysis as appropriate.

Table 3; demographic characteristics at intake

	Not assigned to GCAT sample N=221	Assigned-to- GCAT sample N=157	χ^2/Z
Age (years)	Range 17-87 M = 34.51, SD=11.74	Range 18-64 M = 34.65, SD=10.67	-0.35
Ethnicity			
White	164 (74%)	138 (88%)	
Non-White	8 (4%)	7 (4%)	$\chi^2(1, N=317)=0.005$
Unknown	49 (22%)	12 (8%)	
Relationship Status			
In a relationship	77 (35%)	86 (55%)	
Not in a relationship	100 (45%)	62 (39%)	$\chi^2(1, N=325)=6.88^*$
Unknown	44 (20%)	9 (6%)	
Employment Status			
Paid employment	39 (18%)	44 (28%)	
Unemployed	84 (38%)	66 (42%)	$\chi^2(2, N=271)=2.53$
Studying/Retired	23 (10%)	15 (10%)	
Unknown	75 (34%)	32 (20%)	

* $p < 0.05$

There was no significant difference between the groups of patients assigned and not assigned to GCAT in terms of age ($z = -0.35$, $p = n/s$), ethnicity ($\chi^2(1, N=317) = 0.005$, $p = n/s$), or employment status ($\chi^2(2, N=271) = 2.53$, $p = n/s$).

However, those assigned to GCAT following assessment were significantly more likely to report being in a relationship ($\chi^2 (1, N=325)=6.88, p=0.009$).

Independent t-tests compared the intake scores for the assigned-to-GCAT sample (n=157) with participants who attended assessment but were not offered GCAT (n=46; see Table 4). Scores are contextualised by the published means and standard deviations for each measure where available, highlighting the elevated levels of distress experienced by this sample. The level of global distress for the assigned-to-GCAT sample at intake (BSI-GSI; M=2.23, SD=0.91) is much higher than data reported in a recent British outpatient sample (M=1.66, SD = 0.83; Ryan, 2007) and a previous study of female survivors completing 16-session CAT in routine clinical practice (M=1.80, SD=1.13 ; Clarke & Llewelyn, 1994).

Table 4; published norms, and outcome profile at intake for not assigned to GCAT and assigned-to-GCAT sample at intake

Measure	PUBLISHED NORMS			EMPRICIAL STUDY SAMPLE						
	N	Mean	SD	Not offered GCAT (n=46)			Assigned-to-GCAT (n=157)			t
				N	Mean	SD	N	Mean	SD	
BSI-GSI ¹	252 ^a 8 ^b 376 ^c	1.66 1.80 0.44	0.83 1.13 0.47	42	2.20	0.89	150	2.23	0.91	t(190)=-2.23
IIP-32 ²	76 ^d 45 ^e	1.47 0.95	0.65 0.52	42	2.01	0.71	147	1.97	0.62	t(187)=0.34
RSES ³				43	18.93	5.77	147	15.30	4.96	t(188)=4.85***
HADS ⁴										
<i>Anxiety</i>	1792 ^f	3.68	3.07	46	14.17	3.54	152	14.01	4.09	t(196)=0.28
<i>Depression</i>	1792 ^f	6.14	3.76	46	11.06	4.05	151	11.29	4.64	t(195)=-0.37
GHQ ⁵	1670 ^g	5.68	6.15	40	15.60	7.86	150	16.58	8.21	t(188)=-0.84

¹Brief Symptom Inventory – Global Severity Index ²Inventory of Interpersonal Problems-32 ³Rosenberg Self-Esteem Scale ⁴Hospital Anxiety and Depression Scale ⁵General Health Questionnaire

^aRyan, 2007; clinical sample ^bClarke & Llewelyn, 1994; clinical sample ^cFrancis, Rajan, & Turner, 1990; non-clinical sample ^dBarkham, Hardy, & Startup, 1996; clinical sample ^eBarkham, Hardy, & Startup, 1996; non-clinical sample ^fCrawford, Henry, Crombie, & Taylor, 2001; non-clinical sample ^gWillmott, Boardman, Henshaw, & Jones, 2004; non-clinical sample

*** $p < 0.001$

Given that the sample was restricted to those completing intake outcome measures it is impossible to draw firm conclusions on the representativeness of those patients assigned to GCAT compared with the wider population presenting to the service. However, four outcome measures suggest no significant differences between those assigned to GCAT and patients not assigned to GCAT at assessment; BSI-GSI ($t(190)=-2.23, p=n/s$), IIP-32 ($t(218)=0.34, p=n/s$), GHQ-28 ($t(188)=-0.84, p=n/s$) and HADS (Depression: $t(195)=-0.37, p=n/s$; Anxiety: $t(196)=0.28, p=n/s$). However, there appeared to be a significant difference on the RSES ($t(188)=4.85, p<0.001$), indicating that the assigned-to-GCAT sample reported significantly lower self-esteem. Overall, the assigned-to-GCAT sample appeared broadly representative of all patients referred to the service, although the assigned-to-GCAT sample were more likely to be in a relationship and demonstrate lower self-esteem.

Table 5 presents the profile for the non-completer ($n=49$) and completer sample ($n=108$) at intake. For those participants whose demographic characteristics were known, no significant differences were found between the non-completer and completer samples; age ($z=-1.48, p=n/s$), ethnicity ($\chi^2(1, N=145)=0.034, p=n/s$), relationship status, ($\chi^2(1, N=148)=0.001, p=n/s$) and employment status ($\chi^2(2, N=125)=0.401, p=n/s$).

Table 5; *demographic characteristics recorded at Intake for the non-completer and completer sample*

	GCAT Non-completers N=49	GCAT completers N=108	χ^2/Z
Age (years)	Range 18-64 M=33.01, SD=11.23	Range 19-60 M=35.39, SD=10.36	-1.48
Ethnicity			
White	44 (90%)	94 (87%)	$\chi^2 (1, N=145)=0.034$
Non-White	2 (4%)	5 (5%)	
Unknown	3 (6%)	9 (8%)	
Relationship Status			
In a relationship	28 (57%)	58 (54%)	$\chi^2 (1, N=148)=0.001$
Not in a relationship	20 (41%)	42 (39%)	
Unknown	1 (2%)	8 (7%)	
Employment Status			
Paid employment	14 (29%)	30 (28%)	$\chi^2 (2, N=125)=0.401$
Unemployed	21 (43%)	45 (42%)	
Other	6 (12%)	9 (8%)	
Unknown	8 (16%)	24 (22%)	

Means and standard deviations on the measures at intake, pre- and post-GCAT for the 'non-completer' and 'completer' samples are shown in Table 6. Independent t-tests and Mann-Whitney U tests revealed no significant pre-GCAT differences between the non-completers and the completer samples on the BSI-GSI, $t(148)=0.41, p=n/s$, IIP-32, $t(145)=-0.52, p=n/s$, HADS-Depression, $t(149)=-0.92,$

$p=n/s$, HADS-Anxiety, $z=-0.67$, $p=n/s$ and GHQ, $z=-1.24$, $p=n/s$. However at the start of group completers demonstrated significantly higher self-esteem (RSES, $z=-3.02$, $p=0.003$) than non-completers.

Overall, the only difference between those who did, and did not complete GCAT, was that completers demonstrated higher self-esteem at the start of group.

Table 6; means and standard deviations at intake, pre and post-GCAT for the non-completer and completer samples

<i>Outcome measure</i>	Intake		Pre-GCAT		Post-GCAT		t/Z
	M	SD	M	SD	M	SD	
BSI-GSI							
Non-completer (n=47)	2.21	0.92	2.11	0.89	2.12	0.89	t(148)=0.41
Completer (n=103)	2.31	0.84	2.18	0.92	1.87	1.03	
IIP-32							
Non-completer (n=46)	2.00	0.69	1.96	0.70	1.96	0.70	t(145)=-0.52
Completer (n=101)	1.96	0.59	1.91	0.62	1.69	0.68	
RSES							
Non-completer (n=45)	15.42	5.95	16.36	5.48	16.42	5.46	-3.02*
Completer (n=102)	15.27	4.54	19.42	5.21	16.41	5.91	
HADS							
Depression							
Non-completer (n=48)	10.40	4.88	10.08	4.76	9.96	4.76	t(149)=-0.92
Completer (n=103)	11.71	4.53	11.18	4.61	9.13	5.37	
Anxiety							
Non-completer (n=48)	13.71	4.81	13.33	4.85	13.25	4.86	-0.67
Completer (n=104)	14.15	3.76	14.03	4.10	12.26	4.47	
GHQ							
Non-completer (n=49)	15.41	8.53	14.48	8.20	14.36	8.20	-1.24
Completer (n=101)	17.07	8.10	16.11	9.05	11.97	9.72	

Note. Baseline, pre-and post-GCAT *n* ranged due to missing data on some measures.

