Science and Practice of Cognitive Behaviour Therapy

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The evolution of cognitive behaviour therapy

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It is fitting that a book prepared in honour of Professor Gelder should include a chapter on the evolution of cognitive behaviour therapy. He was one of a small group of psychiatrists, most of them trained at the Institute of Psychiatry and the Maudsley Hospital, who took an active interest in the applications of psychology to psychiatry in the 1960s. During this period, important advances were made in the shaping of behaviour therapy, and Michael Gelder played a leading role in these developments, notably in the treatment of agoraphobia, and of sexual disorders. When he was appointed head of the Department of Psychiatry at Oxford, he used the opportunity to promote the development of psychological treatments and gathered a group of gifted research workers and clinicians. They were admirably prepared to play a key part in the growth of behaviour therapy and its expansion into cognitive behaviour therapy. The subject and the Department flourished and Oxford became the world's leading centre for cognitive behaviour therapy.

Professor Gelder prompted, encouraged, and directed these developments and yet found the time and energy to participate in the daily business of clinical research. Among his crucial abilities in fostering the remarkable achievements at Oxford, Professor Gelder successfully combines incisive thinking, a calmly balanced view of controversial problems, and a lucid writing and speaking style. Personally, I recall with pleasure and gratitude many enlightening and rewarding discussions with Professor Gelder.

The evolution of cognitive behaviour therapy (CBT) took place in three stages. Initially, behaviour therapy emerged in independent but parallel developments in the United Kingdom and United States, in the period 1950–70. The second stage, the growth of cognitive therapy, took place in the United States in the mid 1960s onwards. The third stage, the merging of behaviour and cognitive therapy into cognitive behaviour therapy, gathered momentum in the late 1980s and is now well advanced in Europe and North America. CBT is widely accepted and is practised by growing numbers of clinicians; in all likelihood, it is today the most broadly and confidently

endorsed form of psychological therapy. Cognitive behaviour therapy dominates clinical research and practice in many parts of the world.

Stage 1: the evolution of behaviour therapy

It is impossible to give a precise date for the birth of a new form of psychological therapy but one can identify the period in which it emerged—even when as in this instance, the new therapy emerged independently, in two forms, in two countries.

The British form of behaviour therapy (BT), which emerged in the early 1950s, concentrated on neurotic disorders in adults and was derived mainly from the ideas of Pavlov, Watson, and Hull. The major contributors to the early growth of BT were Joseph Wolpe, whose laboratory research on the experimental induction of neurotic behaviour in animals laid the basis for his fear-reduction techniques (Wolpe 1958), and Hans Eysenck who provided a firm theoretical structure and rationale for the new therapy (Eysenck 1960). Unlike Wolpe, whose pioneering and even later work was conducted in relative isolation, Professor Eysenck was the dominating head of the largest graduate research department of psychology in Britain, a position which he deftly used to encourage research into and the teaching of behaviour therapy, at the Institute of Psychiatry at the University of London.

While these events were taking place, a group of American psychologists were pushing ahead with the application of Skinnerian ideas and techniques to clinical problems. Encouraged by Skinner, some of his students and colleagues attempted to replicate the effects of conditioning, so readily achieved with pigeons and other laboratory animals, in psychiatric patients. In one of the earliest attempts, by Lindsley (1956), a replica of a Skinner box, complete with lever and delivery chute, was constructed for research on psychiatric problems. The hope and intention was to shape and re-shape the behaviour of the affected patient by systematic operant conditioning in the 'box'. As in research with animals, attempts were made to deliver tangible rewards whenever the patient emitted a constructive response and withhold rewards after the emission of a disruptive response, such as incoherent rambling. The underlying ideas were that the 'abnormal' behaviour of the psychiatric patients is the result of an inappropriate conditioning history and secondly, that the 'abnormal' behaviour could be re-shaped into normal forms by arranging the correct 'reinforcement contingencies'. By arranging the patient's expressions of adaptive behaviour, such as appropriate speech, to be followed promptly by a reward, the psychologist could re-shape the patient's abnormal activities. It was merely a matter of ensuring that the rewarding contingencies

followed appropriate behaviour, however elicited, and that non-rewarding consequences followed inappropriate behaviour.

The concepts of 'psychiatric disorder' and 'abnormal behaviour' were radically altered, and their value was firmly denied. The patient's predicament was redefined as a problem of behaviour, pure and simple, and the solution lay in providing a corrective programme of operant conditioning. The later introduction of the term 'behaviour therapy' to this American version of the new therapy was exquisitely accurate. As will emerge, the term was less precise when applied to the British version of therapy.

The American psychologists continued their research into and analysis of the behaviour of patients who were, in the conventional description, suffering from chronic psychiatric disorders. Some of these intrepid early researchers, such as Ayllon and Azrin (1968), nobly, and even defiantly insisted on working with the most severe cases, with patients who lived in the 'back' wards of large psychiatric hospitals. Much of their research was distinguished by its ingenuity and inventiveness.

For institutional reasons and more interesting theoretical reasons, the American and British clinicians turned their attention to different samples and different problems. The Americans continued to devote most attention to institutionalized patients with severe problems, believed by most other health workers to be chronic and indeed unchangeable, and the British group pursued their research into adult neurotic problems, working mainly with out-patient samples.

