A Portfolio of Study, Practice and Research submitted for The Doctorate of Psychology (PsychD) in Clinical Psychology Conversion Programme; University of Surrey.

Perfectionism, guilt and responsibility in obsessive-compulsive disorder: an investigation of cognitive and emotional styles.

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Contents

Personal study plan

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Academic Dossier

Title and description of proposed reviews.

1. *Solution-focused Brief Therapy: a critical review*

Solution-focused therapy is an approach which derives from systemic models of brief therapy. It has been applied in a wide variety of therapeutic settings and appears to be regarded as a simple but powerful method of change. This essay will critically review the literature on solution-focused therapy, with particular reference to its application in mental health and its relevance to clinical psychology.

2. *Cognitive-behavioural therapy and schizophrenia: current status and applications to early intervention*

Treatments for schizophrenia have traditionally rested within the domain of pharmacotherapy. Cognitive-behavioural therapy has recently been applied to this severe disorder, with promising results. Research has now turned to the possibilites for treatment in the early stages of schizophrenia. This review will look at the current status of theories of schizophrenia and early intervention in general, and will then explore the contribution of cognitive-behavioural theory and practice to the early treatment of this illness.

Professional Dossier

Professional practice, training and development

This part of the dossier will outline my achievements and demonstrate continued professional development since qualification. It will include a CV which summarises my professional practice since qualifying, followed by a summary of training and service developments. I will include reflections on my professional career and my hopes for continuing development in the future.

Developing a family therapy service in adult mental health: a review and preliminary audit of the first two years

The second part of this dossier will describe the development of a new family therapy service, and the role of clinical psychology in setting up and evaluating the service.

Research Dossier

Title: *Perfectionism, guilt and responsibility in obsessive-compulsive disorder: an investigation of cognitive and emotional styles*

Research supervisor: Professor Sarah Hampson

Background

Obsessive-compulsive disorder (OCD) is a severe and disabling condition and is considered to be one of the most treatment resistant of the anxiety disorders. Behavioural models developed in the 1960s led to interventions which greatly improved the prognosis for OCD sufferers, and treatments such as exposure and response prevention are now treatments of choice. However, as a theory of etiology, the behavioural model has its limitations, and in recent years cognitive models of OCD have been proposed which claim that patients display certain cognitive traits or thinking styles which predispose them to obsessionality.

Perfectionism and an excessive sense of responsibility have both been linked clinically to OCD, while guilt, as an affective trait, has also been hypothesised to play a key role in the condition. However much of the research in this field utilises normal populations or fails to include comparison groups. In addition, few empirical studies have addressed the question of how these styles and traits might relate to each other in OCD.

The proposed study will measure perfectionism, responsibility and guilt in patients with OCD compared to patients with other anxiety disorders. It is hypothesised that OCD patients will show significantly higher levels of these traits.

ACADEMIC DOSSIER

Solution-focused Brief Therapy: a critical review

Introduction

Clinical psychology as a mental health profession places strong emphasis on its scientific heritage. Its academic base rests upon carefully designed research, with the randomised case-controlled study held as a benchmark of good methodology. In practice, clinical psychology generally adheres, in terms of training at least, to the principles of the scientist-practitioner model and evidence-based practice. Indeed the latter is now considered a key requirement for all types of psychological therapy in the health services (NHS Executive, 1996).

Clinical psychologists working 'in the field' however are likely to encounter a range of new therapeutic models and ideas, either through their multi-disciplinary colleagues or via the multitude of courses and workshops available to the helping professions. To what extent these new ideas are adopted by clinical psychologists is unclear, although it is generally acknowledged that practising psychologists rarely adhere strictly to the theoretical framework that most are trained in, that is cognitive behavioural theory (Pilgrim, 1997).

One model which is becoming increasingly popular in both health and social services is Solution-focused Brief Therapy (SFBT), an approach developed by Steve de Shazer and his colleagues at the Brief Family Therapy Center in Milwaukee (Molnar and de Shazer, 1987). This approach is applied to individuals, families and groups and focuses not on clients' problems or pathology but on what clients are doing that 'works', utilising specific questions and techniques to reinforce and amplify these strengths. SFBT, primarily through frequent workshops and training courses but often through word of mouth, has attracted the interest of many health professionals including clinical psychologists (Valinejad et al., 1996). The reasons for this interest are varied but a number of factors seem pertinent. The notion of a *brief* therapy, in these times of financial restraint and long waiting lists, is an attractive proposition to clients, clinicians and health care purchasers alike. SFBT offers an average of 4.6 sessions (Johnson and Miller, 1994), and even single-session SFBT is promoted by its originator as an effective treatment (Talmon, 1990), though this is based on anecdotal evidence only. Training too is brief, relatively low cost and open to both public and professionals. The model itself is intrinsically appealing with its emphasis on non-pathologising, empowering techniques which are easy to learn. Courses and publications are offered in the UK which apply SFBT to couples, families, adolescents, older adults, abuse, crisis intervention, substance misuse, male violence, schizophrenia, child protection, school problems, and social work.