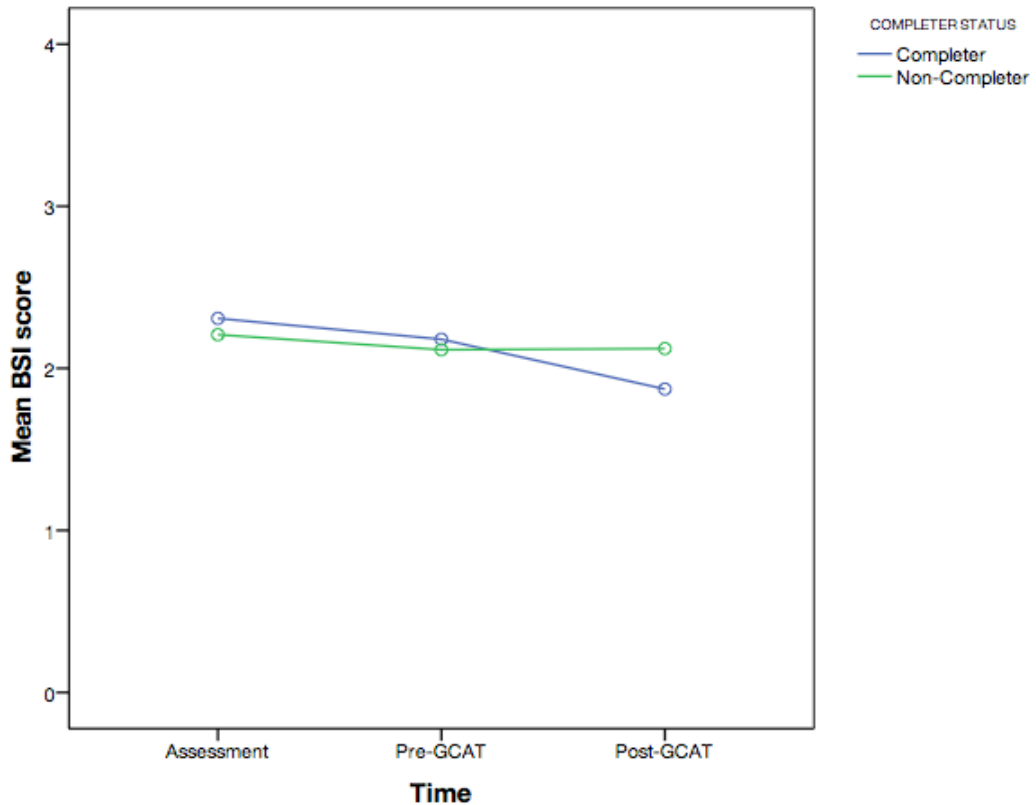
**p*<0.05

OUTCOMES OF GCAT*Statistical significance*

Two x three analysis of variance (ANOVA; completer status x time) to investigate change in the primary outcome measure of global functioning (BSI-GSI, see Table 6 for means and SDs) found a main effect of time ($F(1.76, 260.79)=9.93, p<0.001$), but not of completer status ($F(1, 148)=0.037, p=n/s$). Simple contrasts suggest that all those assigned to GCAT ($n=150$) demonstrated highly significant change from assessment to end of GCAT ($F(1, 148)=14.77, p<0.001$), with significant change observed from assessment to start of GCAT ($F(1, 148)=5.08, p=0.026$) and during GCAT ($F(1,148)=6.76, p=0.010$). This significant change observed during baseline consequently reduces the confidence with which change during GCAT can be attributed to the intervention.

There was also a significant interaction between completer status and time ($F(1.76, 260.79)=5.374, p=0.007$; see Figure 2), with simple contrasts suggesting that completers achieved significantly more therapeutic gains during GCAT (see Figure 2; $F(1,148)=7.43, p=0.007$).

Figure 2; graph to illustrate interaction between completer status and time as measured on the Brief Symptom Inventory (n=150)



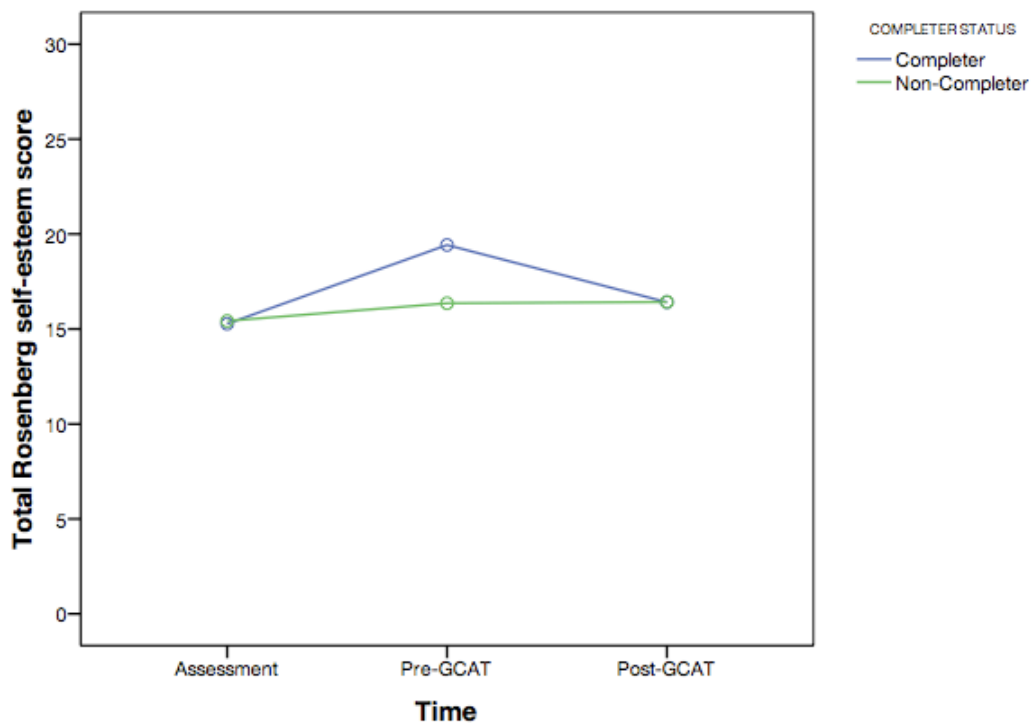
A 2x3 ANOVA (completer x time) also indicated a main effect of GCAT ($F(1.67, 242.07)=6.31, p=0.004$) on the IIP-32, a measure of interpersonal functioning. Of all those assigned to GCAT ($n=147$), no significant improvement was observed between assessment and pre-GCAT ($F(1,145)=1.55, p=n/s$), but a significant difference was observed during GCAT ($F(1,145)=5.75, p=0.018$). Therefore, it is with more confidence that it is possible to infer that interpersonal change achieved during GCAT was at least partially due to the intervention. No main effect was observed for completer status ($F(1,145)=1.49, p=n/s$), although completer status and time interacted ($F(1.67, 242.067)=4.22, p=0.022$) to suggest that completers achieved more change on the IIP during intervention ($F(1,145)=5.41, p=0.021$).

Highly significant main effects over time were observed for both sub-scales of the HADS; depression ($n=151$; $F(1.43, 212.35)=17.72, p < 0.001$), anxiety ($n=152$; $F(1.62, 243.02)=9.39, p < 0.001$). Simple contrasts revealed an overall significant improvement in depression scores during baseline ($F(1,149)=6.86, p=0.01$) and highly significant change in depression scores during GCAT ($F(1,149)=14.10, p < 0.001$). However, overall anxiety scores appeared stable during baseline ($F(1,150)=1.28, p=n/s$) with significant improvement in anxiety scores observed during GCAT ($F(1,150)=10.61, p=0.001$). No main effects were observed for completer status on either sub-scale: depression, $F(1,149)=0.45, p=n/s$; anxiety, $F(1,150)=0.01, p=n/s$. However, a significant interactions were observed on the depression ($F(1.43,212.35)=10.19, p < 0.001$) and anxiety sub-scale ($F(1.62, 243.02)=5.06, p=0.01$). Completers scores' improved significantly more than non-completers' scores during GCAT (Depression: $F(1,149)=11.06, p=0.001$; Anxiety: $F(1,150)=8.79, p=0.004$). No significant interaction was observed during baseline for either depression ($F(1,149)=0.44, p=n/s$) or anxiety scores ($F(1,150)=0.34, p=n/s$).

Self-esteem scores also showed a highly significant main effect of intervention over time ($F(1.80, 261.54)=12.21, p < 0.001$); for the assigned-to-GCAT sample ($n=147$) there was a highly significant increase in self-esteem scores whilst waiting for the group ($F(1,145)=20.85, p < 0.001$) and a significant difference was also observed during GCAT ($F(1,145)=12.14, p=0.001$). Overall, the difference in self-esteem scores between assessment and end of GCAT approached significance ($F(1,145)=3.67, p=0.057$). These results can be explained by examining the interaction between time and completers status ($F(1.80, 261.54)=6.20, p=0.003$). There was a significant difference between completers'/non-completers' RSES

scores during baseline ($F(1,145)=8.35, p=0.004$), with completers achieving significantly more gains. There was also a highly significant difference between completers'/non-completers' scores during GCAT ($F(1,145)=13.26, p<0.001$) but interestingly, not in the expected direction. During GCAT completers' self-esteem scores demonstrated significant deterioration (see Figure 3). No main effect for completer status was observed ($F(1,145)=1.65, p=n/s$).

Figure 3; graph to illustrate interaction between completer status and time as measured on the Rosenberg self-esteem scale ($n=147$)



Finally, on a measure of general well-being there was a highly significant main effect of time ($F(1.74, 256.88)=11.09, p<0.001$). Simple contrasts reveal no significant change in scores during baseline ($F(1,148)=3.27, p=n/s$) but a highly significant overall improvement in patients' scores ($n=150$) on the GHQ during GCAT ($F(1,148)=8.82, p=0.003$). No main effect of completer status was observed

($F(1,148)=0.05, p=n/s$), but completer status and time significantly interacted ($F(1.74, 256.88)=6.04, p=0.004$). No significant interaction was observed during baseline ($F(1, 148)=0.001, p=n/s$), but again completers made significantly more gains overall ($F(1,148)=7.37, p=0.007$) reflected in the significant gains completers achieved during GCAT ($F(1,148)=7.83, p=0.006$).

In summary, whilst waiting for the group, there was an overall trend for statistical improvement on measures of global distress, depression and self-esteem. During GCAT, depression and global distress continued to improve, with overall significant improvements also observed on measures of interpersonal functioning, anxiety and mental wellbeing. However, GCAT was associated with a significant deterioration in group members' self-esteem. Interaction effects consistently support previous findings that completers achieve more therapeutic gains than non-completers (e.g. Cahill et al., 2003).

