British Journal of Psychiatry (1984), **145**, 254–262

Review Article

Cognitive-Behaviour Therapy for Depression: Problems and Perspectives

J. MARK G. WILLIAMS

Summary: A number of reviews of the effectiveness of cognitive behaviour therapy for depression, though optimistic, have pointed to some difficulties in interpretation of the outcome literature. These difficulties, and the extent to which more recent research has dealt with them, are reviewed. It is suggested that while some of the genuine difficulties have been resolved, and other commonly expressed criticisms may prove to be unfounded, there remain a number of important areas of ignorance which demand further investigation.

Cognitive behaviour therapy (CBT) is a multi-faceted treatment approach which has been the subject of much debate in recent years. The treatment involves a combination of behavioural techniques (e.g. task assignment) and cognitive techniques (e.g. reality testing) in an active, time-limited and collaborative approach. Typically, between 15 and 20 structured sessions explore aspects of patients' current functioning, concentrating on the task of discovering links between depressive thoughts and interpretations, affect and behaviour. Homework assignments are given to patients, initially merely to monitor their own thoughts feelings and behaviour, but later, with more substantive behavioural targets. However, the emphasis is not on successful completion of assignments, rather a 'data collection' approach in which even non-occurrence of a homework may lead to discovery of the 'roadblocks' that prevent progress in this area of the patient's life. Throughout treatment, patients are encouraged to reconstrue negative thoughts, not as necessarily reflecting reality but as hypotheses to be tested. The therapist refuses either to give bland reassurance that 'things are really OK' or to directly engage in counter argument to the patient's negative self-talk; instead he takes an explicitly empirical approach to the truth or falsehood of the patient's negative conclusions. In this way, the aim is to encourage 'distancing' of the patient from his or her negative automatic thoughts, and to loosen thereby the link between thought and depressed affect. Williams (1984) outlines 19 separate techniques which have been included in outcome studies of CBT for depression (Table I), and gives details of their use in day to day clinical practice. But to what extent are these procedures effective?

In 1979, several reviews of the outcome literature concluded that CBT *was* efficacious with depressed patients, but with reservations (Rehm & Kornblith, 1979; Weissman, 1979; Whitehead, 1979) which revolved around three main issues: the lack of adequate control procedures, the lack of adequate follow-up, and the lack of adequate blind assessment of outcome. This paper examines the extent to which these reservations still exist in the light of more recent studies.

Lack of adequate control procedures

Of the main studies up until 1979, CBT and the other treatments with which they were compared differed in the amount of therapist time available to the patient. Despite the fact that few clinicians would wish to argue that therapist time alone is of therapeutic benefit for depressed patients, a design which does not control this variable (e.g. McLean *et al*, 1973; Rush *et al*, 1977; Blackburn *et al*, 1981; Murphy *et al*, 1984) is clearly deficient, whatever other strengths it may have. In McLean *et al's* study, for example, patients who were not assigned to the psychological treatment group were sent back to the referring agent for whatever routine treatment they thought most appropriate. The situation was slightly improved in Rush *et al's* study, where each patient allocated to the 'drug' group (up to 250 mg per day of imipramine) saw a psychiatrist for 20 minutes weekly for the 12 weeks of the trial. But by

https://doi.org/10.1192/bjp.145.3.254 Published online by Cambridge University Press

TABLE I

Treatment components in cognitive-behaviour therapies

1. Teaching self-monitoring of activities.
2. Teaching self-monitoring of mood.
3. Teaching self-monitoring of thoughts.
4. (Graded) task assignment—teaching how to set appropriate goals.
5. Teaching self-evaluation of behavioural achievement; mastery and pleasure techniques.
6. Teaching self-reinforcement for behavioural achievement.
7. Teaching thought-catching and how to identify themes and assumptions in thought content.
8. Teaching 'distancing' of thoughts by labelling as 'hypotheses'.
9. Teaching how to evaluate evidence for 'hypotheses' (reality testing).
10. Teaching how to deal with implications of thought evaluation.
11. Teaching how to find alternative rational responses to negative thoughts.
12. Social skills/assertion training exercises (modelling, rehearsal, etc.)
13. Reattribution training.
14. 'Alternative therapy'.
15. Role playing.
16. Cognitive rehearsal.
17. Stress inoculation.
18. Thought stopping and/or distraction.
19. Teaching how to increase formerly or potentially pleasant activities.

contrast, in the same 12 weeks, the CBT group received up to 20 hourly sessions of the psychological treatment (mean 15 sessions).