SFBT has its roots in systems theory and, in more recent forms, eschews an empirical or positivistic view of knowledge. Consequently it has developed and been evaluated within a different context compared to mainstream psychological theory. However, given the increasing impact of this approach on clinical practice, it is timely to attempt an analysis of the strengths and weaknesses of de Shazer's ideas. This review will begin by a brief description of the main features of SFBT. It will then consider the theory and philosophy underlying SFBT and how this might relate to other models of therapy, including cognitive behaviour therapy. The evidence for the effectiveness of SFBT will be addressed, and lastly some ethical and professional issues raised by the use of SFBT in clinical practice will be highlighted.

Solution-focused brief therapy in practice

SFBT has been summed up by three 'rules' as follows;

"(1) If it ain't broke, DON'T FIX IT!
(2) Once you know what works, DO MORE OF IT!
(3) If it doesn't work, then don't do it again, DO SOMETHING DIFFERENT!"
(Carpenter, 1997, p.117)

This 'common sense' philosophy suggests a simple, easy to learn technique and indeed SFBT 'how-to' books and training courses also present SFBT as a relatively straightforward set of questions and tasks. George et al., (1990) describe eight basic steps to be taken in the first session which also serve as a blueprint for all subsequent sessions.

1. Problem-free talk: building rapport and locating strengths.
2. Statement of the problem pattern.
3. Exploration of solution patterns i.e. exceptions to the rules of the complaint.
4. Establishing goals for therapy. Asking the 'miracle question' (what would life look like if a miracle occurred and the problem no longer existed).
5. Definition of potential solutions: use of scaling questions to assess small changes.
6. Consultation break (with a team or alone).
7. Feedback (positive only: compliments).
8. Task setting. (adapted from George et al., 1990, p. 7)

The SFBT approach rests on a large number of assumptions and prescriptions which do not necessarily relate logically to one another. Essentially this reflects the diverse models and theories from which solution-focused therapists have drawn their inspiration. This will be addressed in the next section. There are some core assumptions however which are characteristic of SFBT including the following;

- Change is always occurring.
- Changes in one part of a system can result in changes elsewhere in that system.
- Focusing on exceptions/solutions to problems will make it more likely that exceptions will outweigh problems.
- Focusing on the problem and history of the problem is unhelpful.
- The client's own beliefs, rules, language define the reality of the therapeutic conversation, without reference to external or 'objective' concepts such as normal or correct behaviours/beliefs.

- The client sets the goals and identifies when they have been met and when therapy should stop.
- The therapist is a collaborative facilitator, not an 'expert'.

Training in SFBT typically involves 2 or 4 day workshops which focus almost entirely on technique, and make little reference to the sources of various solution-focused ideas. The training approach is explicitly practical and prescriptive (Shilts and Gordon, 1996) which sits uneasily with some of the frameworks used (for example the relativist assumptions about reality mentioned above), and is certainly far removed from traditional clinical psychology training with its emphasis on a theory-driven scientific basis for any therapeutic technique, and its reference to complex variables which may need to be taken into account in understanding cause and effect. SFBT training is certainly a challenge to this tradition as participants are exposed to aspects of Ericksonian hypnosis, behavioural analysis, systemic models and even Eastern philosophy (George et al., 1990) along the way.

Theory in solution-focused brief therapy

Tracking the theoretical development of SFBT is a challenging task. The approach draws upon a variety of theoretical perspectives and even different scientific paradigms. As Cade and O'Hanlon comment in their Brief Guide to Brief Therapy (1993) "brief therapists are identified more by how they act than by their theoretical formulations."(p.10). However, SFBT is presented by its proponents as a distinctive therapy using specific techniques for specific purposes, and as such it is reasonable to 'unpick' SFBT to assess its theoretical status. For the purposes of this review a theory is taken to be a set of propositions which form a deductive system. That is, "there must be a logical connection between the propositions and between at least some propositions and the world of experience" (Klein and White, 1996, p.14). This assumes of course an empiricist view of knowledge and science which is itself challenged by some SFBT therapists.