These differences can be illustrated by reference to some typical problems tackled by the two groups. The American group tried to improve the behaviour of people with severe psychiatric disorders, such as schizophrenia, manic-depressive disorders, and then branched out into other severe problems such as childhood autism, mental handicap, and self-injurious behaviour. The British clinical researchers worked on treatments for agoraphobia and other anxiety disorders among otherwise intact, functioning adult out-patients. For the sake of clarity and narrative flow, I am using the terms that were conventionally used for these disorders at the time; the American researchers particularly and insistently avoided using terms such as mental illness. Instead, the 'disorders' were re-described in purely behavioural terms, such as 'verbal behaviour deficits', or 'disruptive behaviour', etc. The noteworthy early attempts in the United States include Lindsley's (1956) re-shaping of verbal responses in psychiatric patients, Krasner's (1958) expansion of the scope of verbal conditioning, Lovaas' (1961) extraordinarily persistent work on the treatment of autism, and the ingenious techniques developed by Ayllon (1963) and Ayllon and Azrin (1968) for dealing with bizarre, psychotic behaviour. Ayllon and Azrin, helped by the research of Staats (1962 personal communication), laid the basis for what later became known as 'token economy systems', that is,

systematic programmes of reinforcement for appropriate behaviour and the omission of reinforcements for inappropriate behaviour (tokens, originally plastic discs, were introduced as readily dispensable and usable markers of reinforcement which the patients were able to exchange for tangible material rewards, such as sweets, cigarettes, magazines, etc.). The earliest token economy systems were established in psychiatric institutions and later introduced into other institutions such as schools, homes for delinquent youths, hostels, and hospitals for mentally retarded people. In all of these endeavours the exclusive emphasis on behaviour was steadily maintained, and care was taken to use the new (behavioural) vocabulary and the new definition of all of these problems in behavioural terms. As with many radical changes, there was an insistence on using the newly correct vocabulary and an impatience with people and papers that continued to use the conventional terms. But this was not mere mannerism or awkwardness—the way in which a psychological problem is described can, of course, have profound implications.

In sum, the American psychologists were Skinnerian in training and outlook, radically behaviourist in their thinking and language, believed that all psychological/psychiatric disorders were problems of faulty learning, concentrated their efforts exclusively on behaviour, and worked mainly with people who resided in institutions and had severe, 'intractable' problems. They espoused an unqualified environmentalism, and tended to regard themselves as 'behavioural engineers'.

In Britain, the pioneering work on behaviour therapy was conducted by psychologists and psychiatrists, unlike in the United States where psychiatrists played little part. The British contributors were not associated with any specific theorist or theory, and certainly were critical of Skinner's ideas (1959) which they tended to reject as narrow and unhelpful. Among the psychologists there was a quiet reverence for the ideas and findings of Pavlov, and the two leading contributors, Hans Eysenck and Wolpe, both favoured the Hullian learning theory, a hypothetico-deductive theory which offered admirable precision and scientific formality, and was widely subscribed to at the time (Hull 1943). The potential extension of Hull's theory to clinical problems, and the hope of developing a quantitative hypothetico-deductive structure, of unquestionably scientific appearance, for abnormal as well as normal behaviour, were attractive possibilities. The selection of the clinical problems and samples arose partly out of the existing health care arrangements, with the large majority of psychiatrists and virtually all clinical psychologists working for the National Health Service. The concentration of clinical psychologists in the Maudsley Hospital (London), which is associated with the largest British training centre for psychologists and psychiatrists (the Institute of Psychiatry), ensured that the fledgling behaviour therapists encountered a goodly selection of psychiatric problems at this extremely active, major psychiatric establish-

ment. The Maudsley Hospital did not cater for patients with chronic psychiatric problems, but provided a selective in-patient service and an extensive out-patient service. Many of the patients using these services had neurotic problems, notably anxiety and depression. Hence, the British concentration on the neurotic problems of adults was determined partly by circumstances and partly by intrinsic interest (Eysenck and Wolpe were deeply interested in the nature of neuroses).

The British also construed many psychological/psychiatric disorders as the product of faulty learning but did not neglect genetic contributions, as did their American counterparts (the terms 'genetic, heredity, inherited', found no place in contemporary publications in the United States). The British espoused a qualified environmentalism in which *neurotic* disorders were regarded as the product of environmental events, learning experiences, and conditioning in particular—but even these learning processes were thought to be influenced by inherited properties. All of the anxiety disorders were considered to be the result of unfortunate conditioning events. Traumatic conditioning or repeated sub-traumatic episodes were thought to be responsible for anxiety. As a result, the affected person experienced conditioned fear in response to conditioned stimuli that were adventitious and the conditioned responses were inappropriate. Particular attention was focused on agoraphobia which was regarded as a conditioned response that provided the basis and cause for the extensive avoidance behaviour which helped to define the disorder.

It is easy to see why agoraphobia was chosen as the prototypical neurotic disorder. The conditioned fear response was dependably provoked by broadly identifiable stimuli (public transport, shops, open spaces, etc.) and disturbed *behaviour* was a defining feature of the disorder. The British group too was preoccupied with the study and modification of behaviour; they too were behaviourists, albeit of a different hue. Most of the inspiration for the conditioning and other learning theories came from laboratory research on animal behaviour, and in particular the research on the induction of experimental neuroses in animals was of critical importance to the emerging theories of abnormal behaviour.

The results of work on experimental neuroses, beginning with Pavlov's original demonstrations, and strongly supported by the American research carried out by Gantt (1944), Liddell (1944), Masserman (1943), and others, provided a basis for thinking about human neuroses. It had been shown that many animals were vulnerable to neurotic behaviour and that conditioning techniques were dependably capable of producing such neurotic behaviour. Moreover this neurotic behaviour showed all of the characteristics of normal learning, conditioned processes—namely, stimulus generalization, extinction, second order conditioning, etc. Plainly, we were in possession of a nearly ideal experimental model.

There were, it is true, some limitations such as the relatively primitive behaviour of the animals, the absence of speech, and acknowledgment that the experimental analogue did not and could not prove that human neuroses developed in this way (i.e., by traumatic or sub-traumatic conditioning). The experiments showed that it was possible, and that in any event, the experimental product had so many features of normal learning as to permit legitimate speculation.