Effect sizes

Tables 7 and 8 present the pre-GCAT means, change score, 95% confidence intervals and uncontrolled effect sizes for the assigned-to-GCAT sample (range 0.23-0.38) and completer sample (range 0.34-0.58) respectively. These results suggest that GCAT may be a promisingly effective intervention (Conway et al., 2003) in routine clinical practice for female survivors of CSA given, (a) the severity of initial distress reported, (b) the large pre-GCAT variance in scores and (c) patients' lack of therapeutic gains in previous, lower level service settings. However, the RSES effect size of 0.58 for the 'completer' sample indicates deterioration in levels of self-esteem for 'completers' during GCAT.

Table 7; outcome profile, mean pre/post difference and effect sizes for assigned-to-GCAT sample

Measure	N	Pre-GCAT Mean (SD)	Post-GCAT Mean (SD)	Pre-post change score	95% C.I. change	Effect Size
BSI-GSI	150	2.14 (0.91)	1.93 (1.00)	0.21	0.00-0.46	0.23
IIP-32	147	1.93 (0.65)	1.78 (0.69)	0.15	0.00-0.46	0.23
RSES	147	18.49 (5.47)	16.42 (5.76)	2.07	0.15-0.61	0.38
HADS						
<i>Depression</i>	151	10.83 (4.67)	9.39 (5.18)	1.44	0.08-0.54	0.31
<i>Anxiety</i>	152	13.81 (4.35)	12.58 (4.60)	1.23	0.06-0.51	0.28
GHQ-28	150	15.57 (8.78)	12.75 (9.29)	2.82	0.09-0.55	0.32

Note. n ranged from 147 to 152 due to missing data on some measures.

*** $p < 0.001$

Table 8; *outcome profile, mean pre/post difference and associated effect sizes for completer sample*

Measure	N	Pre-GCAT Mean (SD)	Post-GCAT Mean (SD)	Pre-post change score	95% C.I. change	Effect Size
BSI-GSI	103	2.18 (0.92)	1.87 (1.03)	0.31	0.06-0.61	0.34
IIP-32	101	1.91 (0.62)	1.69 (0.68)	0.22	0.07-0.63	0.35
RSES	102	19.42 (5.21)	16.41 (5.91)	3.01	0.29-0.86	0.58
HADS						
<i>Depression</i>	103	11.18 (4.61)	9.13 (5.37)	2.05	0.17-0.72	0.44
<i>Anxiety</i>	104	14.03 (4.10)	12.26 (4.47)	1.77	0.15-0.71	0.43
GHQ-28	101	16.11 (9.05)	11.97 (9.72)	4.14	0.17-0.74	0.46

Note. n ranged from 101 to 104 due to missing data on some measures.

*** $p < 0.001$

Benchmarking completer outcomes

Table 9 presents benchmarking results from within-subject studies of survivors completing group interventions. The BSI-GSI score for the GCAT 'completer' sample was almost 1 SD above the pre-therapy mean of the Saxe and Johnson (1999) and Lundqvist and Ojehagen (2001) studies, and 0.9 SD above the pre-therapy mean score of the long-term systemic therapy group (Lau & Kristensen, 2007). The completers' mean pre-GCAT BSI-GSI score was most similar to the long-term analytic group (Lau & Kristensen, 2007), scoring within 0.3 SD of their mean score.

Table 9; *pre- and post-therapy scores and effect sizes for group interventions for CSA survivors*

Author(s)	Group Treatment (Duration) Sample Size	Setting	Pre-therapy M (SD)	Post-therapy M (SD)	Effect size
Lubin (1998)‡	Trauma focused CBT (16 sessions); n=29	Community	113.31 (78.14)	86.69 (75.32)	0.34
Saxe and Johnson (1999)†	'Recovery' therapy (20 sessions); n=32	Clinical outpatient	1.62 (0.58)	1.03 (0.65)	1.02
Talbot (1999)‡	Trauma recovery therapy (10 sessions); n=20	Inpatient	57.45 (7.79)	49.90 (9.41)	0.97
Lundqvist and Ojehagen (2001)‡	Long-term psychodynamic therapy (2 years); n=22	Clinical outpatient	1.38 (0.80)	0.96 (0.80)	0.53
Lau and Kristensen (2007)‡	Analytic therapy ^a (46 sessions); n=40	Clinical outpatient	1.95 (0.75)	1.63 (0.77)	0.43
	Systemic therapy ^b (17 sessions); n=46		1.61 (0.61)	0.99 (0.69)	1.02

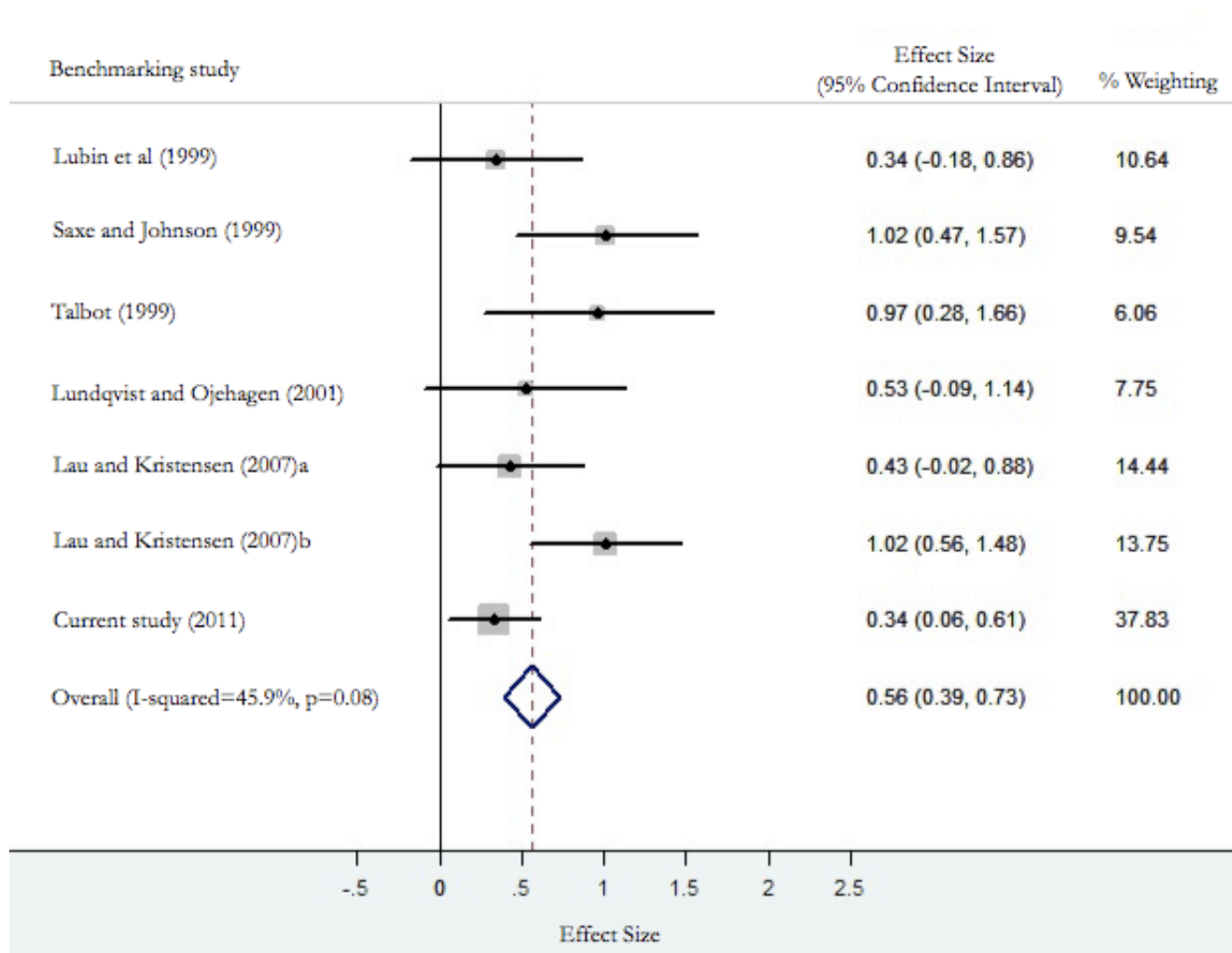
Table 9 continued

Current sample [†]	Cognitive Analytic Therapy informed group (24 sessions); n=103	Clinical outpatient	2.18 (0.92)	1.87 (1.03)	0.34
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‡SCL-90. †BSI

Figure 4 illustrates the uncontrolled effect size for each benchmarking study, together with the 95% confidence intervals. The study sample size (graphically represented by the grey boxes) determines the percentage weight that each study contributes to the overall benchmark. The current study yielded a within-study uncontrolled effect size of 0.34. This contributed 37.83% to the overall between-studies effect size of 0.56 (95% CI 0.39-0.73), represented by the dashed vertical line. Homogeneity between studies did not significantly differ ($p=0.08$), suggesting that it is appropriate to draw tentative comparative conclusions between studies. Results suggest that GCAT is not as effective as some other group interventions for female survivors in reducing global distress. The lack of information about treatment focus and varying patient and group characteristics, however, limits comparative conclusions that may be drawn. Furthermore, only completers' scores are used to calculate this overall between-studies uncontrolled effect size of group psychotherapy for female survivors.

Figure 4; forest plot of 7 within-study effect sizes for group interventions for CSA survivors using BSI/SCL-90-R and overall between-studies effect size



Reliable and Clinically Significant Change (RCSC)

Tables 10 and 11 present i) the rate of patients achieving RCSC during GCAT across all outcomes, ii) the rate of patients achieving reliable, but not clinically significant improvement during GCAT and iii) the rate of patients demonstrating reliable deterioration following GCAT, for those patients scoring above the clinical threshold at intake (see Table 2), for the assigned-to-GCAT and completer samples respectively. As would be expected the response rates are lower for the assigned-to-GCAT sample, due to the use of the LOCF method for non-attenders' and non-completers' scores.