In studies by Blackburn *et al* (1981) and Murphy *et al* (1984), patients received either CBT alone, drugs alone, or a combination of CBT and drugs; Murphy's study also included a CBT plus placebo group. Although the 'drugs alone' group were in the trial for a number of weeks comparable to the other treatment groups, it appears that they only saw the prescribing clinician for 20 minutes (every two or three weeks in Blackburn's study; every week in Murphy's study). Also in these studies, the CBT involved homework assignments, thus extending the influence of the therapist beyond the discussions within the therapy sessions.

There are two problems here. Firstly, any therapy which gave such intense within-and/or between-session psychological treatment might be effective in the management of depression. Secondly, because the CBT gives more intense therapeutic involvement, it may be seen as a more plausible treatment by the patients. In behaviour therapy for anxiety and phobic states, it has become clear how treatment procedures which are plausible to the patient can, in modifying efficacy expectations, enhance treatment outcome (Kazdin & Wilcoxon, 1976). In a later study (which had not been available to the 1979 reviewers) McLean & Hakstian included for comparison treatment groups which controlled for just these variables of treatment length, exposure to therapist, and plausibility.

McLean & Hakstian (1979) studied the effectiveness of three active treatments: 'behaviour therapy' (which included cognitive components); short-term psychotherapy, and drugs (150 mg/day of amitriptyline). These were compared with a relaxation control condition. The study used 178 depressed out-patients, between the ages of 20 and 60; their depression had to be of at least two months' duration prior to the study, and had to be of sufficient intensity to have functionally impaired the patient (unable to work, social withdrawal, etc). All patients' were 'primary depressives' (Feighner *et al*, 1972) and their minimum Beck Depression Intensity score was 23 ('moderately depressed'). Although the patients in the drug group seem to have been seen less often by therapists than those in the CBT group, the presence of two additional psychological treatment procedures, involving the same amount of therapist contact as CBT, provided excellent control conditions. All three psychological treatments (behaviour therapy, psychotherapy, and relaxation) involved ten weekly out-patient visits. Therapists had pre-training to maximise agreement about treatment procedures, as well as regular peer-monitoring sessions throughout the investigation. All sessions were audiotaped and sampled to monitor the use of techniques. The therapists in the psychotherapy treatment were using their preferred methods of therapeutic practice, and focussed on the restoration of pre-episode level of functioning through the 'development of insight into the psychodynamic forces that initiated the current depression, and through the recognition of personality problems'. Patients in the relaxation therapy group were given a rationale of depression as tension-induced. Importantly, in addition to the ten one-hour sessions of relaxation training, clients received extensive homework assignments for relaxation practice and log-sheets to monitor relaxation effectiveness.

In summary, this study deliberately controlled for possible non-specific variables in CBT by including alternative treatment procedures which could be made to seem credible to the patient; these included equivalent therapist contact, and one of them (relaxation) extended the therapist's influence outside the therapy session with the use of homework assignments. Assessment of the outcome of treatment was made on a large range of self-descriptive and behavioural variables.

https://doi.org/10.1192/bjp.145.3.254 Published online by Cambridge University Press

Results showed that whereas all groups improved from pre- to post-treatment, there was a clear-cut difference between CBT and the other three treatments. On the Beck Depression Inventory (BDI), self-rated number of complaints, goal achievement, social adjustment, mood indices (e.g. Depressive Adjective Check List) and average satisfaction (with job, marriage, housekeeping etc), CBT was superior to psychotherapy. On all these indices except the BDI and social adjustment, CBT was superior to relaxation and drug therapy, these two doing no differently from each other. Although the scale of these differences was diminished at three-months' follow-up, the pattern of results was maintained. Furthermore, the drop-out rate from the ten-week trial differed markedly for the different treatments; 5% for CBT, but varying between 26% and 36% for the other three treatments.

Thus, this well-controlled study provides evidence that sheer therapist time and attention, influence outside the therapy session, or mere credibility of treatment technique by themselves are insufficient to explain the superiority of CBT in this type of patient.

The lack of adequate follow-up

Rush *et al* (1977) concentrate on presenting data obtained from the clinical trial itself, rather than follow-up data. McLean *et al* (1973) and McLean & Hakstian 1979 did include a three month follow-up, and Murphy *et al* (1984) include a one-month follow-up, but these intervals are not usually considered long enough to draw reliable conclusions about the long term effectiveness of any therapeutic intervention.