There was almost no human research to guide these moves, and as a result Watson and Rayner's (1920) famous demonstration of the genesis of a conditional fear reaction in young Albert was over-interpreted. It did however support the idea that human fears can be conditioned, and also inspired the invaluable research of Mary Cover Jones (1924) on the unlearning of children's fears. Her research, in particular, made the enterprise seem viable, and directly influenced the forms of behaviour therapy that were developed for children some 30 years later. The full value of her work emerged after a dormancy of three decades, an historical example that provides a spark of hope for clinical researchers who yearn for the recognition of their unjustly neglected gems.

It is an historical curiosity that the father of conditioning process and theory, having found a way to produce abnormal, neurotic behaviour in the laboratory did not take the logical next step and seek conditioning techniques to undo the artificial neuroses. Instead, Pavlov tried the predominantly pharmacological remedies that were in current use, and also prescribed prolonged rest. The first experimentalist to make systematic attempts to find a conditioning or learning cure was Masserman, and he certainly made useful progress by proving that it is possible to reduce an animal's fear and abnormal behaviour through conditioning techniques. Wolpe consolidated and expanded these findings and concluded that the most effective way to reduce the induced fear/abnormalities was to re-introduce the frightened animal to the fear-conditioning stimulus in a graded and gradual manner while feeding the animal at intervals, in order to inhibit the fear. These experiments provided a foundation of systematic desensitization, which in turn provided the basis for all modern, behaviourally based, procedures for reducing fear.

After some further experiments, and trial and error, Wolpe accomplished the successful transition from laboratory to clinic by making two changes in the method of systematic desensitization. For practical purposes, direct exposures to the fear stimuli were replaced by imaginal exposures, and relaxation replaced feeding as the means for inhibiting fear. On the basis of his findings Wolpe formulated the theory of reciprocal inhibition, according to which all or most therapeutic improvements (in anxiety disorders at least) were achieved by the repeated reciprocal inhibition of fear by the imposition of an incompatible response, such as relaxation. In his animal experiments the

feeding of the hungry animals was incompatible with the expression of fear—the feeding response inhibited the fear. Repeated instances of such reciprocal inhibition of fear will give rise to the development of a permanent inhibition of the fear (so-called conditioned inhibition). In treating his patients Wolpe substituted relaxation for feeding as the main inhibitor of fear, but he was careful to argue that any inhibitor of fear would do as well.

Wolpe's theory had useful explanatory value and enabled clinicians to think constructively and systematically about therapeutic problems, but it proved difficult to subject the theory to definitive tests despite the clarity of Wolpe's statements. The theory continues to be helpful but even if it proves to be largely correct, it can no longer offer a comprehensive explanation for therapeutic effects in the anxiety disorders. In similar fashion, the conditioning theory of fear-acquisition, of which Wolpe was co-author, retains useful explanatory power but can no longer provide an account of the genesis of all fears. There appear to be at least two other pathways involved, vicarious acquisition and informational acquisition (Rachman 1978, 1990). Recent developments in conditioning theory (e.g., Mackintosh 1983; Rescorla 1988), emphasizing the informational quality of conditioning, may well prepare the way for a satisfactorily comprehensive new theory of fear and anxiety (Rachman 1991).

Theorists attempted to explain the persistence of anxiety and unadaptive behaviour in addition to its genesis. Here Mowrer's (1960) explanation of the 'neurotic paradox' proved to be extremely useful. He set out a two-stage model of fear and avoidance and argued that avoidance behaviour persists precisely because it is successful—at least in the short term. Any avoidant or other behaviour that reduces anxiety will be strengthened. This model was timely and fitting and behaviour therapists exploited its explanatory value to the full (its limitations became evident in the middle to late 1970s). By this means they were able to explain the genesis and maintenance of significant anxiety and of 'unadaptive' avoidance behaviour, notably agoraphobia.

Wolpe's desensitization method was placed on a firm experimental foundation by the pioneering research of Peter Lang (1963, 1968), who established the laboratory procedures that provided a model for the hundreds of experiments which validated the fear-reducing effects of desensitization and expanded our understanding of the nature of fear.

In addition to his didactic role, Hans Eysenck adroitly advanced behaviour therapy into the position of a leading form of psychotherapy. In his hands, behaviour therapy became important not only for what it asserted but also for what it rejected. In the aftermath of his famously severe criticism of the claims of interpretive psychotherapists, (Eysenck 1952), he went on to dispute the theoretical basis of psychodynamic therapy, and was able to present behaviour therapy as a constructive alternative. He rejected the theory that

neuroses are caused by unconscious (sexual) conflicts and that the manifest symptoms are defences against distress that would otherwise be unendurable. In characteristically provocative style he asserted that if you get rid of the symptoms, you get rid of the neurosis (Eysenck 1960). Actually, he argued that the disturbed behaviour was not in fact symptomatic of anything, but rather, constituted the whole problem. Neurotic behaviour, it was argued, is *learned* behaviour and is therefore open to un-learning. The behaviour is the problem and the un-learning of the behaviour is the solution.

In common with other radical changes in thinking, the development of behaviour therapy was in part reactive. Eysenck, Wolpe, and their colleagues in Britain, and Skinner and the applied behaviourists in the United States, had scant regard for psychoanalysis and the associated ideas of therapy. It was argued that analysis and its derivatives lacked empirical support and that the analysts' claims were unfounded; other critics regarded the whole endeavour as irrelevant and indeed beyond the pale. Wolpe, Eysenck, and their colleagues were strongly influenced by the writings of Karl Popper (1959), and for most of this period Popper's view that psychoanalysis is unfalsifiable and hence outside of science, was accepted and propounded. (This view is now contested by Grunbaum (1984) who argues that psychoanalysis *is* falsifiable, and erroneous.) It is sometimes stated that theories do not fade away but are replaced by preferable, stronger ideas, and numbers of the early behaviour therapists felt the need to provide a replacement. Others were directly challenged to produce a superior form of treatment and set out to oblige. The period 1958–70 was extremely productive; there was movement in the air and an unmistakable sense of change. Enthusiasm reached un-British levels, and certainly a radically different and decidedly new theory and therapy did emerge at this time.