As can be seen in table 11, approximately one in five completers achieved at least reliable improvement in global (21%; n=21) and interpersonal functioning (19%; n=15), with 8% (n=8) of patients scoring within a non-clinical population on the BSI-GSI at end of GCAT. On the HADS anxiety sub-scale, 26% (n=20) of completers achieved reliable improvement, of whom 14 (18%) recovered according to the more conservative criterion of reliable change from a clinical to a non-clinical population. The greatest improvement rate for the completer sample was observed in levels of depression (HADS-D: RCSC=34%, n=16). The completers achieved a recovery rate of 24% (n=20) on a measure of general mental wellbeing. Despite GCAT shown to have a statistically detrimental effect on the groups' overall level of self-esteem, 14% (n=14) of completers achieved RCSC in self-esteem and a further 11% (n=11) achieved reliable, but not clinically significant, improvement. No patients recovered in terms of interpersonal functioning, and 6% (n=6) of completers experienced a reliable deterioration in levels of global psychological distress following GCAT.

Table 10; rates of change achieved during GCAT for the ‘assigned-to-GCAT sample’ (n=157) scoring above the clinical threshold at intake

Measure	N	Recovery Reliable and clinically significant change (RCSC) n(%)	Reliable improvement only (RI) n(%)	No reliable change n(%)	Reliable deterioration n(%)
BSI-GSI ¹	137	8 (6%)	13 (9%)	109 (80%)	7 (5%)
IIP-32 ²	147	0	19 (13%)	126 (86%)	2 (1%)
RSES ³	139	14 (10%)	10 (7%)	111 (80%)	4 (3%)
HADS ⁴					
<i>Depression</i>	65	17 (26%)	3 (5%)	43 (66%)	2 (3%)
<i>Anxiety</i>	108	14 (13%)	6 (5%)	87 (81%)	1 (1%)
GHQ-28 ⁵	121	19 (16%)	9 (7%)	91 (75%)	2 (2%)

Note. n ranged from 65 to 147 due to missing data on some measures. Total assigned-to-GCAT sample n=157.

¹Brief Symptom Inventory – Global Severity Index ²Inventory of Interpersonal Problems-32 ³Rosenberg Self-Esteem Scale ⁴Hospital Anxiety and Depression Scale

⁵General Health Questionnaire

Table 11; rates of change achieved during GCAT for the completer sample scoring above the clinical threshold at intake

Measure	N	Recovery Reliable and clinically significant improvement (RCSI) n(%)	Reliable improvement only (RI) n(%)	No reliable change n(%)	Reliable deterioration n(%)
BSI-GSI ¹	98	8 (8%)	13 (13%)	71 (73%)	6 (6%)
IIP-32 ²	80	0	15 (19%)	64 (80%)	1 (1%)
RSES ³	102	14 (14%)	11 (11%)	74 (72%)	3 (3%)
HADS ⁴					
<i>Depression</i>	47	16 (34%)	3 (6%)	25 (53%)	2 (4%)
<i>Anxiety</i>	77	14 (18%)	6 (8%)	56 (73%)	1 (1%)
GHQ-28 ⁵	82	20 (24%)	8 (10%)	52 (63%)	2 (3%)

Note. n ranged from 47 to 102 due to missing data on some measures. Total completer sample n=108.

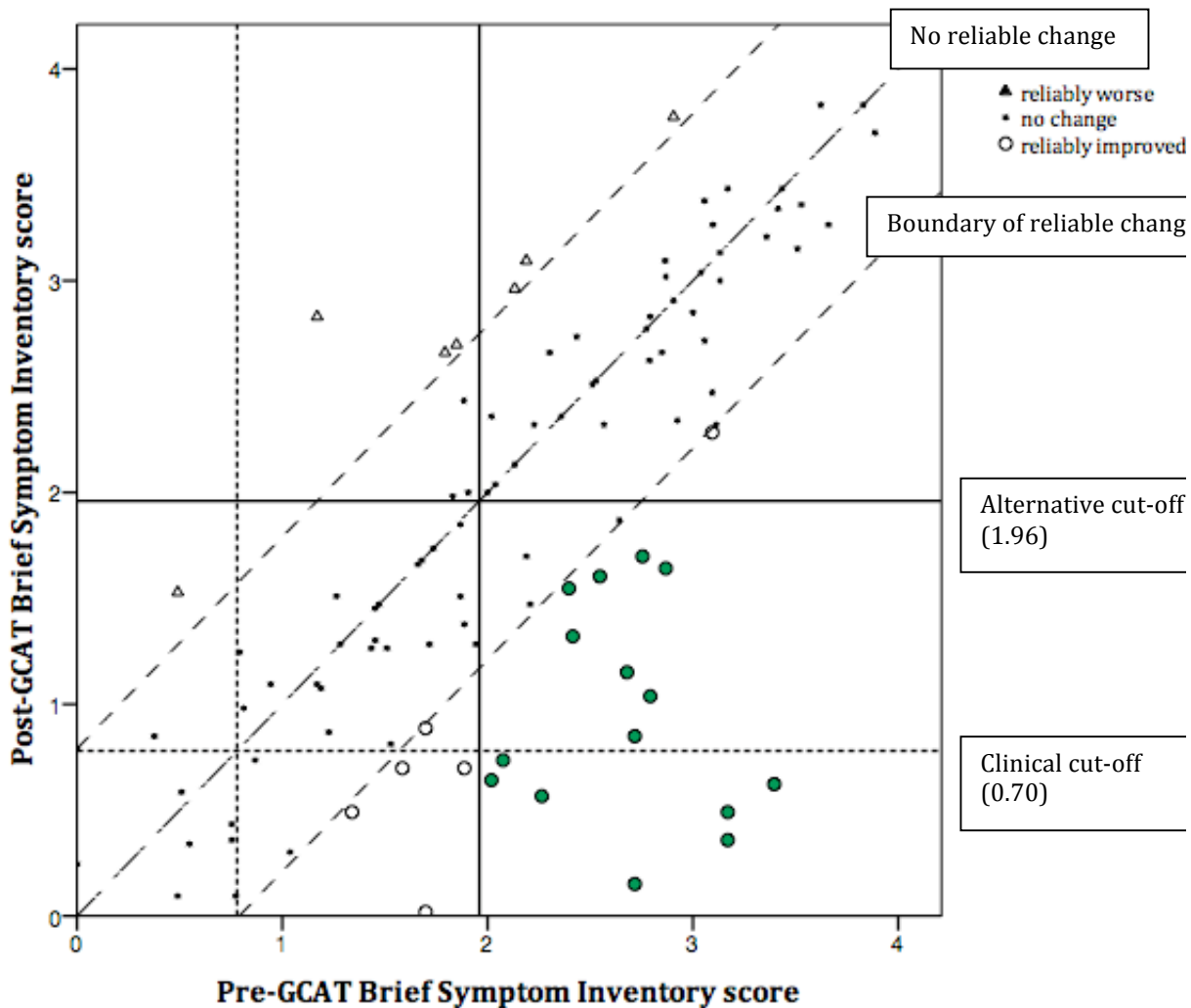
¹Brief Symptom Inventory – Global Severity Index ²Inventory of Interpersonal Problems-32 ³Rosenberg Self-Esteem Scale ⁴Hospital Anxiety and Depression Scale

⁵General Health Questionnaire

McNemar tests indicated that there was a significant difference in the proportion of completers achieving reliable improvement during baseline compared with those achieving RCI during GCAT on all outcome measures: RSES ($p=0.001$); BSI-GSI ($p=0.004$); GHQ ($p<0.001$); IIP-32 ($p=0.001$); HAD-Depression ($p<0.001$) and HAD-Anxiety ($p<0.001$). There was also a significant difference in the proportion of patients in the assigned-to-GCAT sample achieving reliable improvement during GCAT: BSI-GSI ($p=0.036$); IIP-32 ($p=0.002$); RSES ($p=0.009$); HADS-Depression ($p<0.001$); HADS-Anxiety subscale ($p<0.001$) and GHQ ($p=0.001$).

Figure 5 graphically represents the rate of completers achieving RCSC during GCAT on the BSI-GSI. Patients' pre-therapy scores are plotted on the x-axis against their post-therapy scores on the y-axis. The diagonal line through the origin represents no change achieved during therapy, with the broken diagonal tramlines either side representing the boundary for reliable change. Patients plotted above and below these broken tramlines indicate that change in their score is greater than would be expected given inherent measurement error, and change can thus be considered reliable. The dashed vertical and horizontal lines represent the clinical cut-off (0.70) pre- and post-therapy respectively, indicating 'caseness' at each time point. An alternative clinical cut-off of 1.96 was calculated to represent a more realistic threshold for improvement, using assigned-to-GCAT pre-therapy outcomes and outpatient norms (Ryan, 2007). The filled circles represent patients reliably moving from a severe clinical population to a less severe clinical population ($n=15$) using the alternative cut-off.

Figure 5; scatterplot of outcomes illustrating reliable and clinically significant change on the Brief Symptom Inventory for the 'completers' sample (N=105)



Discussion

This study represents the first attempt to evaluate GCAT for female survivors of childhood sexual abuse, utilising a dataset collected in clinical practice in a tertiary level psychotherapy service. The overall aim was to consider the effectiveness of GCAT for this sample and identify avenues for further investigation. Some encouraging findings indicated that GCAT, could be a promising intervention for female survivors experiencing severe levels of distress, as an adjunct to care provided in secondary mental health settings. The present

study suggests that empirical concepts of 'recovery' need to be developed to take into account levels of presenting distress that often occur in samples such as CSA survivors, where change to a non-clinical population is unrealistic, to meaningfully estimate the effectiveness of a psychotherapy approach.