Kovacs *et al* (1981) reported one-year follow-up data on 35 of the patients in the original Rush *et al* (1977) study. That study had randomly assigned patients with a clear and unmixed diagnosis of depression (and scores of at least 17 on the Beck Depression Inventory and at least 14 on the Hamilton Rating Scale) to 12 weeks of either CBT or imipramine (up to 250 mg per day). At the end of the 12 weeks, this CBT group had improved to a greater extent than the drugs groups. The mean of the CBT group was within one s.d. of the mean for a non-depressed normative group and within two s.d.s of the equivalent value on the MMPI. The mean of the drug group remained several s.d.s above the normative non-depressed mean level on both assessments. As with McLean and Hakstian's study, there had been marked differences in drop-out rates between the two groups; 32% of the pharmacotherapy group dropped out and had to be replaced, whereas only 5% of the CBT group dropped out of the trial.

In Kovacs *et al's* study, data are presented for 18 CBT patients and 17 drug patients; the results showed that their treatment gains had been maintained, in that there were no significant differences between dependent measures at the end of one year from those taken at the end of treatment (BDI, MMPI, Hamilton and the Raskin Rating Scale). On the other hand, the two groups were not significantly different from each other at one-year follow-up. Additionally, the investigations had not only taken measures at one year, but had assessed patients frequently throughout the year. In this way, they were able to map out each patient's progress fairly accurately, following termination of treatment and considerable variability was found in the progress of patients from both groups. Inter-group differences, when they occurred, however, always favoured the CBT group, and at some points in the year achieved statistical significance.

Defining 'relapse' as either a BDI score of over 16 during the year at any time, or as receiving further psychological treatment, the drugs group were found to have been twice as likely to relapse as the CBT group. This is important, because much of the argument in favour of CBT is not only its claim to be more effective than other treatment methods, but its claim to render people less vulnerable to further breakdown under stress. In any event, this study suggest that the effects of CBT are not merely short-term.

The lack of adequate blind assessment

Blind assessment has not been a feature of most outcome studies of CBT, which is surprising, given the comprehensive nature of the assessment of some studies. McLean & Hakstian 1979, for example, employed 31 self-rated outcome measures (some of which were single-question measures such as 'hours of sleep' or 'number of productive hours') as well as the Beck Depression Inventory; there was no blind assessment. Zeiss *et al* (1979) included blind observation and coding of the verbal interaction style of depressives under treatment (either interpersonal skills training, cognitive therapy, or activity scheduling). All groups improved significantly in observer-rated social skills, but since a waiting-list control changed by a similar amount, the changes could not be attributed to the therapy. This result may have been due to the fact that the 'patients' in the study were in fact solicited by advertisement for 'free treatment of depression', and it is unclear how comparable this solicited group is with clinical patients.

Rotzer *et al* (1982) have reported a study comparing various sub-components of the CBT package with or without drugs; this evaluated outcome using blind physician-rated Hamilton Rating Scale (Depression) (HRS (D)) data. Thirty-eight depressed out-patients were randomly assigned to receive either Activity Training, or Activity Training plus Self-Regulation Training, or Activity Training plus Coverant Control

https://doi.org/10.1192/bjp.145.3.254 Published online by Cambridge University Press

Training (see Williams 1984 for detailed description of these techniques); each treatment lasted for 12 weeks. Half the patients received medication (amitriptyline or maprotiline up to 150 mg/day) in addition to the psychological treatments, while half did not. The characteristics of the subjects in this study show them to be a more appropriate group than that in studies by Zeiss *et al*. The average time since first onset of depression was eight years six months, and the current illness phase had lasted for a mean of 32 months. Sixteen of the sample had been chronically depressed for ten years or more; nine of the sample were endogenous (ICD 296.1) and 29 neurotic/reactive (ICD 300.4).

Results on all measures in the study showed significant improvement in all treatments, with or without drugs. The blind Hamilton Rating Scores confirmed these results, the Activity Training group changing from a mean of 17 (pre) to 6 (post); Activity Training plus Self-Regulation from 16 (pre) to 10 (post); and Activity Training plus Coverant Control from 15 (pre) to 5 (post). Thus, the overall percentage change scores from the three groups' HRS of 65%, 38%, and 67% respectively compare fairly well with the percentage change scores on the HRS in Blackburn *et al's* 1981 Edinburgh study, the slightly lower overall change in Rotzer *et al's* patients being perhaps attributable to the reduced treatment length (12 weeks, versus up to 20 weeks in the Edinburgh Study).