Common ground

In contrast to their counterparts in the United States, the British group were critical of Skinner's ideas and used broadly behavioural ideas and methods. They first tackled adult neurotic disorders and devoted little or no attention to chronic psychiatric illnesses or to the difficulties of people who were intellectually handicapped, until significant progress had been achieved in the United States. Both groups of workers concentrated on behavioural problems and both groups believed that it was necessary and usually sufficient to change the affected person's behaviour. Both groups regarded psychological problems as problems of faulty learning, but the British group was not dedicated to an unqualified environmentalism. Both groups were attempting to apply behavioural science to psychological/psychiatric problems, both

espoused the use of strict scientific standards, and both were participants in the march of empiricism.

The separate development of early behaviour therapy in the United States and the United Kingdom was followed by a merging of the two approaches, at least in terms of aims and most methods. The strong adherence to a Skinnerian framework remained confined to the United States and eventually faded even there. In the period from 1970 to 1980 there was a consolidation of the early advances and a shift from innovative ideas and techniques to the less glamorous business of rigorously evaluating the claims of therapeutic efficacy. Outcome studies abounded, much to the subsequent delight of dedicated meta-analysts. Overall, the commitment to empirical, applied science led to the development of increasingly refined, stringent criteria methods for evaluating therapeutic effects. Behaviour therapists played a leading role in establishing these demanding standards, many of which have now become common coin.

In the course of following these scientific paths, and subsequently, large numbers of people benefited from the skilful and enthusiastic efforts of the new therapists. Greatest progress was achieved in reducing the anxiety disorders, overcoming a range of childhood disorders, such as aggressive and oppositional behaviour, and improving the quality of life of handicapped people. There were disappointments in this early stage, and little progress was made in dealing with depression or the so-called appetitive disorders such as alcoholism, eating disorders, sexual abnormalities, psychotic disorders. Attempts to analyse and treat depression by purely behavioural means had little success.

In the first decade of behaviour therapy there was a good deal of theorizing and grand designs were offered, but the emphasis gradually shifted to technical topics, notably to the assessment of therapeutic efficacy. The shift from theory to practice was natural, even healthy for the time, and marked a victory of sorts. The adoption of empiricism, one of the 'inward workings of the age', transformed the methods of clinical psychology, and behaviour therapy became one of its 'outward facts'.

The attractions of establishing behaviour therapy included the legitimacy of its foundations in academic–scientific psychology and its advocates' insistence on empirical standards (partly as a reaction to the frustrations of an elusive psychoanalysis). The establishment of behaviour therapy, mainly by psychologists, was a major factor in the advancement of the profession of clinical psychology, which had formerly been restricted to measurement and assessment. The introduction of a soundly based, effective psychological treatment provided psychologists with justification and tools.

Curiously, the specific connections with academic psychology, notably the dominating learning theories of the time, fell away, and now few seem to

know and even fewer to care about those connections or their loss. The passionate debates over the strengths and flaws of the schemes of Hull, Skinner, and Guthrie gradually lost their appeal, and one cannot help wondering whether or not it was worthwhile—historical inevitability perhaps? Learning theory certainly played a critical role in the launch of behaviour therapy but then faded out of sight. Instead, we saw the development of an empirical and energetic behaviour therapy, which made valuable practical progress, but manifested a declining interest in theory-building. It shifted from science to technology (Wolpert 1992). The introduction of cognitive concepts has gone some way to filling the gap between technology and science, and the rapid acceptance of these ideas is, I believe, a reflection of dissatisfaction with an uncritical and undemanding form of empiricism.

The absence of progress in theorizing about behaviour therapy in the period 1970–90 gradually became a source of discontent. In addition, there was a second and more practical need to expand the search beyond the essentially behavioural techniques that characterized the first stage of this new type of psychological therapy. The early and considerable successes in reducing anxiety and overcoming unadaptive avoidance behaviour, as in agoraphobia for example, were not accompanied by successes in dealing with depression, the other major component of 'negative affect' (most adult patients complain of a mixture of anxiety and depression). The early attempts to treat depression, by rearranging the reinforcement contingencies for depressive *behaviour*, having made scant progress the door was opened to cognitive therapy. (Oddly, a renewed attempt at modifying such contingencies may now prove to be more successful than it was during the original attempt—particularly as the earlier unwillingness of behaviour therapists to encourage cognitive analyses and provide explanations and advice has disappeared.) The cognitive element in depression is large and obvious, and given the lack of behavioural success, it became the first target of cognitive therapy.

Stage 2: the emergence of cognitive therapy

Given the lack of progress in treating depression and the waning prohibition against using cognitive concepts, many behaviour therapists read Beck's work with growing interest, reassured in part by the inclusion of behavioural assignments in his programme. They were also impressed by his insistence on accurate and constant recording of events and the self-correcting nature of the programme itself. Setting aside whatever remaining suspicions they had about the acceptability of dabbling with these non-behavioural cognitive concepts, numbers of behaviour therapists began treating depressed patients with

cognitive therapy. Early successes were reported and these helped to remove the remaining inhibitions about cognitive therapy, at least when used alongside behaviour therapy and with an emphasis on the behavioural component of cognitive therapy.

Two of the most productive and influential pioneers of cognitive therapy, Beck and Ellis, whose work provides the basis for this historical analysis, shared the view that most disturbances arise from faulty cognitions and/or faulty cognitive processing, and that the remedy is to be found in corrective actions. Both of their forms of therapy are directed at correcting these faulty processes/cognitions, both concentrate on present problems and present thinking in contrast to the historical dredging of earlier forms of psychotherapy, and both recommend the inclusion of behavioural exercises. It is worth mentioning that Ellis and Beck both started out as psychoanalytic-dynamic therapists, and even Wolpe's earliest clinical techniques were psychodynamic. Therapists had little choice in those days, so the likes of Beck, Ellis and Wolpe forged their own tools.