Summary of findings

Eligible patients were referred to the service via normal care pathways and offered group work based on an assessment of clinical need. The only exclusion criteria imposed by the research was the need to have completed at least baseline measures. Results indicate that survivors in a relationship and demonstrating lower self-esteem were more likely to be offered group intervention. Of the 157 patients offered GCAT, 108 (69%) completed the intervention, (12% DNA and 19% dropped out). This is a relatively low dropout rate compared to 41% from a 6-month group for female survivors suffering from depression (Fisher et al., 1993), 38% from a systemic group (Lau & Kristensen, 2007) and 58% from 'trauma recovery' group (Talbot et al., 1999). Future research with survivors should consistently report attrition rates from interventions and explore factors predicting poorer outcomes.

Analysis indicated a trend for improvement in depression and global functioning during waitlist time, with evidence of continuing gains during GCAT. Interesting findings arose from exploring survivors' self-esteem scores. Comparisons showed that those assigned to GCAT had lower self-esteem than patients assigned to other interventions, and that non-completers of GCAT demonstrated significantly lower self-esteem than completers. Overall, there was a significant improvement in self-esteem scores following GCAT, however, exploring this finding in more detail illustrated that completers' self-esteem significantly

improved whilst waiting for the group, but GCAT was associated with a significant deterioration in completers' self-esteem. Previous CAT research with CSA survivors has also indicated a deterioration of self-esteem scores in those with a narcissistic personality structure (Clarke & Pearson, 2000). Group therapy is often a challenging environment in which personal learning and insight is facilitated, perhaps threatening a fragile sense of self, and this may account for the drop in self-esteem during the intervention. The self-esteem findings also illustrate a discrepancy between statistically significant change and clinically significant change. A quarter of completers demonstrated reliable, clinical improvement in self-esteem during GCAT. Further longitudinal research is required to better understand the relationship between GCAT and survivors' self-esteem over time in response to interventions.

Interpersonal functioning, anxiety and mental wellbeing appeared stable during baseline and improved during GCAT. The results suggest that those completing therapy achieved more gains, consistent with findings from other outcome studies (e.g. Cahill et al., 2003). Completer analysis indicated a moderate uncontrolled pre/post treatment effect size of 0.34 on the primary outcome measure (Conway et al., 2003), similar to that reported in an analytic treatment group for survivors reporting similar levels of distress (Lau & Kristensen, 2007). The effect size is lower than treatment effect sizes for other group interventions, yet differences between effect sizes may be due to factors such as variance in group climate or specific patient or therapist variables.

Approximately one in ten patients achieved recovery on the primary outcome measure during GCAT, with one in five achieving at least reliable improvement. However, almost one in twenty patients completing GCAT demonstrated reliable deterioration in global functioning during the intervention.

One in five of the 'completer' sample demonstrated reliable improvement in interpersonal difficulties, although no patients achieved reliable, clinically significant change in their levels of interpersonal problems.

Scores at intake illustrate the severity of disturbance experienced by this sample of CSA survivors. Such profile information raises the important question of which criterion of 'recovery' should be adopted in outcome research in estimating effectiveness of psychotherapy with populations that demonstrate severe and complex difficulties. There is a lack of consensus on what constitutes recovery for complex clients and there is a need to develop more sensitive measures of recovery according to context and diagnosis. This has been identified as one factor that has hindered the measuring, monitoring and management of outcomes in routine practice (Cape & Barkham, 2002). For a practice-based dataset to contribute convincingly to clinical care and development of services, there must be consideration of how to optimise meaningful implementation of outcome measures in practice and adhere to minimum data quality standards.

Methodological Critique

Cahill et al. (2010) differentiate between *efficacy*, *effectiveness* and *practice-based evidence* (PBE). The methodology developed for examining efficacy of psychotherapy has been largely based on the assumptions of a traditional medical model (Bower & Gilbody, 2010), whereby study designs aspire to the 'gold standard' of randomised controlled trials. There are serious limitations to applying such methodological strictures to practice-based contexts, which aim to reflect the reality of routine clinical settings to appraise the effectiveness of interventions and therefore place "*near-zero limitations*" (Barkham et al., 2010, p.39) on data inclusion. However, there are ubiquitous methodological

compromises associated with utilising data collected in routine clinical practice (Barkham & Margison, 2007), and results from this study should therefore be interpreted with due caution.

The study is characterised by low internal validity. An obvious limitation of the study is the lack of a comparison or control group to compare outcomes against. Although the study attempted to address this by use of a within group waitlist comparison, this was not an adequate control as it was not possible to account for the differing lengths of time that patients waited for GCAT or concurrent interventions received. Therefore, it is impossible to say with any level of certainty that the improvement observed for the patients assigned to GCAT and completers can be solely attributed to the intervention. The nature of this study did not permit systematic recording of concurrent interventions and therefore observed change may be a reflection of variables not measured in this study. Furthermore, it is difficult to make firm conclusions on the effectiveness of GCAT, given that it was delivered as an unstandardised intervention and no measure of fidelity to the GCAT model was undertaken. However, as Stiles et al., (2008; 2006) purport, methodological requirements such as assessing treatment fidelity and diagnostic validity are derived from efficacy trials and are arguably of limited value in a PBE context, where interventions are often a pragmatic and eclectic enterprise (Parry, 2000). This study also attempted to benchmark outcomes from GCAT with findings from other group effectiveness studies. However, given the paucity of outcome research for survivors, the range of available benchmarks was severely limited. Only tentative comparative conclusions can be made, since the benchmarking studies reported pre/post data and findings are restricted to completers.

A strength of this study is the large sample size and broad range of valid, reliable instruments employed (Westbury & Tutty, 1999), although these were all self-report questionnaires and thus fallible to bias in reporting. The study has high external validity, with findings drawn from a clinically representative data source (Shadish et al., 1997). Clients were referred through usual procedures, representing a heterogeneous population being treated in routine practice. Facilitators all offered GCAT to survivors as part of their regular clinical caseload, (Shadish et al., 1997). No follow-up data was available and this represents a major weakness of the study. Longitudinal follow-up data is required to evaluate the effectiveness of GCAT over time and also take into account broader variables such as subsequent use of services, risk and quality of life. Despite these limitations, there are some significant trends that warrant further investigation.

Statistically significant improvement

There was a trend for improvement in depression, self-esteem and global functioning prior to GCAT. Previous research has highlighted the therapeutic impact of collaborative assessment (Finn & Tonsager, 1997), although the associated change mechanisms remain largely unknown (Poston & Hanson, 2010).

Depression and global functioning continued to improve during intervention; mean global distress reduced from a highly severe to a less severe category during GCAT. However, the trend for remission over time significantly reduces the confidence that change in global distress and depression can be attributed to GCAT.

The trend for self-esteem scores to significantly deteriorate during GCAT warrants further investigation. It may be that the construct of self-esteem is particularly vulnerable in this population being treated in a group context. No

change was observed pre-GCAT in interpersonal functioning, anxiety and mental wellbeing, which all significantly improved during intervention. Further research is required to elucidate the mechanisms of change. Given the limitations outlined above, what may be tentatively suggested is that GCAT appears a promising adjunct to survivors' package of care and certainly merits future methodologically sound research.

Benchmarking outcomes

The results suggest that completers achieve more gains than non-completers. Intention to treat analysis suggested an uncontrolled effect size of 0.23 on a measure of global functioning. Patients completing therapy (N=108, 69% of assigned-to-GCAT sample) demonstrated an uncontrolled effect size of 0.34, suggesting that GCAT may be a moderately effective intervention in routine practice for female survivors (Conway et al., 2003).

Yet, this difference between completers and non-completers may be an artefact of employing LOCF. In effect, carrying forward the last observation in this way reduces the likelihood of finding a significant effect of the intervention (Barkham et al., 2011). LOCF is more appropriate in studies that employ regular (e.g. session-by-session) monitoring of outcomes. In the current study there were only three time-points of data collection and using LOCF to calculate change scores assumed that patients who did not complete therapy, or who did not fill in end of therapy measures, did not improve at all during GCAT. The results may therefore be unrealistically conservative.

The effect sizes in the current study were lower than 'recovery therapy' (Saxe & Johnson, 1999) and systemic therapy (Lau & Kristensen, 2007). An identical effect size to that in the current study was achieved in the community

sample receiving 16-session CBT (Lubin, 1998). It appears that GCAT may be less effective than other group interventions for survivors completing therapy. However, it is important to consider differences between the studies (Lucock, Leach, Iveson, Lynch, Horsefield, & Hall, 2003), as outcomes are moderated by factors such as treatment focus, practice settings, patient characteristics and group climate (Ogrodniczuk, Piper, & Joyce, 2006). Although empirical literature consistently suggests that group interventions can be effective for survivors (Alexander et al., 1989; Hazzard, Rogers, & Angert, 1993; Westbury & Tutty, 1999; Kessler, White & Nelson, 2003; Taylor & Harvey, 2010), further research is required to isolate the active components of effective group treatment for this population (Classen et al., 2011). The 'recovery' (Saxe & Johnson, 1999) and systemic group therapy (Lau & Kristensen, 2007) were delivered to a less distressed population. The GCAT sample profile was most similar to the analytic group study (Lau & Kristensen, 2007), which also reported a moderate effect size (Conway et al., 2003).

There is a paucity of efficacy research for survivors (Classen et al., 2011) to contextualise findings from effectiveness studies. Furthermore, effect sizes are expected to be smaller in practice-based studies (Lueger & Barkham, 2010; Barkham et al., 2008), due to the larger variance in scores resulting from the absence of systematic inclusion/exclusion criteria in routine practice. In the current context the fact that the clinical service operates at a tertiary level means that clients have accessed care lower down the stepped care model and found interventions offered as ineffective or unhelpful. Consequently, although 0.34 appears a somewhat small effect size, given the levels of distress, lack of previous gains from therapeutic interventions and large variance within the sample this

effect size is actually quite promising and certainly indicates a need for further empirical investigation.