De Shazer first explored solution-focused ideas in the early 1980's while working as a family therapist at the Brief Family Therapy Center. He departed from the tradition of strategic 'problem solving' therapy practised by Brief therapists at the time by proposing that there should be "a clinical focus on solutions rather than problems" (Molnar and de Shazer, 1987). de Shazer, in considering a conceptual basis for solution-focused tasks, at first suggested that the interventions could be explained from a number of different theoretical perspectives. He drew upon systemic theory in general, the work of Milton Erickson and, interestingly, cognitive and behavioural theory. In an early paper, for example, Molnar and de Shazer (1987) state:

"the work of Mahoney (1974), Beck (1967) and Ellis (1962), who have emphasised the significance of thoughts in changing feelings and behaviour, might, for example, lead to therapeutic practices which closely resemble solution-focused tasks." (p. 351).

Other writers too have noted conceptual similarities between cognitive theory and SFBT. Johnson and Millar (1994) describe SFBT as combining a number of techniques including the focus on meaning of cognitive therapy and the goal orientation of behaviour therapy. Cade and O'Hanlon, key therapists and trainers in the field of SFBT state that:

"Brief therapy is essentially concerned with observable phenomena, is pragmatic and related to the belief that problems are produced and maintained

1. by the constructs through which difficulties are viewed.... and
2. by repetitive behavioural sequences" (1993 p.5)

Cade and O'Hanlon do not acknowledge this resemblance to cognitive behavioural theory themselves, but use a mix of empirical studies drawn from experimental, perceptual and social psychology to support their description of the basis of brief therapy and SFBT. For example the chapter entitled “What is it that happens between the ears?” makes brief reference to construct theory, experimenter bias, gestalt theory, interpersonal theory, along with biology and neuroscience, combining these to arrive at what they describe as “the most parsimonious frameworks for understanding mental processes” (p.29). The links proposed are tenuous and the evidence presented idiosyncratic, but the authors’ attempts to generate a model of therapy are further undermined by later chapters in which they take an anti-empiricist stance which of course cannot be reconciled with the empirically based evidence relied upon earlier.

Cognitive and behavioural elements are clearly evident in the techniques of SFBT and feature in the conceptual accounts referred to above. However SFBT could not be regarded as a cognitive behavioural *theory*, essentially because other propositions in SFBT derive from very different theoretical frameworks. SFBT was first developed within and by a community of brief family therapists and draws on systemic strategic models in both technique and in terms of theoretical orientation. Systems theory in general has been subject to criticism, not least the charge that it is not a theory in any scientific (i.e. hypothetico-deductive) sense but a model or “flow-chart approach”. (Klein and White, 1996). Other criticisms include the problem that many ideas in systems theory are too vague and abstract to be meaningful, and the assertion that family therapists tend to reify the idea of a system, that is they attribute reality to a heuristic model for understanding. In so far as SFBT has some of its origins in systems theory these criticisms might equally apply to the approach being discussed here.

It is outside the scope of this review to describe or critique systems theory in general, but in any case a fundamental paradigm shift in systems theory and hence family therapy in recent years neatly side-steps criticisms made from an empirical standpoint regardless of the detail. Constructivist paradigms have emerged in family therapy which draw on post-modern ideas from the fields of philosophy, sociology, the arts and politics. Social constructivism brings into question the notion of an objective reality that can be observed and measured, instead proposing that all knowledge is socially constructed through the ideas, meanings and linguistic rules that a particular group or society chooses to adopt. Thus there are no scientific facts or value-free absolute truths but rather ways of viewing the world that are socially defined and that can change according to who is in a position of power to define and describe those 'truths'. Madness, disease, dysfunction, normality are socially constructed concepts defined through the discourses of science, medicine, law and so on. These ideas have been taken on and developed by a number of different family therapists and in the process quite different 'versions' of constructivist thinking have emerged. Some practitioners have allied themselves with the ideas of the social and political philosopher Michel Foucault (1965), and developed what is termed narrative therapy (White and Epston, 1990; Monk et al., 1997). In this approach, clients' problems are seen as stories or narratives shaped by the social, cultural and political context. Clients are helped to deconstruct these dominant stories and through recognising exceptions or unique outcomes (for example occasions where they were able to cope better), are able to build alternative, more hopeful narratives about their strengths and possibilities for change.