Both Beck and Ellis acknowledged the value of behaviour therapy (as well as its insufficiencies), and as a result behaviour therapists were sympathetic to the Beck/Ellis versions of cognitive therapy—which were regarded more as supplements than opponents. Beck writes that, in the mid 1960s: 'I became familiar with behavior therapy and incorporated many principles from this approach' (Beck 1993, p. 13). Beck and Ellis regarded the behavioural exercises as means of obtaining new, corrective, information and in this sense, they differed from the behaviour therapists who regarded the behaviour changes as the essence of therapy rather than one of several secondary methods of producing change. The debate about interactions between cognitions and behavioural change continues. For example, it is not yet clear exactly why behavioural exposure exercises are followed by such significant reductions in panic cognitions and panic episodes. Nor is it clear why this seemingly indirect exposure method should be followed by cognitive changes that sometimes equal those which follow from directly cognitive therapy.

Beck's form of cognitive therapy was based on the 'rationale that an individual's affect and behavior are largely determined by the way in which he structures the world' (Beck *et al.*, 1979, p. 3), and the therapeutic techniques were designed to 'identify, reality test, and correct distorted conceptualizations and the dysfunctional beliefs (schemas) underlying these cognitions' (p. 4). All of it distinctively cognitive and too diffuse for determined behaviour therapists, but the reported successes in treating depression could not be overlooked—and Beck's therapy had a respectably behavioural component.

Albert Ellis (1958, 1962) was an early and assertive advocate of a directive form of cognitive theory which he originally described as rational psychotherapy, a term which he later expanded into rational–emotive

psychotherapy. He argued that 'emotional or psychological disturbances are largely a result of (the person) thinking illogically or irrationally; and that he can rid himself of most of his emotional or mental unhappiness... and disturbance if he learns to maximize his rational and minimize his irrational thinking' (Ellis 1962, p. 36). In Ellis' view, 'people are uniquely rational as well as uniquely irrational' (p. 36), and 'their difficulties largely result from distorted perception and illogical thinking'. The way to overcome their difficulties is to help them to improve their thinking and perceptions.

Early in his career, Ellis concluded that 'insight alone was not likely to lead an individual to overcome his deep-seated fears and hostilities; he *also* needed a large degree of fear-and-hostility-combating *action*' (original emphases, Ellis 1962, p. 10). He recognized that the need for professional psychological assistance arises largely from expressions of irrationality, particularly those in which emotional reactions/behaviour clash with the person's rational assessment of the circumstances. In company with Beck (see Beck 1993 for examples), he was early to recognize the impact which behaviour can have on a person's emotions and thinking, and he took care to include behavioural assignments and exercises in his treatment programmes. They were given a more important and more explicit role in therapy by Ellis than by Beck in his early writings, and Ellis acknowledged a direct debt to learning theory. In the early part of his career he explored ways of bringing about a convergence between psychoanalysis and learning theory, having received his original training as a psychoanalyst. He was also early to recognize that the crux of cognitive forms of therapy is the relationship between thinking and emotion. 'The theoretical foundations of rational therapy are based on the assumption that human thinking and emotion are *not* two disparate or different approaches, but they significantly overlap and are in some respects, for all practical purposes, essentially the same thing... and never can be seen wholly apart from each other' (Ellis 1962, p. 38).

Despite his prescience in identifying the interaction between cognition and emotion as the central question in cognitive approaches to therapy, his early recognition of the power of behavioural change, and his persistent and assertive advocacy of his form of rational therapy, the work of Ellis did not attract as much research and clinical attention as did that of Beck. The scientific status of Ellis' work lagged behind Beck's (Kendall *et al.* 1995). In part, no doubt, this difference can be traced to the form of their writings. In comparison with Beck's, the writings of Ellis were more anecdotal and loosely formulated. In addition, the thrust of Beck's early work was on understanding and treating depression, a clinical problem that remained essentially unsolved by behaviour therapists. It was understandable, therefore, that clinicians, even those who were using behavioural techniques for the management of other clinical problems, should turn to the work of Beck for guidance in trying to

help people overcome depression. If Beck had carried out his initial investigations on anxiety disorders, in which behaviour therapy had already made significant progress, appreciation of his work may well have been delayed for a considerable period.

Beck's cognitive therapy for depression was based on the assumption that the affected people engage in faulty information processing and reasoning, and subscribe to schema that are self-defeating. In particular, depressed people are subject to what Beck called the 'cognitive triad' in which they have feelings of pessimistic helplessness about themselves, the world, and their future. The aim of the cognitive therapist is to identify and then help patients to correct these distorted ideas, and also to improve their information-processing and reasoning. The therapeutic procedures are highly structured and time-limited and begin with the recognition of the connections between cognitions and affect, careful recording of these connections, collection of evidence for and against the ideas, followed by the substitution of more adaptive and realistic interpretations. In addition to recording the dysfunctional thoughts and combating them, the patient is encouraged to engage in various homework assignments (a rudimentary form of behavioural therapy). The correct analysis and treatment of depression by these methods requires subtlety and skill which can only be achieved through supervised training.

Although cognitive therapy was developed during a time in which psychology as a whole was moving strongly in the direction of cognitive explanations, there was a curious gap between these two movements. The early claims of connections betwen cognitive behaviour therapy and cognitive psychology were statements of hope rather than fact. Teasdale recently observed that: 'The development of cognitive therapy for depression has proceeded largely in isolation from basic cognitive science' (Teasdale 1993, p. 341; see also Seligman 1988). The two most prominent forms of cognitive therapy share with basic cognitive science a general outlook, but there is little similarity in theorizing, terminology, or methodology. Moreover, both forms of cognitive therapy deal almost exclusively with attempts to correct conscious thoughts and to make them more rational by the collection of information, intellectual analysis, persuasion and encouragement, and behavioural changes. Cognitive therapists' tendency to refer 'solely to consciously experienced thoughts and images ... clearly diverges from the much wider use of the term in cognitive psychology. There, it is assumed that the majority of cognitive processing is not experienced as consciously accessible thoughts or images' (Teasdale 1993, p. 340).