Reliable and Clinically Significant Change

Statistical significance has limited bearing on the clinical meaningfulness of findings (Jacobson & Truax, 1991). Arguably, an effective intervention should produce reliable and clinically significance change. This study evaluated both *improvement* (reliable change) and *recovery* (reliable and clinically significant change), as a patient can reliably improve and yet still score within a clinical sample at the end of treatment (Beutler & Moleiro, 2001).

Recovery can be conceptualised in various ways, which clearly impacts inferred estimates of effectiveness, and there is as yet no agreed definition in the empirical literature for how effectiveness should be construed in practice-based research. This study took a stringent definition of recovery; reliable change from a clinical to a non-clinical population. Employing less stringent recovery criteria (i.e. reliable improvement only) improved the recovery rate of GCAT.

More patients achieved reliable improvement across all measures during GCAT than baseline, suggesting that GCAT is of promising clinical utility for female survivors. In line with Marriott and Kellett's (2009) study of 1:1 CAT in routine practice, GCAT appeared to be of most clinical utility in improving depression and general mental health, with the least clinical change observed on the IIP-32. Varying rates of improvement across measures highlights the impact of choice of measure on perceived effectiveness of intervention. There is no agreed battery of outcome measures to be used with survivors or in wider CAT research and this represents a key development area for the field.

It is an interesting finding that GCAT had the least impact on interpersonal functioning given that CAT is described as a relational therapy (Ryle & Kerr, 2002). However, the test-retest reliability of the IIP-32 is low (Kellett, 2007) making it difficult to achieve reliable change. Alternatively, limited change on the IIP-32 may reflect the process of increasing psychological insight; as patients' symptoms improve, so too does their awareness of difficulties in relationships (Conway et al., 2003) reflected in their higher ratings of interpersonal difficulties. The low rates of change may be due to the brevity of the current intervention given the severity and complexity of patient characteristics; over 3 out of 4 of the sample scored within the clinical population on the IIP-32 at intake. Previous research has suggested that patients with more severe difficulties (depression, global distress, interpersonal problems) require longer in group therapy to achieve improvements in interpersonal functioning (Lorentzen & Høglend, 2008). Budman and Gurman (1988) suggested 65 sessions, of 90 minutes duration for patients with severe and enduring difficulties in intimate relationships, which far exceeds the 24-session format evaluated in the current study. Further research is required to determine the optimum length of GCAT treatment duration for survivors.

Both improvement and recovery rates as indicators of effectiveness are dependent upon the parameters employed. As Barkham et al. (2011) demonstrate, the parameters employed to calculate RCSC introduce arbitrary variation. For example, had reliable change indices been calculated using Cronbach's α rather than the more stringent test-retest co-efficient, the RCI for the BSI-GSI would have been lower, therefore requiring less change during GCAT to be considered reliable improvement. Various methods for calculating a clinical threshold value have been advocated (Evans et al., 1998). This study utilised stringent published clinical cut-offs where available.

There is a distinct lack of evidence regarding the most appropriate clinical threshold to apply to those experiencing severe mental health difficulties (Cape & Barkham, 2002) in services adopting the 'recovery model' (NIMHE, 2005). Previous small-scale CAT research with female survivors reported that despite improvements, four out of eight patients remained in a clinical sample following termination of 16-session CAT (Clarke & Llewelyn, 1994). Mental health services aim to increase patients' quality of life, whilst acknowledging that many people with severe and enduring mental health problems may remain in a clinical population. Building robust and relevant practice-based evidence requires appropriate comparative data from referential samples and clinically meaningful thresholds.

Clinical Implications

Research clearly indicates the heterogeneous difficulties experienced by survivor populations (Pribor & Dinwiddie, 1992; Saunders, Villeponteaux, Lipovsky, Kilpatrick, & Veronen, 1992). Overall, this study suggests that GCAT is a promising intervention for female survivors, as an adjunct to care received from secondary mental health services. GCAT appears particularly effective in ameliorating survivors' difficulties with depression and for those demonstrating higher levels of interpersonal distress. Group psychotherapy has intrinsic pragmatic relevance in the current NHS context (Maple & Simpson, 1995) due to cost and economic implications. The development of a GCAT treatment manual will enable more methodologically rigorous evaluations of the efficacy and effectiveness of GCAT across the stepped model of care with diverse populations.

Within the methodological limitations already outlined, organisational implications may be cautiously inferred (Lucock et al., 2003). It is apparent that

routinely measuring and monitoring clinical outcomes in practice represents a significant challenge to the current infrastructure and culture of traditional NHS mental health services (Rao, Hendry, & Watson, 2010). Research efforts have explored barriers to implementing routine outcome measurement, highlighting the need to bring about a culture shift in attitudes towards outcome monitoring (Trauer, Callaly, & Herrman, 2009; Garland, Kruse, & Aarons, 2003). This research suggests that it would be helpful for the service to clarify a systematic process for administering and recording routine outcome data and consider ways in which practitioners' and patients' ownership of data collection procedures may be increased to improve data quality (Barkham et al., 2010; Barkham & Mellor-Clark, 2003).

To drive quality improvements in services, a practice-based research paradigm should be integral to the planning of services. Barkham (2007) notes that establishing practice-based structures requires an organisational commitment incorporating both 'top-down' and 'bottom-up' impetus. Payment by Results (DoH, 2010) represents a clear top-down directive. Bottom-up initiatives have included the establishment of practice research networks (PRNs; Parry, Castonguay, Borkovec, & Wolf, 2010). Given the likelihood that CAT is being used with survivors across the UK, the CAT community may consider establishing a 'CAT with survivors PRN'.

Consideration must be given to the acceptability and feasibility of outcome measures administered in clinical practice (Cape & Barkham, 2002). There is stark lack of research exploring patients' views and experiences of complying with routine outcome measurement systems (Happell, 2008). Preliminary findings have shown that patients are less likely to comply with outcome monitoring if they are

unhappy with the process (Stedman et al., 2000). Services should involve patients in contributing to the development of meaningful outcome systems.

Theoretical Implications

The quality of object relations has been shown to significantly moderate outcome following group therapy (Piper et al., 2001). Survivors of CSA arguably have poorer quality object relations, which have been internalised to form damaging reciprocal role procedures. Llewelyn (2002) argues that CAT may be particularly useful for working with survivors due to the span of its theoretical base. GCAT aims to link reciprocal roles and TPPs within the context of the patient's history and current relationships (Maple & Simpson, 1995). Stowell-Smith, Gopfert, and Mitzman (2001) hypothesised that "multiple reciprocal role enactments" emerge in group dynamics and GCAT provides a framework for actively processing these in the context of the here-and-now of the group, thereby facilitating change as patients renegotiate an internal relational model. Perhaps one of the most crucial theoretical implications of the study is the recognition that there is currently a lack of an agreed conceptual CAT model for survivors to guide treatment (Llewelyn, 1997). Although CSA is not a disorder per se, the active tools and associated explicit structure of CAT have been considered particularly helpful for survivors (Llewelyn, 2002; Clarke & Pearson, 2000; Clarke & Llewelyn, 1994). Developing a theoretically coherent CAT model for working with survivors would be a fruitful enterprise and underpin future clinical and research endeavours with this population. Furthermore, development and testing of a group CAT model is clearly indicated.

Future research

Clearly there is a need for more research regarding GCAT. Some future research avenues have already been identified: developing a CAT model for survivors; longitudinal research of GCAT with survivors particularly focusing on short and long-term follow-up; comparing GCAT with other group interventions delivered in clinical practice; exploring effect of group climate on outcome for survivors; and investigating optimal 'dose' of therapy. An investigation of GCAT in less distressed CSA survivor populations is indicated from the current PBE. The findings from this study are limited to female survivors and future research should prioritise investigating outcomes for male survivors, a much under-represented research population (O'Leary & Gould, 2010).

Barkham and Parry (2008) note that although practice-based evidence is commonly conceptualised as complementing evidence-based research (and vice versa), there is a danger that without adequate cross-fertilisation between the paradigms, they remain disparate endeavours and this vision is not realised. A practice-based trial of GCAT for survivors, incorporating randomisation, is indicated from the current study, which would stimulate further effectiveness research (Barkham & Mellor-Clark, 2003). However, if practice-based datasets are going to meaningfully contribute to a cumulative evidence-base, such datasets need to adhere to stringent minimum data capture requirements.

Conclusion

Over the past 30 years, with the growing awareness of the prevalence of CSA and its associated psychological toxicity, has come an increasing need for mental health services to appropriately respond to survivors' distress. This study is the first to report on the effectiveness of GCAT delivered in routine clinical practice to female

survivors of CSA, and benchmark findings against comparative group psychotherapy data. GCAT was associated with lower dropout rates than other group interventions for survivors, suggesting that it is an acceptable treatment. It transpires that a dataset collected in routine clinical practice was of only limited utility in testing a set of hypotheses regarding the effectiveness of the intervention. There was a tentative trend that indicated that survivors benefited from GCAT. Some survivors achieved 'recovery', based on a stringent criterion for change (reliable movement from a clinical to non-clinical population). GCAT appeared of most utility in ameliorating survivors' depression with only limited change achieved on a measure of interpersonal functioning.

In terms of CAT, whilst the approach seeks to remain innovative and relevant to the current NHS context, it currently lacks a sufficiently robust evidence-base (Margison, 2000). There is no agreed model of CAT for survivors, or a model of group CAT. It is imperative that research efforts continue to offer cumulative evidence that aids the development of CAT theory, and ultimately effective treatments for survivors.