De Shazer, in contrast to his earliest writings, has in recent years placed his own version of the constructivist paradigm at the heart of SFBT, and presents his "theoretical analysis" in his book Putting Difference to Work (1991). Given that de Shazer is still regarded as the main influence in SFBT it is important to look at his ideas in some detail. While some of the narrative therapy ideas are also found in SFBT (the premise that 'exceptions' are a way of undermining a problem-dominated story for example), de Shazer departs from narrative's affinity with Foucault and instead cites the philosophers Wittgenstein and Derrida as key influences on his thinking about SFBT. The constructivist ideas about power, politics and knowledge are noticeable by their absence, and in their place is selected aspects of both Wittgenstein and Derrida's constructivist linguistic philosophy. De Shazer makes the point at the beginning of Putting Difference to Work that his analysis is;

"certainly not a Theory with a capital T; rather, the analysis leads away from such a grand design, emphasizing instead the variability of the clinical situation and the people involved (both clients and therapists), the variability of events, and the variability of problems and solutions." (p.10).

This position raises a number of issues. Firstly de Shazer explicitly rejects the idea of SFBT being a theory in any formal sense. This is not a surprising position to have taken of course, but it leads to considerable difficulties when considering attempts of de Shazer and his colleagues to define, measure and evaluate the effectiveness of his approach. This will be addressed in the next section.

De Shazer's comments about variability relate to a constructivist paradigm and in particular Wittgenstein's ideas about language. De Shazer applies the concept of *language-games* to the therapeutic conversations which take place in SFBT. In essence Wittgenstein argued against private mental states, processes or independent rules lying behind language, but saw language as a host of different activities such as describing, giving orders, questioning, warning etc. which have their meaning only in terms of the use they are put to, no more. These socially constructed activities he called language-games and since the rules of language rest on the agreed practices of a community they are their own justification, with no objective world or set of facts governing their use. De Shazer applies these ideas to SFBT in the following way;

"The therapeutic relationship is a negotiated, consensual, and cooperative endeavor in which the solution-focused therapist and client jointly produce various language games focused on (a) exceptions, (b) goals, and (c) solutions." (de Shazer, 1988).

As many authors have noted, Wittgenstein's philosophy is extremely difficult and complex, being described as "cryptic, elliptical and dogmatic" (Flew, 1979). His later work, which de Shazer draws most heavily on, is elaborate, vague and metaphorical. Critics have argued that on closer scrutiny his ideas about language and meaning are problematic (for example his ideas require him to argue from a position of cognitive relativism which is generally regarded as untenable), and conclude that his later philosophy is "not as it stands persuasive." (Grayling, 1988). de Shazer does not address these difficulties, and his versions of Wittgenstein's ideas bear only a superficial resemblance to the philosophical works he draws upon. Similar criticism can be made of de Shazer's use of Derrida's most complex ideas about words which are appropriated for the purposes of underpinning with philosophical foundations manoeuvres like looking for exceptions.

In summary, de Shazer's ideas can indeed be said to be ambitious, with de Shazer described as "trying (too hard) to provide a (pseudo) philosophical overlay in pursuit of respectability which his thinking and previous work do not require." (Jenkins, 1993). The word 'overlay' is significant here, for de Shazer's philosophical analysis appears to be post hoc, with all the weaknesses that that implies. His suggestions about what his therapy *is* may have changed radically since his earlier references to behavioural theory, but he has not yet offered us a coherent framework in their place.

The claim to be working within a constructivist paradigm made by both narrative and SFBT approaches is itself open to criticism. Gibney (1996) argues that these therapies are simply versions of strategic family therapy and accuses them of reconstructing the wheel! He states that;

" Clearly, both these practitioners have valuable practice methods to offer, but is the family therapy community best served by their attempts to locate themselves in French poststructuralist philosophy, as opposed to their more obvious connections with American pragmatic therapy?" (p. 99)

Other authors too have questioned the validity of SBFT's claim to be constructivist or poststructural. Atwood (1995) claims that SFBT is in fact *modernist* in that it operates from a position of certainty that families without problems have solutions and that the therapy of choice is to focus only on those solutions. Hence there is an implicit definition of competency behind the technique. Furthermore, the emphasis in SFBT on prescriptive technique and word play where a therapy session is a "self-contained linguistic system" (de Shazer and Berg, 1992), is a far cry from a 'deconstruction' of the sociocultural construction and constraints of meaning and language in the spirit of Foucault.

Before concluding this overview of theory in SFBT it is worth mentioning a few further strands to the debate. Milton Erikson was a key influence on the community of Brief family therapists. He was regarded as a great strategic therapist whose innovative ways of helping people to change, including his hypnotic techniques, are adapted and applied in SFBT (Cade and O'Hanlon, 1993). Some of his hypnotic approaches, for example, were incorporated into an SFBT workshop on therapy with victims of abuse as coping techniques for dealing with dissociation. How his persuasive and directive techniques relate to a constructivist view of SFBT is not clarified in the literature. This highlights again the uneasy alliance between the different elements of the solution-focused approach.