The narrow use of the term 'cognition' and associated concepts, the neglect of 'non-conscious' processes and the absence of useful connections with cognitive science, have all given cause for concern. In addition, Teasdale (1993) has drawn attention to problems that have been encountered specifically by Beckian

cognitive therapy. He argues that cognitive therapy is not uniquely more effective than other treatments for depression that do not deal with negative thinking directly, that improvements after medication are associated with changes in depressive thinking similar to those observed after cognitive therapy, that there has been a failure to demonstrate the persistence of dysfunctional attitudes in vulnerable people after the depression has remitted, that many patients experience emotional reactions without being able to identify proportionate negative thoughts, that rational argument is frequently ineffective in changing the emotions, and that cognitive therapy is too often ineffective. However, his deepest concern is the gap between cognitive therapy and cognitive science.

Accordingly, Teasdale and Barnard (1993) set out a comprehensive theoretical framework intended to accommodate contemporary cognitive science knowledge and the clinical cognitive theory exemplified by the work of Beck. This is not the place for a detailed description of their scheme, except to say that they proposed the existence of nine types of information, each representing a different aspect of experience. Each type of information is separately processed and has a separate memory store. Their model proposes that there are mental codes related to two levels of meaning, a specific and a more generic type. Patterns of propositional code represent specific meanings, whereas the implicational codes represent generic and holistic levels of meaning. It is this latter type that is most important in emotional experiences. These codes are 'directly linked to emotion' (Teasdale 1993, p. 345). The implicational level of meaning contains representations at a high level of abstraction, and the implicit knowledge encoded at the implicational level represents models of experience which capture regularities in the person's world. Sensory variables, such as gesture, make a direct contribution to these implicational meanings. The importance of the implicational codes lies in their direct connection with emotion. In contrast, the other type of representation, propositional, 'cannot, alone, elicit emotion' (p. 346). According to Teasdale, 'the central goal of therapy should be to replace implicational code patterns related to depressive schematic models with alternative patterns related to more adaptive higher level meanings or schematic models' (p. 349). The ultimate value of this ambitious and complex model will not become apparent until a good deal of research and debate has taken place, but the recognition by Teasdale and Barnard of the problems confronting cognitive therapy is timely. They are perhaps excessive in excluding the possibility that propositional representations can elicit emotion, and their theorizing is too exclusively concerned with Beck's theory and the clinical problem of depression. Some of their arguments are plausible with respect to depression, but less convincing when applied to the anxiety disorders such as panic.

The connections between early forms of cognitive therapy and cognitive psychology were sparse and even those were loose. However, it is now

established that attentional biases do occur during emotional experiences, and that memories, at least those involving recall rather than recognition, often are mood-related, if not entirely mood-dependent (e.g., M. W. Eysenck 1982; Williams *et al.*, 1988). A small start has also been made in capturing the distortions of perception that are caused by, or at least related to mood disturbances. For example, feared objects, such as snakes, may appear to be larger or more active during periods of elevated fear than they do during periods of tranquillity. In fear, bridges loom larger, rooms shrink Alice-like (Rachman and Cuk, 1992).

So, fears and sadnesses, appear to influence what we attend to, they influence how we perceive what we attend to, and they influence our memories of what we have attended to and perceived in the past. This interplay of emotions and cognitions is fascinating and will occupy intense scientific curiosity in the coming decades. However, the very interconnectedness of emotions and cognitions that is so fascinating, will be a great challenge to researchers. The selection and isolation of the variable of interest, in a manner that allows one to maintain constancy in the rest of the interconnecting web, is difficult and will require the introduction of new methods to replace our presently superficial ones. Here ingenuity will be all.

Stage 3: the merging

The adoption of cognitive ideas and methods, such as Beck's, was facilitated, indeed promoted, by the major shift towards cognitive psychology that was taking place in psychology in general. Previously suspect cognitive concepts and language became allowable, even necessary, and steadily replaced such yawn-inducing concepts as fractional anticipatory goal responses. I suspect that a significant prompt to the shift from pure learning theory to cognitive science was a dissatisfaction with the overwhelming efforts devoted to laboratory animal research, especially the obliging white rat, and the priority assigned to this work; it became increasingly remote from the original aim of our scientific curiosity—human behaviour and experience.

The time was ripe for a cognitive behavioural form of therapy and the two streams, cognitive and behavioural, were welded together by the successful development of a treatment for panic disorder.

In the process of merging behaviour therapy and cognitive therapy, the behavioural emphasis on empiricism has been absorbed into cognitive therapy. The behavioural style of conducting outcome research has been adopted, with its demands for rigorous controls, statistical designs, treatment integrity and credibility, and the rest. The merging of the old (behaviour therapy), with the new (cognitive therapy) is not free of problems, and we have already discovered

that behavioural changes are more accessible and easier to measure than evanescent cognitions and their fluctuations. Leaving these complexities aside for the moment, it is possible to discern an exchange, in which cognitive concepts were absorbed into behaviour therapy, and cognitive therapists attached increasing emphasis to behavioural experiments and exercises. Researchers and therapists are acutely interested in, and attuned to, the patients'/clients' explanations, understanding, wishes and fears, as never before. *Cognitive therapy is supplying content to behaviour therapy.* This development confirms the historical connection, seldom acknowledged, between *phenomenological psychopathology* (e.g., Jaspers 1963) and cognitive therapy.

Long before the infusion of cognitive ideas into this field, obsessions were regarded, indeed *defined*, as unwanted, intrusive thoughts (plus, of course, images and impulses). But the precise content of these unwanted thoughts was of little interest. Thanks to the influence of cognitive therapy, we now are intensely interested in this content, and can begin to hope that eventually we will be able to trace the content of these thoughts closer and closer to the true nature (and functions) of obsessions.