In conclusion, within the severe constraints posed by this data set, there are indications that suggest that GCAT appears a promising tertiary level intervention for adult female survivors of CSA, as an adjunct to complex and varied care offered in secondary care settings, when pre-treatment levels of psychological distress and the service setting are taken into account. These trends certainly warrant further investigation. It is imperative that research efforts continue to strive to ultimately improve the overall quality of care offered to those struggling with the psychological damage of being sexually abused as a child.

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Appendix i; Journal approval letter



Department Of Psychology. Clinical Psychology Unit.

Doctor of Clinical Psychology (DClin Psy) Programme
Clinical supervision training and NHS research training
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21st February 2011

Rachel Simmonds
Third year trainee
Clinical Psychology Unit
University of Sheffield

Dear Rachel

I am writing to indicate our approval of the journal(s) you have nominated for publishing work contained in your research thesis.

Literature Review: Psychology and Psychotherapy: Theory, Research and Practice

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Please ensure that you bind this letter and copies of the relevant Instructions to Authors into an appendix in your thesis.

Yours sincerely

A handwritten signature in black ink, appearing to read 'Rebecca Knowles', written on a light blue background.

Dr Rebecca Knowles
Research Tutor

Psychology and Psychotherapy: Theory, Research and Practice

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Author Guidelines

Psychology and Psychotherapy: Theory Research and Practice (formerly The British Journal of Medical Psychology) is an international scientific journal with a focus on the psychological aspects of mental health difficulties and well-being; and psychological problems and their psychological treatments. We welcome submissions from mental health professionals and researchers from all relevant professional backgrounds. The Journal welcomes submissions of original high quality empirical research and rigorous theoretical papers of any theoretical provenance provided they have a bearing upon vulnerability to, adjustment to, assessment of, and recovery (assisted or otherwise) from psychological disorders. Submission of systematic reviews and other research reports which support evidence-based practice are also welcomed, as are relevant high quality analogue studies. The Journal thus aims to promote theoretical and research developments in the understanding of cognitive and emotional factors in psychological disorders, interpersonal attitudes, behaviour and relationships, and psychological therapies (including both process and outcome research) where mental health is concerned. Clinical or case studies will not normally be considered except where they illustrate particularly unusual forms of psychopathology or innovative forms of therapy and meet scientific criteria through appropriate use of single case experimental designs.

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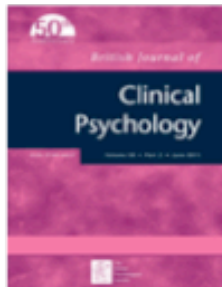
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The following types of paper are invited:

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Papers should normally be no more than 5000 words (excluding abstract, reference list, tables and figures), although the Editor retains discretion to publish papers beyond this length in cases where the clear and concise expression of the scientific content requires greater length.

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- Manuscripts should be preceded by a title page which includes a full list of authors and their affiliations, as well as the corresponding author's contact details. A template can be downloaded from [here](#).
- Tables should be typed in double spacing, each on a separate page with a self-explanatory title. Tables should be comprehensible without reference to the text. They should be placed at the end of the manuscript with their approximate locations indicated in the text.
- Figures can be included at the end of the document or attached as separate files, carefully labelled in initial capital/lower case lettering with symbols in a form consistent with text use. Unnecessary background patterns, lines and shading should be avoided. Captions should be listed on a separate sheet. The resolution of digital images must be at least 300 dpi.
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headings: Objective, Method, Results, Conclusions. There should be no more than one table or figure, which should only be included if it conveys information more efficiently than the text. Title, author name and address are not included in the word limit.

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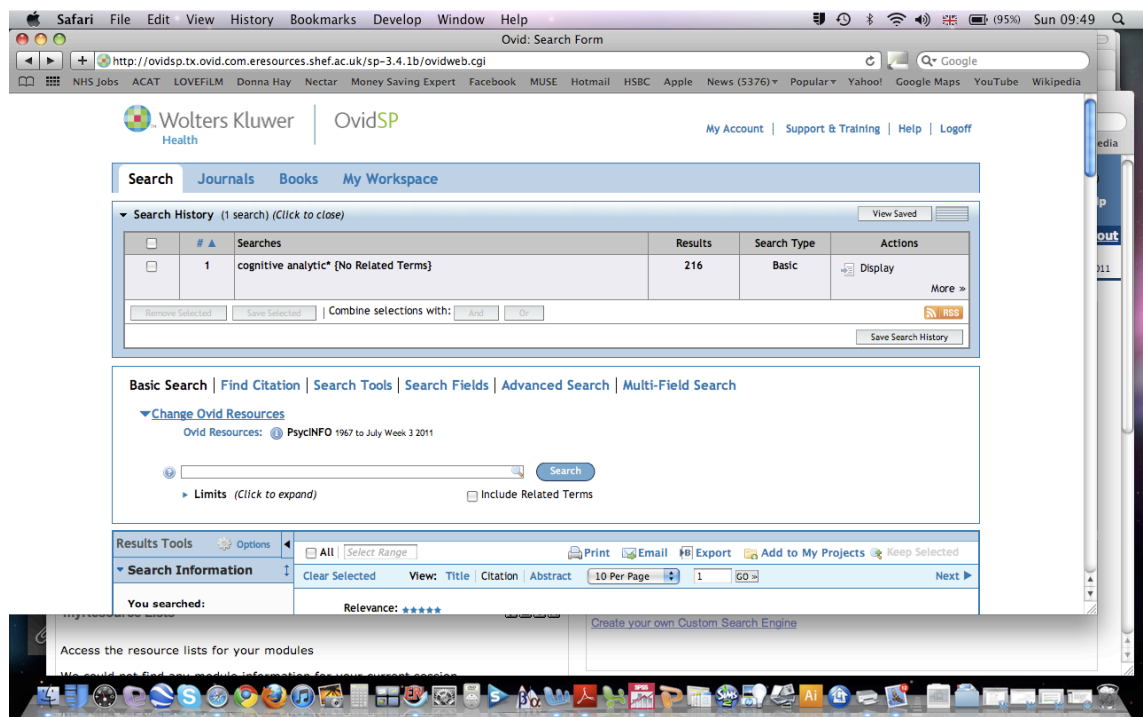
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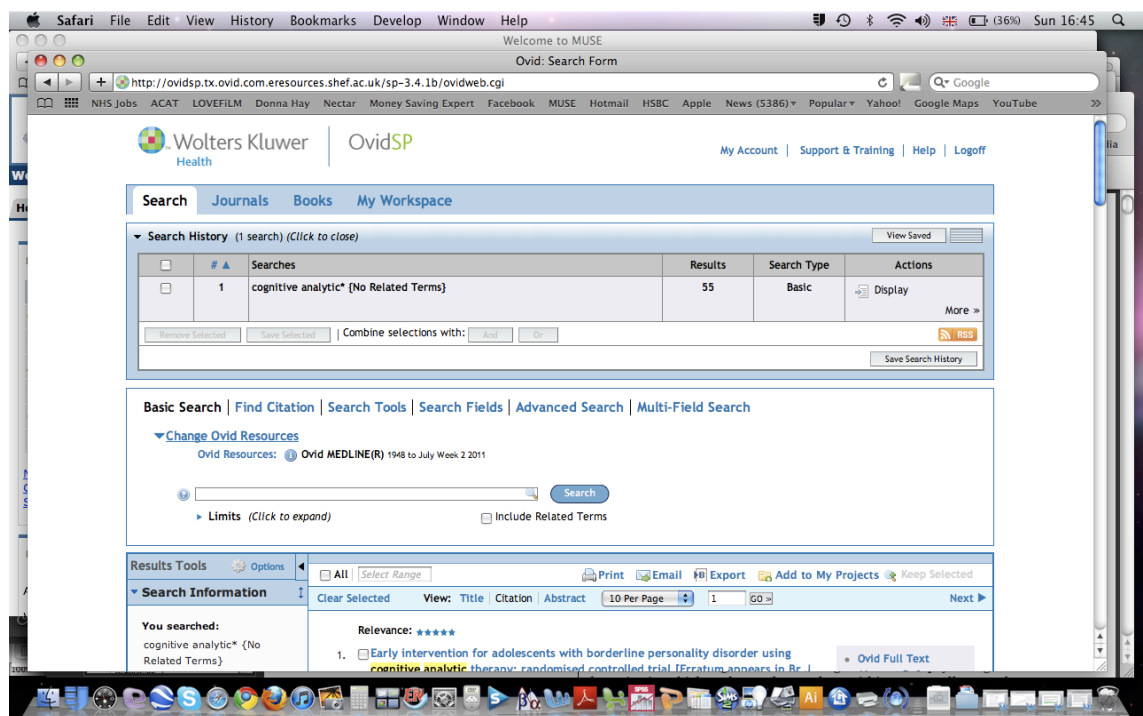
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Appendix iii; output of search term "Cognitive analytic"

Screen shot of output from PsychInfo search (1967-2011)



Screen shot of output from MEDLINE search (1948-2011)



Appendix iv; quality rating checklist (Downs & Black, 1998)

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Appendix v; sub-scale and total quality rating scores for studies included in the review clustered according to treatment focus

Quality rating sub-scale scores ranged from 0 (external validity subscale score for Dissociative Identity Disorder study) to 71% (n=4.25, internal validity sub-scale score for Eating Disorder studies; n=5.00, internal reliability sub-scale score for Childhood Sexual Abuse studies). Overall, studies addressed 45% (n=15.48) of the total quality criteria, which included a measure of power. On average, studies reported approximately half of the 'reporting' (n=5.71; 52%) and 'internal reliability' (n=3.71; 53%) criteria. Studies only achieved 32% (n=0.95) of external validity criteria and 24% (n=1.42) of internal validity criteria outlined in the checklist.