Some therapists have argued that strategic therapy can be construed as an insight-oriented approach (Duncan and Solovey, 1989), and that therapist-ascribed meanings (such as reframing exceptions as solutions) share features with psychodynamic interpretation, an interesting view given that psychodynamic theory is an anathema to de Shazer's constructivist position. Whether or not this analogy holds in terms of formal psychotherapy, it raises valid questions about the impact of the therapist and the therapeutic relationship in SFBT which are not adequately addressed in the literature. Several authors have noted the lack of attention paid to emotion, transference and so on, and have argued cogently for a synthesis of strategic and psychodynamic theory (Flaskas, 1996). This mirrors in some ways the development in recent years of new models of cognitive behaviour theory and therapy, where greater emphasis is being placed on the therapeutic relationship in helping people change (Coon, 1994).

In summary, SFBT is not one coherent model of human behaviour or of therapeutic change. As reviewed in detail here, it incorporates many diverse features culled from other models and theories, some of which stem from competing philosophical paradigms. Its coherence derives mainly from an identified collection of techniques commonly quoted in the SFBT literature, underpinned by a value system which challenges current orthodoxy in therapeutic practice, particularly pathological categories and linear or 'cause and effect' aspects of many scientific models of mental ill-health. SFBT is far from achieving the status of a theory in any formal or scientific sense. Fundamentally it lacks any explanatory power. This is something de Shazer would happily agree with. He states of SFBT that " one cannot know how it works, one can only know that it does work." (de Shazer, 1991, p. xvii). However, his remarks seem disingenuous, especially given the promotion of SFBT as a powerful and effective therapy. Whether in the form of a scientific enquiry or philosophical debate, therapists need to make explicit their ideas, values and biases regarding why SFBT works, and thus allow the approach to be evaluated in a systematic fashion.

Studies in Solution-focused brief therapy

SFBT proponents claim that their approach helps people solve problems quickly and effectively. Unfortunately there has been little in the way of systematic research to support this contention. From the start family therapy has developed outside of the mainstream scientific communities of psychiatry and clinical psychology, and historically did not follow the tradition of employing formal quantitative research to explore its effectiveness, though in recent years a number of case-controlled studies have been published (Crisp et al., 1991). SFBT, regarded at times as a maverick approach even within the family therapy field, has had even less of a presence in the scientific literature.

There are a number of issues which make it difficult to evaluate SFBT. Firstly, since SFBT is claimed to spring from a constructivist paradigm which is explicitly anti-empirical then it becomes 'immune' to positivist methods of enquiry. Related to this point is the description of SFBT as *non-normative*, (de Shazer, 1991) which effectively rules out any external validation of its claims of success. Secondly, even if SFBT is conceptualised within an empirical framework there are serious obstacles to its study. The fact that it is not a theory has already been discussed above, but what we are left with is a disparate collection of ideas with no agreed definition of what solution-focused therapy is in operational terms. Different therapists appear to have different and quite personal views of what constitutes SFBT. Despite the assertions made by some practitioners that SFBT is evidence-based, the evidence presented for its efficacy remains largely outside the 'scientific' approach.

Published studies on SFBT are few. Some take the form of simple case examples, (Lignon, 1996; Shilts and Gordon, 1996), while others are more general audits of the use of the approach in practice. Mason et al (1995) applied SFBT in an addictions treatment clinic and make some important points about how SFBT helped them challenge overly rigid, client-blaming practices. Mason et al used their approach with five clients two of whom did not improve, but client and therapist satisfaction were reported as high. Thus, in empirical terms this study says little, but the authors are enthusiastic about SFBT nonetheless and write of their intention to adopt this framework in their clinic. From an evidence-based perspective, and in terms of good clinical practice, it might be considered inappropriate to adopt an approach which may be popular but as yet unproven. The issue of client satisfaction will be returned to later.