This is a specific and telling example, but the influence of cognitive therapy goes further and wider. It also offers the promise of greater explanatory power and deeper understanding of abnormal behaviour and its springs. The outstanding example here is, of course, the psychological theory of panic [theories actually (see Barlow 1988), but for present purposes Clark's theory (1986) will be used in exposition]. Despite the difficulties it has encountered, some of which are discussed below, Clark's theory has increased our understanding, removed some blurs, and made explicable, many facets of panic. The phenomenon now is seen to be coherent, to be psychologically understandable. In many cases, studies, and experiments, we are able to make good sense of the person's thoughts and fears, and how they connect. If anything, the danger now is that cognitive explanations are being fashioned too readily and that their evident plausibility can become a trap.

In short, cognitive concepts have widened the explanatory range of behaviour therapy and helped to fill in the picture. The most solid advances have been achieved in understanding and treating panic disorder, and these advances have spilled over and enlivened research and thinking on obsessive–compulsive disorder and on hypochondriasis, better now regarded as excessive health anxiety (Salkovskis and Warwick 1986). We can anticipate important developments in understanding these disorders, and indeed in the full range of anxiety disorders. Progress is underway in research on social phobias, generalized anxiety disorder, obsessive compulsive disorder, and post-traumatic stress disorder.

The nature and the treatment of depression are exceedingly complex subjects and will not be pursued here in any depth. Retrospectively, it has to

be said that therapists were over-optimistic. Beck's (1967, 1976) ambitious, complex, and many-layered theory has a broad plausibility but is marred by internal problems (Teasdale and Barnard 1993), some moot assumptions and the unmanageability that often flows from complex and complicated explanations. At times it seems overdressed. Progress in coming to grips with this grand scheme has been slow, and the failure of the large-scale collaborative study to produce evidence of the superiority of cognitive therapy (Elkin 1994; Elkin and Shea 1989; Shea and Elkin 1992) was a disappointment, notwithstanding some flaws in that study. The elusiveness of evidence to support the claim that there are exclusive connections between specific cognitive changes and reductions in depression, is also troubling, and there is an echo of this problem even in the most recent research on panic.

The exhilarating advances in understanding anxiety inevitably raise fresh theoretical questions, some of which are discussed below.

There are prickly questions of causality, and the results of cognitive behaviour therapy are open to alternative interpretations: cause, consequence, or correlate? The decline in cognitions, and in bodily sensations, observed after successful treatment is open to more than a single interpretation (e.g., Seligman 1988). The decline in cognitions, and/or in bodily sensations, may produce the reduction of the panics. But it is possible that the decline in cognitions, and in bodily sensations, are consequences of the reduced episodes of panic, and not the cause. It is also possible that the decline of cognitions is a correlate of the reduction in the episodes of panic [some critics have suggested that the cognitions and their decline may be mere epiphenomena (e.g. Seligman 1988; Wolpe and Rowan 1988)].

One reason for giving serious consideration to these alternative explanations arises from the fact that in Margraf and Schneider's (1991) and Margraf's (1995) study of panic disorder, the patients who received pure exposure treatment without cognitive manipulations showed improvements as large and as enduring as the patients receiving pure cognitive therapy in which exposures were excluded. Moreover, the cognitions declined to the same extent in both groups. It appears that negative cognitions can decline after a direct attack or after an indirect attack. Indeed, a satisfactory cognitive explanation needs to account for the declining cognitions that occur after a non-direct treatment, such as exposure. The most obvious possibility is that with each exposure, the patient acquires fresh, disconfirmatory evidence (e.g., no heart attack, did not lose control). The accumulation of this personal, direct, disconfirmatory evidence weakens the catastrophic cognitions. However, one is nevertheless left to ponder why the direct assault on cognitions was not significantly more effective than the indirect, incidental effects of exposure in the studies by Margraf and by Ost. It remains possible that the longer-term effects of cognitive behaviour therapy are superior — even

when differences in cognitions are not evident at post-treatment (e.g., Cooper and Steere 1995). Furthermore, there is evidence of a dose-responsiveness relationship between cognitive behaviour therapy and cognitive change. For example, in the important outcome study by Clark *et al.* (1994), panic patients who received added, direct cognitive therapy had a superior therapeutic outcome to those who received indirect treatments.

These complex theoretical matters will be sorted out over the next few years, but to return to the therapeutic mechanisms of cognitive therapy, we need to ascertain whether or not the reduction/elimination of key cognitions is the critical element in this form of therapy. We already know that the direct modification of cognitions can be a sufficient (or even superior, e.g. Clark *et al.* 1994) condition for treatment success, but we also know that direct modification is not a necessary condition for success (e.g., exposure alone can be as effective as cognitive therapy, imipramine and other medications produce therapeutic improvements, but presumably via different mechanisms, etc.).

In analysing the treatment of anxiety, as in the treatment of depression, one obstacle to severe tests of the theory arises from the need for control over the timing of events. If the reductions in negative cognitions are no more than correlates of panic reduction, or if the cognitive changes follow rather than precede the reduction of panic, we need to study the sequence of events with care. Reductions in fear are easier to observe and record, but they can occur slowly, over weeks rather than minutes. In cases of panic, the measures typically range over days or weeks (e.g., the number of panics recorded per week or even per month). So if the patient records a decrease in panics, say from four per week to one per week, when exactly did this decline take place?

Cognitive changes can be even more difficult to track. Major changes can occur suddenly (e.g., Ost 1989; Rachman and Whittal 1989), and are therefore easy to record. In many, perhaps most occurrences, clinical or experimental, the cognitive shifts are slow to develop, changing over weeks rather than minutes [e.g., the cognitive therapy group in the Booth and Rachman (1992) study]. To make matters worse, the changes in fear and in fearful cognitions can and undoubtedly often do occur even when the affected person is separated from and out of contact with the fear-provoking stimulus (Rachman 1990). It is not possible to determine precisely when the change occurred, assuming, of course, and there is a complete change in the first place. So we are left with the awkward task of timing the sequence of changes in the cognitions and in the episodes of panic, knowing that these changes may take place over an extended period and that the determination of a precise point of change will be difficult or impossible. We also have some evidence that cognitive shifts can initiate a process of change that becomes evident some time later.