The lack of no-treatment control in Rotzer's study, though understandable in terms of the aims of the project to look at different sub-components of psychological treatment, makes interpretation of the results difficult. However the fact that there was such marked improvement (clinically as well as statistically) in a group of patients with such long and chronic histories of depression (normally regarded as a negative predictor of outcome) is striking. Of course, the fact that this improvement—similar in scale to that of other studies— was documented on a blind-rated depression scale, does not provide complete reassurance that the efficacy evaluations of other CBT outcome trials, which have not used blind assessment, have been free from experimenter bias effects, but it at least indicates that genuine therapeutic improvement of this magnitude can be obtained, even when such care in evaluating the outcome is taken.

In fact, the alternative to blind assessment by an observer which has mostly been used in outcome research, is not non-blind observer assessments but rather self-ratings. To some extent, this lessons the risk that reported outcomes merely reflect the inadvertent biases of an observer who knows what the study is about and which patients ought to be improving, on the study's hypotheses. In general, when rating themselves, patients are not comparing themselves with others in the research trial, though they may be trying to please their therapist. That is why equivalence of contact time with the therapist is important, if the outcome measures depend on self-ratings. McLean & Hakstian had a psychotherapy control group, receiving equivalent contact with therapists who were using techniques to which they were committed; if the self-ratings had reflected merely the desire to please their therapist, then CBT and psychotherapy should have had equivalent outcomes on this study, but they did not.

A further problem with self-ratings arises if one of the therapies is specifically designed to treat a number of key variables, which then form the greatest proportion of the self-rating scales. For example, if one therapy is designed to decrease depression by alleviating self-esteem deficits and the other by increasing activities, it would be inappropriate to assess outcome with measures which mainly reflect self-esteem levels. However, this problem does not arise in the case of the major outcome studies of CBT (Rush *et al*, 1977; McLean & Hakstian, 1979; Blackburn *et al*, 1981) where a range of self-rating scales covering somatic, behavioural, and cognitive aspects are used.

In the two sections that follow, some common criticisms are discussed, which seem to be based on misapprehensions about CBT, and this is followed by discussion of some more cogent criticisms.

Some common criticisms evaluated

(a) 'CBT produces little extra improvement over drugs alone, so does not justify the additional (costly) time spent on it.'

Table II shows the proportionate changes in the Beck Depression Inventory, in those studies which have used it; the extra benefit of CBT over drugs seems indeed to be relatively small, and in one study (Murphy *et al*, 1984) non-existent. (The discrepancy between Murphy's results and the others is probably due to the greater efforts this trial made to keep the dosage of the nortriptyline within a therapeutic window by regular blood tests. The change-scores certainly support the view that the reason for the lack of difference between drugs and CBT lies as much in the greater effectiveness of the drugs as in the diminished effectiveness of the CBT, compared with other studies.) Yet there are a number of reasons why one should be cautious in jumping to the conclusion that CBT ought to be ruled out on grounds of cost. Firstly, several studies (e.g. Rush *et al*, 1977; McLean & Hakstian 1979) have found differential attrition rates for drugs (over 25%) and CBT (5%). That is, a much larger proportion of drug patients tended to drop-out of therapy, and these are not included in the data analysis. Secondly, there are indications from the follow-up study of Kovacs *et al*

https://doi.org/10.1192/bjp.145.3.254 Published online by Cambridge University Press

TABLE II

Studies using Beck Depression Inventory—changes with treatment

Study	Treatment	Subjects	BDI Score Pre	Post	Proportionate change
Blackburn *et al* (1981)	CBT	GP patients	—	—	0.84
	CBT+drug[1]	Outpatients	—	—	0.79
	CBT+drug[1]	GP patients	—	—	0.72
	Drugs[1] alone	Outpatients	—	—	0.60
	CBT	Outpatients	—	—	0.48
	Drugs[1] alone	GP patients	—	—	0.14
Rush *et al* (1977)	CBT	Outpatients	30.3	5.9	0.81
	Drug[2]	Outpatients	30.8	13.0	0.58
McLean & Hakstian (1979)	CBT	Outpatients	26.8	9.7	0.64
	Drug[3]	Outpatients	27.2	14.1	0.48
	Relaxation	Outpatients	26.8	15.0	0.44
	Psychotherapy	Outpatients	27.0	16.8	0.38
Murphy *et al* (1984)	CBT	Outpatients	28.7	9.5	0.67
	Drug[4]	Outpatients	29.2	8.9	0.69
	CBT+Drug	Outpatients	29.1	8.8	0.70
	CBT+Placebo	Outpatients	30.3	8.2	0.73

[1] Amitriptyline and clomipramine
[2] Imipramine
[3] Amitriptyline
[4] Nortriptyline

(1981) that CBT reduces relapse rate. If CBT is found in other studies to reduce vulnerability to future depressive breakdown, then it may be less costly in the long term. Thirdly, as Murphy *et al* point out, even if treatments are *equally* effective, the least expensive should not be the treatment of choice necessarily, since some patients tolerate the side-effects of drugs poorly, and some antidepressants are cardiotoxic in overdose.