The constructivist perspective of the solution-focused approach perhaps lends itself more readily to qualitative analysis. Franklin (1996) chose to explore the change processes in SFBT using Recursive Dialectic Analysis. However, once again a different form of SFBT was used where standardised assessment forms were administered both as outcome measures but also as a way to "normalize behaviour, reframe cognitive appraisals concerning problems, and reinforce changes" (p. 34). This represents quite a shift from de Shazer's view of non-normative language games. The case presented was also not typical of SFBT. The couple concerned were still being seen after 40 sessions over 17 months, which is hardly brief, and the treatment included focusing on past family of origin issues. In terms of results, statistics are not presented and the qualitative analysis is not described in sufficient detail to allow an evaluation. Metcalf and Thomas (1994) published a qualitative study which is interesting because it assessed six therapists and their clients' views of solution-focused therapy carried out at the Brief family Therapy Center, where de Shazer developed SFBT. One might expect the therapists here to apply a fairly 'pure' version of the therapy. Using a simple coding system to identify themes in structured interviews, the researchers found that descriptions of what the therapist did differed greatly, with clients regarding their therapists as more active and directive than the therapists judged themselves. Furthermore they conclude; "the therapists' use of pathological terminology deviate from the philosophy of solution focused brief therapy philosophy stated in de Shazer's descriptions" (p. 59). Regardless of the validity of the qualitative analysis, fundamental concerns are once again raised about the nature and definition of SFBT. Johnson and Miller (1994) present a paper on treating depression in which they combine Seligman's cognitive model of the disorder with SFBT and present a case example to illustrate their approach. The authors propose that cognitive-behaviour therapy takes too long to achieve results and that SFBT helps clients more quickly. They state that the 'theory' seems to yield "very good results in the majority of cases" (p. 251). If shown to be the case this would indeed be a challenging finding for clinical psychology, where cognitive-behaviour therapy is the treatment of choice for depression. Unfortunately Johnson and Miller do not present any supporting evidence for such a grand claim. They do point out that SFBT is particularly reliant on client reports of outcome and that this is inadequate for research of a new technique. They suggest standardised assessment tools and two year follow-up to address the significant problem of monitoring relapse.

In general, research evidence, qualitative or quantitative, has been noticeable by its absence, and in response to this the Journal of Family Therapy devoted a whole volume in 1997 to research in SFBT. The editorial (Carpenter, 1997) notes that the extravagant claims made in the literature are no longer sufficient, and that simple success measures on clients who complete do not establish effectiveness or efficacy. It argues that control groups and standardised pre and post treatment measures are required to properly evaluate the approach. This is uncontroversial from an empirical perspective, the paradox comes when de Shazer and Berg then introduce the volume in question by arguing that since they and his colleagues were always trying something then seeing if it worked, " the approach can be described as experimental and research orientated from the beginning.... we saw what we were doing was sufficient in itself as a research endeavour." (de Shazer and Berg, 1997 p. 121). This claim is quite startling given de Shazer's explicit rejection of empiricism and scientific enquiry in earlier works. They suggest that research has been minimal since SFBT was developed in 1982 because they and their clients were "busy inventing a rather radical approach to 'therapy' " (p.121). Regardless of scientific orientation, the suggestion that a therapist might be too busy to research the effects upon their clients of a new therapy approach is open to serious criticism from an ethical standpoint.

The papers presented in the special issue are an eclectic mix of approaches. Zimmerman et al compared pre and post test changes in a couples therapy treatment group and a no-treatment group. Standardised assessment tools were used. However because of significant bias in the recruitment procedures (treatment couples were self-selected) the researchers did not compare pre and post scores *between* groups and neither did they present data on changes over time in the no-treatment group. Effectively therefore, there was no comparison group and we are left with an uncontrolled study showing only that the treatment group improved over time. A further crucial weakness is that the SFBT described here omits characteristic, and arguably necessary, features of the approach such as the Miracle question and scaling procedures. The only other paper to include a comparison group also used an idiosyncratic variation of SFBT. Eakes et al piloted a solution-focused model of working with "schizophrenic individuals" (a term that according to constructivist theory would be rejected as a socially constructed concept to exert social control). The study treated five volunteer families with five SFBT sessions and compared them with five families receiving standard care. The authors give little information about the treatment procedure or its aims, and used only the Family Environment Scale (Moos and Moos, 1994) to evaluate the results. The authors reported that the experimental families increased in expressiveness and congruence, although it is unclear what these changes represented in clinical or practical terms. As the authors point out themselves, there is an urgent need for more thorough investigations of this approach.

Beyebach and Carranza (1997) present a detailed analysis of relational communication in solution-focused therapy. While it is difficult to judge the merits of the complex procedures used, the study does address the issue of dropout in SFBT, a topic which is often ignored because, since the client decides when to finish therapy, there is logically no such thing as premature or inappropriate cessation of treatment. As the authors point out, this is not a satisfactory way of addressing what is a significant clinical and ethical issue.

In summary, while there is a welcome move, in family therapy if not in clinical psychology, to research more carefully the theory and practice of SFBT, the task is fraught with difficulty. Conflicting models, lack of an operational definition and minimal empirical evidence require that conclusions about the merits of this approach must at best be tentative. De Shazer and Berg's pronouncement that SFBT is "consistently successful" (1997, p.122) is extravagant in the extreme.