The processes set in train during cognitive behaviour therapy reach a conclusion at some point between sessions; in this sense, CBT sessions *initiate* emotional processing (Rachman, 1980, 1990) that reaches completion only after an interval in which the fearful person has no contact with the phobic stimulus and usually cannot recall having made deliberate attempts to facilitate the fear reduction between sessions. This apparent delay in the effects of cognitive behaviour therapy was also encountered in the experimental reduction of claustrophobia described by Booth and Rachman (1992).

In Salkovskis' (1985) refreshing cognitive analysis of obsessive–compulsive disorders, the affected person's construal of the behaviour and urges was taken as the starting point and the conclusion of the problem. Salkovskis focused attention on the explanation which the affected person provides for his/her obsessive–compulsive urges, behaviour, and motives. In this way he succeeded in filling a previously unknown stage. Previously, the nature and significance of the specific *content* of the obsessions and compulsions remained unexamined. This is, I believe, an important advance and one that will absorb a great deal of thought and effort in the next few years.

Given their common ancestry (Salkovskis and Clark 1993), it is no surprise that the cognitive analyses of hypochondriasis and of panic disorder are similar. The cognitive theory of hypochondriasis shares the boldness that characterizes the theory of panic disorder. It is argued that 'bodily signs and symptoms are perceived as more dangerous than they really are, and that a particular illness is believed to be more probable than it really is' (Warwick and Salkovskis 1990, p. 110). In panic disorder, the affected person is assumed to make catastrophic misinterpretations of bodily sensations and hence panic (Clark 1986). Importantly, the panic theory pertains to expectations of imminent catastrophe (e.g., 'I am having a heart attack'). The hypochondriasis theory pertains to threats to one's health or well-being that can be equally catastrophic but are more remote (e.g., 'This bump on my skin will develop into a cancer'), but the underlying mechanisms are assumed to be common to both disorders.

The cognitive approaches to phobias, obsessive–compulsive disorder, and hypochondriasis and the emerging analyses of post-traumatic stress disorder (Ehlers, in prep.) have a common core that is derived mainly from the cognitive theory of panic. The future of cognitive behaviour therapy in the treatment of panic disorder will have major ramifications for the cognitive approaches to all of the anxiety disorders.

The advance of cognitive behaviour therapy, and especially the successes in treating panic, gave rise to the first and only major *psychological* alternative to the then widely accepted biological theory of panic (see Rachman and Maser 1988). The debate will rumble along and also incorporate competing

explanations for obsessive–compulsive disorders. Importantly, the cognitive explanation for the results of cognitive therapy in treating panic is the best supported at present. *Indeed, there is no plausible alternative explanation for the effects of cognitive therapy at present.*

Predicted trends

As predicted in 1990, the advances in treating panic disorder triggered off a stimulating series of cognitive re-analyses of all of the anxiety disorders. The broad forward sweep is well under way and promises gold.

I wish to draw attention to another, less obvious development, namely the extension of cognitive analyses to non-psychiatric aspects of medicine. It was argued in 1975 (Rachman and Philips) that psychologists should expand the scope of their clinical work beyond the exclusive concern with psychiatric problems. To some extent an expansion did take place, most conspicuously in the increasingly successful application of psychology to clinical problems of pain. But progress on other medical topics has been slower than desired or expected.

We are now in a position to expedite the desired expansion by a systematic and inventive application of cognitive analyses to a wide range of medical–psychological problems. Some obvious and early subjects that will be tackled with increasing earnestness, in addition to pain, are the cognitive aspects of undergoing stressful medical procedures, doctor–patient communication, and the nature and efficacy of clinical reasoning. Even more deeply, we can expect subtle and enlightening cognitive analyses of the way people construe their health and illnesses, their construal of medical information and treatments, and so forth.

During the next few years we can also expect to see a forceful expansion of cognitive theory and therapy into a variety of non-psychiatric medical problems, in keeping with the fundamental expansion of clinical psychology itself. Before long, a fully cognitive clinical psychology will be established.

These changes, especially the expansion beyond psychiatric medicine, will involve institutional as well as scientific changes, and new inter-disciplinary arrangements will emerge. How will cognitive behaviour therapy fit into these changes?

Evidently and essentially, the collaborative interchange with cognitive psychology will improve. The relationship with psychiatry, recently strained by disagreements about the limits of exclusively biological explanations of psychopathology, probably will be restored by advances in neuroscience. Many medical scientists, not least Michael Gelder, will find the introduction of cognitive analyses into general medicine refreshing and illuminating. I

envisage nothing short of a cognitive revolution in medical psychology, in which many aspects of health, illness, and treatment will be re-analysed in cognitive terms.

Growth points

To summarize, I anticipate that several growth points will emerge.

Within the traditional boundaries of cognitive behaviour therapy, we can expect important progress in understanding and treating the full range of anxiety disorders. Correspondingly, the explanatory use of the concept of emotional processing will be expanded to incorporate the new findings and successes. Look for the emergence of a theory of cognitive–emotional processing. The interplay between affect and cognition will be the critical topic for research.

Cognitive concepts and analyses will be extended into general medicine, beyond the traditional limits of psychiatric psychology. The wider use of cognitive concepts in the psychology of pain will have particularly important repercussions.

Attempts to link cognitive behaviour therapy to progress in neuroscience will continue, but with more success than in the past. The selective use of neuro-imaging techniques will begin to make a valuable contribution to our understanding of the cognitive psychology of abnormal behaviour and experiences, and how to remedy them. And, as ever, advances in clinical research will feed back to enrich the fundamental base, in this instance, cognitive psychology itself.

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