Finally, economics is not the study of 'raw' cost, but rather the study of how 'money' interacts with 'people'; the cost of something is ultimately determined by what people are prepared to pay. Just as some clinicians would be prepared to use a new drug which had greater proven effectiveness (even though it was more expensive), so some patients may feel that a clinic or clinician ought to provide the best and most appropriate treatment, even if it costs more. I have suggested elsewhere (Williams, 1982) that the economic argument against CBT is reminiscent of the argument used some years ago against behavioural treatment for anxiety and phobic disorders—minor tranquillisers are cheaper. But few would now suggest that psychological intervention is not a strategy to be considered for most such patients, if it is available,

(b) 'The drugs used in some studies have not been used in the way they would have been used in routine clinical practice'

There are two issues here. The first concerns that of using a fixed dosage. For example, McLean *et al* (1979) used a fixed dose of amitriptyline (150mg/day), while Rotzer *et al* (1982) started with 75mg/day of amitriptyline or maprotiline and graduated to a dose of 150mg/day for the ten weeks of their trial. Some would have argued that this procedure is unsatisfactory, since in clinical practice the clinician would vary the dose, depending on the response of the patient. On the other hand, some studies *have* allowed the clinician discretion (Rush *et al*, 1977; Blackburn *et al*, 1981), so that this criticism cannot be made of all studies. Allowing the clinician discretion in setting the dosage levels in a research trial is not necessarily an optimal procedure because of the tendency (said to be especially typical in general practice) to under-prescribe antidepressants, but the crucial question is whether patients in these studies were actually getting an adequate dosage in the case of either prescribing procedure. It is fair to say that studies have made efforts to ensure that drug levels reflect 'good prescribing practice'; for example, in Blackburn's trial, GPs and psychiatrists met together at the outset of the study to discuss prescribing

https://doi.org/10.1192/bjp.145.3.254 Published online by Cambridge University Press

practice, agreeing on a range of antidepressant drugs and therapeutic doses.

The second point at issue is the weaning off the antidepressant medication that takes place in some studies at the end of the trial (Rush *et al*, 1977; Rotzer *et al*, 1982). This is done to make the CBT and drug groups comparable, but it is not the practice most clinicians would generally advocate, a maintenance dose being employed in many cases. On the other hand, this strategy does not necessarily favour the CBT group, since most psychotherapists would not abruptly terminate their sessions either, but tail them off gradually. Thus, neither the psychological nor drugs treatment group receive maximally effective therapy at the termination point, and it is impossible to say, without an outcome study to test this specific point, which, if either, style of termination is the more disruptive.

In any event, one study (that of Blackburn *et al*) did not wean patients off at the end of the trial, and the final assessment of outcome was made while patients were still taking the full dose of their medication, so that the criticism of sub-optimality of drug procedure, does not explain all the outcome data. Although more effective antidepressant drugs may exist, or more effective procedures or dosages used by the time outcome studies may eventually be published, investigators have generally attempted to compare drugs with CBT in a fair way. Indeed, one outcome study deliberately stacked the odds in favour of the drug group: Rush *et al* (1977) excluded from the trial, prior to random allocation to CBT or drugs groups, any patient who had a history of non-response to pharmacotherapy, but did not exclude non-responders to previous psychotherapy. The result in favour of CBT in their study is thus all the more significant.

(c) 'CBT is alright for neurotic depressives, but it won't work for those who have a biological depression'

It is certainly true that the majority of studies of the outcome of treatment have used unipolar neurotically depressed out-patients, and have excluded those with delusions and/or hallucinations. Beck *et al* (1979) indeed recommend standard physical treatments for patients who are extremely retarded or agitated, or for highly suicidal patients. However, neither Kovacs *et al* (1981) nor Blackburn *et al* (1981) found any evidence that an 'endogenomorphic' pattern of symptoms in non-psychotic depression predicted any worse outcome in response to CBT. Some clinicians, however, distrust diagnoses made on the standard Research Diagnostic Criteria, and whether CBT is effective with patients selected as having definite biological depression (not merely a more severe depression) on criteria acceptable to such clinicians is a question that awaits further research.