Ethics and professional issues in Solution-focused Brief Therapy

Clinical psychology, as with other clinical professions, is constantly developing in terms of both theory and practice. It is important that the profession continues to adapt to increasing knowledge and changing socio-political perspectives. SFBT is one of many approaches that may offer new insights and ideas to clinicians. However, new approaches need to be adopted within an ethical framework which addresses issues such as effectiveness versus possible harm to clients, competence of the therapist to practice and availability of adequate training and supervision. Given the theoretical confusion, lack of evidence and difficulties in assessing standards of training workshops offered in the field of SFBT, clinical psychologists should be cautious in applying these ideas.

SFBT, taken at face value, is an attractive proposition for many therapists. The fact that it is easy and relatively cheap to become trained in the approach has already been mentioned. SFBT also claims to be empowering, and less pathologising and stigmatising than traditional psychotherapeutic models of care. This certainly appeals to many clinical psychologists who quite rightly wish to challenge "the mythology of the personal inadequacy of patients and the competence of the expert." (Moorey and Markman, 1998: p.19), and wish to encompass social and cultural issues in their work. Unfortunately SFBT writers and practitioners on the whole have *not* addressed these issues in any sophisticated manner, and moreover have no evidence that SFBT clients feel any more empowered or destigmatised than clients of other therapies. For example, the literature on SFBT has not confronted the issue of therapists' power. It is claimed that SFBT is consensual and non-authoritarian (George et al., 1990), and yet the approach is explicitly directive and strategic. Metcalf and Thomas (1994), in their analysis of process in SFBT sessions found that several clients felt disappointed that the therapist had terminated the therapy prematurely, even when the therapists themselves thought they were following SFBT principles and accepting the clients' statements of satisfaction as the deciding factor in terminating. This hints at the problems with a naïve assumption that therapy can be truly consensual and egalitarian (as well as possible evidence of harmful outcome which is not taken up by the authors). Of course other therapies, including supposedly 'collaborative' cognitive-behaviour therapy, can and have been criticised for failing to pay attention to power and disadvantage in their practice. This is beginning to be addressed in clinical psychology (Smail, 1995). Ironically, given its purported links with constructivist thinking which critiques power in institutions, SFBT has yet to properly consider these issues.

Solution-focused approaches may have a useful contribution to make to therapeutic practice - for example practitioners in the field appear to find SFBT techniques helpful in engaging and motivating clients. The approach certainly shares some positive common ground with cognitive-behavioural therapy in its emphasis on collaboration with clients, normalising of pathology, and reinforcement of things that clients find helpful. The constructivist theme too is one partly shared by cognitive-behavioural theory whereby therapists accept a client's 'reality' and help them to make changes within their personal context. Thus, there may be aspects of SFBT which would repay further exploration and research by psychologists. Clinical psychologists are perhaps in a good position, by virtue of their training, to evaluate the merits of SFBT. As Smail (1995) suggests, most clinical psychologists have a commitment to evidence and critical appraisal in their work. These concepts need to be applied most carefully when considering the use of solution-focused brief therapy with our clients.

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Cognitive-behavioural therapy and schizophrenia: Current status and applications to early intervention

Introduction

Schizophrenia is a serious mental health problem with significant consequences in terms of distress and disability for the individual, and social and economic costs of the illness. Since schizophrenia was described by Kraepelin 100 years ago, a mass of research on theories and treatments of the disorder has accumulated. This has tended to be based on a medical model of schizophrenia as a biological disease (Bentall, 1990a). Recently, medical research has focused on understanding in more detail the development of schizophrenia, its stages, and opportunities for treating it early in its course to prevent or delay the illness (early intervention).

Psychological theory and therapy has played little part in traditional accounts of schizophrenia. Some researchers have argued that this stems from several misconceptions about the disease including the idea that it is a biological disorder thus offering no role for psychological interventions, that it is adequately treated by medication, and that it is too severe for psychological approaches. (Birchwood and Shepherd, 1992). However, in the last 10 years, evidence has been accumulating for the usefulness of psychological models and treatments, and a great deal of optimism has accompanied these developments. Most recently psychologists have turned their attention to therapy earlier on in the disorder. Early intervention and psychological treatments are both in their infancy as new developments. This critique looks at how they have been synthesised and researched.

Schizophrenia is complex not just in its pathology but in its relationship to cultural, social and political issues. An account of psychological approaches needs to begin with an appreciation of these issues and how they relate to clinical psychology practice in this field. This essay will begin with a brief outline of the concept of schizophrenia and the controversy surrounding diagnosis and models of aetiology. The natural history of the disease will be outlined, followed by a look at the recent interest within psychiatry in early intervention in schizophrenia. The application of psychological theory and interventions to schizophrenia in recent years will then be addressed with particular focus on cognitive-behavioural therapy (CBT). Recent developments in applying CBT in early stages of the disorder will be addressed with a review of the research in this area and an assessment of the ethical and professional issues it raises.