Appendix v cont'd; Mean sub-scale and total scores (%) of quality criteria addressed by studies grouped according to treatment focus

	<i>Reporting</i> ³⁷	<i>External validity</i> ³⁸	<i>Internal reliability</i> ³⁹	<i>Internal validity</i> ⁴⁰	Total ⁴¹
Treatment focus (number of studies)	n=11 (100%)	n=3 (100%)	N=7 (100%)	n=6 (100%)	n=32 (100%)
Personality Disorders (n=8)	7.00 (64%)	0.69 (23%)	4.06 (58%)	1.94 (32%)	16.38 (51%)
Common Mental Health Problems (n=4) ^Δ	4.17 (38%)	0.67 (22%)	3.88 (55%)	1.13 (19%)	14.25 (45%)
Eating Disorders (n=2) ^Δ	6.50 (59%)	1.25 (42%)	3.25 (46%)	4.25 (71%)	20.25 (63%)
Survivors of Childhood Sexual Abuse (n=2)	5.50 (50%)	2.00 (67%)	5.00 (71%)	0.50 (8%)	15.00 (47%)
Long-term Physical Health Difficulties (n=2)	7.50 (68%)	1.50 (50%)	4.50 (64%)	1.00 (17%)	17.50 (55%)
Morbid Jealousy (n=1)	6.00 (55%)	1.00 (33%)	4.00 (57%)	1.00 (17%)	13.00 (41%)
Dissociative Identity Disorder (n=1)	7.00 (64%)	0	4.00 (57%)	1.00 (17%)	13.00 (41%)
Dissociative Psychosis (n=1) ^Δ	2.00 (20%)	0.50 (17%)	1.00 (14%)	0.50 (8%)	5.00 (16%)
Mean (n=21)	5.71 (52%)	0.95 (32%)	3.71 (53%)	1.42 (24%)	15.48 (45%)

³⁷ was sufficient information was provided to allow the reader to make an unbiased assessment of findings?

³⁸ how generalisable are findings?

³⁹ what measurement or intervention biases were operating that may have confounded findings?

⁴⁰ which selection biases were operating that could have confounded findings?

⁴¹ The power subscale (range 0-5) contributes to the total quality score

^Δ Where more than one rater's scores were available for a paper, an average score across raters was calculated for that paper that then contributed to the overall mean score. For example, eight personality disorder studies (one of which averaged two raters' scores) contributed to the external validity sub-scale yielding a total score of 5.5, and therefore a mean score of 0.69 (23%).

Appendix vi; letters of research approval

South Yorkshire Research Ethics Committee	
Millside Mill Pond Road Meanwood Leeds LS6 4RA	
Telephone: 0113 2065652 Facsimile: 0113 2066772	
02 September 2010	
Miss Rachel Simmonds Trainee Clinical Psychologist Sheffield Health and Social Care Foundation Trust Clinical Psychology Unit Department of Psychology University of Sheffield Sheffield S10 2TP	
Dear Miss Simmonds	
Study Title:	Survivors of Childhood Sexual Abuse: Experience of the outcome monitoring process & outcomes of group Cognitive Analytic Therapy
REC reference number:	10/H1310/50
Protocol number:	URMS: 128528
Thank you for your letter of 05 August 2010, responding to the Committee's request for further information on the above research and submitting revised documentation.	
The further information has been considered on behalf of the Committee by the Chair.	
Confirmation of ethical opinion	
On behalf of the Committee, I am pleased to confirm a favourable ethical opinion for the above research on the basis described in the application form, protocol and supporting documentation as revised, subject to the conditions specified below.	
Ethical review of research sites	
The favourable opinion applies to all NHS sites taking part in the study, subject to management permission being obtained from the NHS/HSC R&D office prior to the start of the study (see "Conditions of the favourable opinion" below).	
Conditions of the favourable opinion	
The favourable opinion is subject to the following conditions being met prior to the start of the study.	
<u>Management permission or approval must be obtained from each host organisation prior to the start of the study at the site concerned.</u>	
For NHS research sites only, management permission for research ("R&D approval") should be obtained from the relevant care organisation(s) in accordance with NHS research governance arrangements. Guidance on applying for NHS permission for research is	

available in the Integrated Research Application System or at <http://www.rdforum.nhs.uk>.

Where the only involvement of the NHS organisation is as a Participant Identification Centre (PIC), management permission for research is not required but the R&D office should be notified of the study and agree to the organisation's involvement. Guidance on procedures for PICs is available in IRAS. Further advice should be sought from the R&D office where necessary.

Sponsors are not required to notify the Committee of approvals from host organisations.

It is the responsibility of the sponsor to ensure that all the conditions are complied with before the start of the study or its initiation at a particular site (as applicable).

Approved documents

The final list of documents reviewed and approved by the Committee is as follows:

<i>Document</i>	<i>Version</i>	<i>Date</i>
Investigator CV	R Simmonds	01 June 2010
Protocol	3	22 April 2010
Supervisors CV	S Kellett	07 June 2010
Response letter addressing points raised in provisional opinion letter		05 August 2010
REC application		
Covering Letter		06 June 2010
Questionnaire: General Health Questionnaire	GHQ28	
Letter of invitation to participant	3	22 April 2010
Participant Information Sheet	4	05 August 2010
Response to Request for Further Information		05 August 2010
Participant Consent Form	4	05 August 2010
Participant Consent Form: Focus Group	4	05 August 2010
Questionnaire: Brief Symptom Inventory		
Questionnaire: Inventory of Interpersonal Problems		
Questionnaire: Anxiety Scale		
Questionnaire: Self Esteem Scale		
Invitation to results presentation	3	22 April 2010
Powerpoint Presentation	1	22 April 2010
Evidence of insurance or indemnity		30 June 2010
Referees or other scientific critique report		21 May 2010

Statement of compliance

The Committee is constituted in accordance with the Governance Arrangements for Research Ethics Committees (July 2001) and complies fully with the Standard Operating Procedures for Research Ethics Committees in the UK.

After ethical review

Now that you have completed the application process please visit the National Research Ethics Service website > After Review

You are invited to give your view of the service that you have received from the National Research Ethics Service and the application procedure. If you wish to make your views known please use the feedback form available on the website.

The attached document "*After ethical review – guidance for researchers*" gives detailed guidance on reporting requirements for studies with a favourable opinion, including:

- Notifying substantial amendments
- Adding new sites and investigators
- Progress and safety reports
- Notifying the end of the study

The NRES website also provides guidance on these topics, which is updated in the light of changes in reporting requirements or procedures.

We would also like to inform you that we consult regularly with stakeholders to improve our service. If you would like to join our Reference Group please email referencegroup@nres.npsa.nhs.uk.

10/H1310/50	Please quote this number on all correspondence
--------------------	---

Yours sincerely

**Miss Jo Abbott
Chair**

Email: Elaine.hazell@leedsth.nhs.uk

<i>Enclosures:</i>	"After ethical review – guidance for researchers" SL-AR2
<i>Copy to:</i>	<i>Lauren Smaller, University of Sheffield, Academic Services, New Spring House, 231 Glossop Road, Sheffield, S10 2GW</i> <i>Sheffield Health & Social Care Foundation Trust, Research Development Unit, Fulwood House, Old Fulwood Road, Sheffield, S10 3TH</i>



National Research Ethics Service

South Yorkshire Research Ethics Committee

Millside
Mill Pond Lane
Meanwood
Leeds
LS6 4RA

Tel: 0113 305 0116

01 March 2011

Miss Rachel Simmonds
Trainee Clinical Psychologist
Sheffield Health and Social Care Foundation Trust
Clinical Psychology Unit
Department of Psychology
University of Sheffield
S10 2TP

Dear Miss Simmonds

Study title: Survivors of Childhood Sexual Abuse: Experience of the outcome monitoring process & outcomes of group Cognitive Analytic Therapy
REC reference: 10/H1310/50
Protocol number: URMS: 128528
Amendment number: 1
Amendment date: 24 January 2011

The above amendment was reviewed by the Sub-Committee in correspondence.

Ethical opinion

The members of the Committee taking part in the review gave a favourable ethical opinion of the amendment on the basis described in the notice of amendment form and supporting documentation.

Approved documents

The documents reviewed and approved at the meeting were:

Document	Version	Date
Letter	1	23 January 2011
Notice of Substantial Amendment (non-CTIMPs)		24 January 2011

Membership of the Committee

The members of the Committee who took part in the review are listed on the attached sheet.

R&D approval

This Research Ethics Committee is an advisory committee to the Yorkshire and The Humber Strategic Health Authority
The National Research Ethics Service (NRES) represents the NRES Directorate within the National Patient Safety Agency and Research Ethics Committees in England

All investigators and research collaborators in the NHS should notify the R&D office for the relevant NHS care organisation of this amendment and check whether it affects R&D approval of the research.

Statement of compliance

The Committee is constituted in accordance with the Governance Arrangements for Research Ethics Committees (July 2001) and complies fully with the Standard Operating Procedures for Research Ethics Committees in the UK.

10/H1310/50:	Please quote this number on all correspondence
---------------------	---

Yours sincerely



Miss Claire Kelly
Committee Assistant Co-ordinator

E-mail: Claire.kelly@leedspft.nhs.uk

Enclosures: List of names and professions of members who took part in the review

Copy to: Miss Lauren Smaller

Appendix vii; measures

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- *Brief Symptom Inventory* (BSI; Derogatis & Melisaratos, 1983)
- *Inventory of Interpersonal Problems-32* (IIP-32; Barkham, Hardy, & Startup, 1996)
- *Rosenberg Self-Esteem Scale* (RSES; Rosenberg, 1989)
- *Hospital Anxiety and Depression Scale* (HADS; Zigmond & Snaith, 1983)
- *General Health Questionnaire-28* (GHQ-28; Goldberg & Hillier, 1979).

Appendix viii; *'lessons learnt to survive': A conceptual tool developed to support GCAT formulation based on the traumgenic dynamics model (Hagan & Gregory, 2001)*

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