(d) 'Many patients are reassured to know that their depression is an illness. The illness model thus continues to be appropriate'

Using such advice to patients can indeed be therapeutic, but the clinician who uses it is using a cognitive device—re-attribution of the depressive symptoms to an 'illness' rather than say, a 'personality defect'. The extent to which the re-attribution is actually therapeutic in its reassurance indicates the extent to which the person's understanding (or misunderstanding) of his own symptoms was contributing to the depression, or blocking progress in reducing it. This is a good example of how a person's cognitions contribute to the disorder and of how cognitive change can be to some extent therapeutic. Of course, such instruction would not be considered enough by a clinician using an 'illness' model of depression, who would also wish often to use physical treatments—but neither would such instruction be considered sufficient by a clinician using a cognitive model, who would also wish to discover other roadblocks to progress, using, for example, cognitive rehearsal, reality testing, or a more extensive re-attribution training.

(e) 'CBT merely systematises good clinical practice'

This is not so much an objection to CBT as a claim that it adds nothing to what ought to be practised at the moment, but the question then becomes how good clinical practice has to be before it will compare as favourably in the treatment of primary depressives with antidepressant drugs, as CBT has been found to do. Cognitive therapists do not claim to have developed many of their procedures first-hand; many of them are indeed used routinely by clinicians. But outcome studies indicate that it is the combination of a number of techniques that is maximally effective. It is CBT's claim to outline a range of such techniques which can be used in an integrated way in therapy which is its novelty. Of course, some combinations of techniques will prove more effective than others, and at the moment it is not known which these are. These and other areas of ignorance are now discussed.

Some cogent criticisms

Although some commonly expressed criticisms may prove to be unfounded, there remain several areas which demand further investigation.

(a) Specifying which technique works for which patient and at what stage in the time-course of the disorder.

It was mentioned above that the 'endogenomorphic'/'non-endogenomorphic' distinction had not discriminated treatment responders from non-responders in Blackburn *et al's* nor Kovacs *et al's* studies. But CBT is a multi-faceted procedure, in which different patients presumably respond to differ-

https://doi.org/10.1192/bjp.145.3.254 Published online by Cambridge University Press

ent sub-components. For example, Williams (1981) discussed the apparent paradox of urging patients to focus attention inwards to their thoughts (self-monitoring, thought catching, etc) when the need often seems to be to direct attention away from negative thoughts and onto the task in hand.

Should we not have a *rational* basis for deciding what technique to use with what patient? There are reasons to think that research findings can indeed bring some order to the field. Teasdale & Rezin (1978) examined the extent to which distracting depressed patients from negative thoughts reduced the frequency of these and alleviated depression; they found that patients with more 'neurotic' symptoms on the Newcastle Diagnostic Scale (Carney *et al*, 1965) achieved greater negative thought reduction than patients with 'endogenous' features. Confirmation of this result, using a different population and different distraction task, has been found by Davies (1982); she used 20 depressed in-patients, eight of whom had a diagnosis of endogenous depression on the RDC (Spitzer *et al*, 1978), and 12 of whom had a non-endogenous symptom pattern. Results showed that for the whole group of 20, distraction (letter cancellation) was significantly better at producing short-term alleviation of depressed mood than were two other control conditions. However, this distraction effect was wholly accounted for by the 12 non-endogenous patients, the eight endogenous patients varying little in their mood as a result of the procedures.

These results suggest that different sub-types of depression, even if their overall response to the cognitive behaviour therapy package are equivalent, may be 'extracting' different sub-techniques for therapeutic benefit. Further research, which takes these sub-techniques singly or in pairs and compares them with others, will be necessary to specify which techniques are useful for which sub-type of depression. Additionally, such research strategies will need to be adapted to examine the effectiveness of sub-techniques at different *stages* in the course of a disorder. To date, research effort has been concentrated on specifying the ways in which type of depression varies *between* individuals. Rather less is known about the way in which time-dependent changes during the course of a depressive episode may render the same person amenable to one type of intervention (pharmacological or psychological) at one stage, and another type of intervention at another stage.

(b) More research needs to be done on the utility of CBT in treating 'treatment non-responders'.

Rush *et al* (1977) excluded drug non-responders for the good reason (within the terms of their study) that they did not want CBT to be shown to be effective only or mostly for those in whom drugs would not work anyway. Yet it is arguably not the routine depressed out-patient where most help is needed, but the severe and chronically depressed in-patient who has repeatedly failed to respond to a range of alternative interventions. A pilot study using CBT with 12 clinically depressed out-patients who had been unresponsive to other forms of somatic and psychotherapy (Harpin *et al*, 1982) showed some significant effects on anxiety and social skills, but was inconclusive with respect to changes on other self-report and behavioural variables. A research trial is currently underway in Newcastle-upon-Tyne using a similar population.

(c) What psychological processes are actually changed by cognitive-behaviour therapy?

Within CBT, it is assumed that therapeutic change is brought about by changing maladaptive cognitions, cognitive style, beliefs, and assumptions, but it remains extremely unlikely that genuine progress in therapy is mediated by changing merely surface manifestations of these cognitive phenomena. The self-talk of a patient may change, but unless the underlying cognitive processes change—the way the person attends to stimuli in the environment, the way the information is encoded, stored and retrieved—changes in behaviour and mood are unlikely to be long-lasting. As yet, though, psychologists do not possess any agreed procedures for reliably assessing these variables. In the same way as psychopharmacotherapies are sometimes used empirically without the mode of action being entirely clear, so cognitive behaviour therapies are known to work in some instances, but we lack the tools to know exactly why. Until further progress is made in this respect, the situation remains unsatisfactory.

Use of CBT in psychiatric practice

Goldberg (1982) concluded his discussion of CBT thus: "If the present promising results with cognitive therapy are confirmed they will have implications for a wide range of training schemes, and will once more direct our attention to psychological treatments for psychological disorders". Nevertheless, he was realistic in pointing out that because depression is a common condition and cognitive therapists are scarce, clinicians would continue to prescribe drugs as a first option, and then look to refer patients to colleagues who have undergone special training, rather than use CBT themselves. In either case, there is still a pressing need to establish what are the indications and contraindications for CBT, so that clinicians may make a rational choice. The most we can say at the moment is that no research trial has used the technique with psychotically depressed individuals (i.e. those who show delusions or hallucinations), and that standard physical treat-

https://doi.org/10.1192/bjp.145.3.254 Published online by Cambridge University Press

ments are recommended for severely regressed, agitated, or highly suicidal patients. However, the mere presence of endogenous-type symptoms is not a contraindication, and pharmacological treatment does not have to fail before CBT can be successful. In an NHS out-patient sample of depressives, the combination of antidepressant medication and CBT seems the most effective (Blackburn *et al*, 1981). What the respective roles of pharmacological and psychological agents are is a question that still needs to be investigated.

Conclusion

Considering that the first controlled group-design study of outcome with behavioural therapy for depression was only in 1973, and the first explicitly using the Beck *et al* package was in 1977, the last few years have seen a very rapid expansion in its use. There has been, as yet, no fully comprehensive outcome study which has taken into account all the criticisms made in the 1979 reviews. However, taking the research as a whole, the balance of evidence suggests that the major criticisms can be answered. But caution is still appropriate, and ultimately there will need to be a single study which takes all the necessary precautions to ensure blind assessment, adequate controls, and sufficient follow-up procedure in the one study. The most pressing need is to investigate the effectiveness of the techniques in patients who have not responded to any other therapeutic approach. There is also a need to continue looking for rational bases for deciding which technique to use with which patient, at what point in the course of the disorder. Far from reaching the situation where these therapeutic procedures can be unequivocally advocated or unequivocally dismissed, research has a long way to go in determining its range of uses and its limitations.

Acknowledgement

Thanks are due to Dr Peter Cooper, Dr Michael Kopelman and Dr Fraser Watts for their comments on an earlier draft of this paper.

References

BECK, A. T., SHAW, A. J., RUSH, B. F. & EMERY, G. (1979) *Cognitive Therapy of Depression*. Chichester: John Wiley.

BLACKBURN, I. M., BISHOP, S., GLEN, I. M., WHALLEY, L. J. & CHRISTIE, J. E. (1981) The efficacy of cognitive therapy in depression: a treatment trial using cognitive therapy and pharmacotherapy, each alone and in combination. *British Journal of Psychiatry*, **139**, 181–9.

CARNEY, M. W. P., ROTH, M. & GARSIDE, R. F. (1965) The diagnosis of depressive syndromes and the prediction of ECT response. *British Journal of Psychiatry*, **111**, 659–74.

DAVIES, E. (1982) *An investigation into the effect of internally and externally focussed tasks on depressed mood*. (Unpublished MSc Thesis) Department of Psychiatry, University of Newcastle upon Tyne).

FEIGHNER, J. P., ROBINS, E., GUZE, S. B., WOODRUFF, R. A., WINOKUR, G. & MINOZ, R. (1972) Diagnostic criteria for use in psychiatric research. *Archives of General Psychiatry*, **26**, 57–63.

GOLDBERG, D. (1982) Cognitive therapy for depression. *British Medical Journal*, **284**, 143–4.

HARPIN, R. E., LIBERMAN, R. P., MARKS, I., STERN, R. & BOHANNON, W. E. (1982) Cognitive behaviour therapy for chronically depressed patients: a controlled pilot study. *The Journal of Nervous and Mental Diseases*, **170**, 295–300.

KAZDIN, A. E. & WILCOXON, L. (1976) Systematic desensitisation and non-specific treatment effects. *Psychological Bulletin*, **83**, 729–55.

KOVACS, M., RUSH, A. D., BECK, A. T. & HOLLON, S. D. (1981) Depressed outpatients treated with cognitive therapy or pharmacotherapy: a one-year follow up. *Archives of General Psychiatry*, **38**, 33–9.

MCLEAN, P. D. & HAKSTIAN, A. R. (1979) Clinical depression: comparative efficacy of outpatient treatments. *Journal of Consulting and Clinical Psychology*, **47**, 818–36.

—— OGSTON, K. & GRAUER, L. (1973) A behavioural approach to the treatment of depression. *Journal of Behavioural Therapy and Experimental Psychiatry*, **4**, 323–30.

MURPHY, G. E., SIMONS, K. D., WETZEL, R. D. & LUSTMAN, P. J. (1984) Cognitive therapy and pharmacotherapy; singly and together in the treatment of depression. *Archives of General Psychiatry*, **41**, 33–41.

REHM, L. P. & KORNBLITH, S. J. (1979) Behaviour therapy for depression. A review of recent developments. In *Progress in Behaviour Modification*, (eds. M. Hersen and P. M. Eisler) (Vol. 7). New York: Academic Press.

ROTZER, F. T., NABITZ, U., KOCH, H. & PFLUG, B. (1982) Zur bedeutung von attribuierungs—prozessen bei der depressionsbehandlung. In *Leur G Bericht des 33, Kongresses der Deutschen Gesellschaft fur Psychologie*. September 1982. Mainz, FRG.

RUSH, A. J., BECK, A. T., KOVACS, M. & HOLLON, S. (1977) Comparative efficacy of cognitive therapy and pharmacotherapy in the treatment of depressed outpatients. *Cognitive Therapy and Research*, **1**, 17–37.

SPITZER, R. L., ENDICOTT, J. & ROBINS, E. (1978) *Research Diagnostic Criteria (RDC) for a Selected Group of Functional Disorders*, 3rd Edition, N.Y. State Psychiatric Institute, Biometrics Research.

TEASDALE, J. D. & REZIN, V. (1978) The effects of reducing frequency of negative thoughts on the mood of depressed patients—tests of a cognitive model of depression. *British Journal of Social and Clinical Psychology*, **17**, 65–74.

WEISSMAN, M. M. (1979) The psychological treatment of depression. Evidence for the efficacy of psychotherapy alone in comparison with and in combination with pharmacotherapy. *Archives of General Psychiatry*, **36**, 1261–9.

https://doi.org/10.1192/bjp.145.3.254 Published online by Cambridge University Press

WHITEHEAD, A. (1979) Psychological treatment of depression: a review. *Behaviour. Research and Therapy*, **17**, 495–509.

WILLIAMS, J. M. G. (1981) Internal vs external focus of attention: paradoxes in factors mediating cognitive-behavioural psychotherapy in depression. In *Research on Psychotherapeutic Approaches: Proceedings of the 1st European Conference on Psychotherapy Research Trier*, (eds. W. R. Minsel and W. Herff). 1981, Vol. II.

—— (1982) Cognitive therapy for depression. *British Medical Journal*, **284**, 506.

—— (1984) *The Psychological Treatment of Depression: A Guide to the Theory and Practice of Cognitive-behaviour Therapy*. Croom Helm: London and Canberra.

ZEISS, A. M., LEWINSOHN, P. M. & MUNOZ, R. F. (1979) Nonspecific improvement effects in depression using interpersonal skills training, pleasant activity schedules, or cognitive training. *Journal of Consulting and Clinical Psychology*, **47**, 427–39.

J. Mark G. Williams, M.A., M.Sc., D.Phil., *MRC Applied Psychology Unit, Chaucer Road, Cambridge*

(Received 7 December 1983; revised 23 March 1984)

https://doi.org/10.1192/bjp.145.3.254 Published online by Cambridge University Press