Conceptual issues in schizophrenia

Schizophrenia is characterised by a range of symptoms including auditory hallucinations, bizarre and irrational beliefs (delusions), disordered thinking, poverty of affect and impaired social functioning. Definitions such as those in DSM-IV (Diagnostic and Statistical Manual) provide apparently clear criteria for the diagnosis of schizophrenia and the term is widely accepted in psychiatry and psychology as representing a recognised illness with its own distinctive course and outcome. However, alongside this orthodoxy there exists considerable philosophical and scientific controversy surrounding the concept of a schizophrenic disorder. Challenges have been made most notably by R.D. Laing (1967), who located the problem of schizophrenia not in the patient but within family and society, and criticised psychiatry for its treatment of psychotic distress as simply medical symptoms. Another important critique of psychiatry came from the psychiatrist Szasz (1964), who claimed that mental illness was a socially constucted myth, and also from within the domain of social constructivism particularly through the writings of Michel Foucault, who argued that definitions of abnormal behaviour are culturally constructed and applied by those in power in order to exert social control over those who deviate from social norms (Fillingham 1993). Foucault's work has had profound influence in many disciplines such as philosophy and sociology, but is notable by its absence from mainstream psychiatric or psychological discourse on schizophrenia.

Whilst steering clear from questioning the reality of mental illness *per se*, other writers have challenged the very narrow construction of schizophrenia as a purely biologically driven disease. Warner for example acknowledges the existence of an illness called schizophrenia but presents convincing evidence for the powerful effects of social factors, notably economic conditions and employment opportunities, on the course and outcome of the illness (Warner, 1994). Bentall (1990), a psychologist, has also argued strongly against the prevailing paradigm which he describes as "the ideology of biological reductionism", and through his detailed critiques of the research concludes that an exclusively biological explanation of schizophrenia cannot be defended on either scientific or logical grounds. More recently, within academic psychiatry itself, there has been debate about the coherence and logic of the DSM-IV definition of schizophrenia which has been criticised for its lack of "an underlying paradigm" (Maj, 1998).

These challenges to the medical orthodoxy are seldom debated head on in the literature, but it is increasingly acknowledged that social, psychological and cultural factors must be given due attention in any understanding of schizophrenia. Whilst the existence of a schizophrenic genotype is rarely disputed in psychiatry or psychology research the field is increasingly receptive to a *vunerability* model of the disease in which both genetic vulnerability and stressful environment are necessary precursors to the onset of psychosis (McGlashan and Johannessen, 1996). This is certainly the predominant framework used in clinical psychology research and most of the developments in psychological treatments of schizophrenia have used this essentially reductionist model to underpin the research and are not therefore as 'revolutionary'or non-medical as they might first appear. Birchwood and Shepherd (1992) welcome the vulnerability model as having potential to "drive clinical formulations and interventions that operate on a number of dimensions",and suggest that it offers a rapprochement between different models of mental illness. Certainly it has allowed pyschological perspectives to enter the psychiatric mainstream in terms of research into schizophrenia.

Despite the enormous amount of research into schizophrenia no clear etiological theory has emerged. Numerous factors have been implicated in causing the disease such as genetic endowment (Van Os et al, 1997), brain abnormalities (Crow 1998), maternal stress (Van Os and Selten,1998), season of birth and viral agents (Crow 1984). There are almost as many theories about the etiology of schizophrenia as there are theories of psychopathology in general, but there is little evidence to substantiate any one school of thought. The most favoured model is, as mentioned above, the stress-diathesis or vunerability model of "a psychological disturbance mediating genetic and environmental effects on the causal pathway to the illness" (Malmberg et al, 1998). However, the lack of concensus on etiology only adds to confusion about the model or conceptualisation of schizophrenia that underpins current research in this area.

This brief appraisal of some of the criticisms of the concept of schizophrenia provides a backdrop against which to assess the contributions of pychology, in particular clinical psychology. The strong empirical and research traditions in psychology have shaped the recent developments in pyschological theories of schizophrenia. Whether this body of work has challenged current concepts of serious mental illness, or added to the debates outlined above remains to be shown. This issue will be returned to later in this review.

The nature of schizophrenia

Diagnosis

It is now widely acknowledged that there is no discreet diagnostic category of schizophrenia as originally envisaged by Kraepelin, but rather a psychosis continuum best described by several symptom dimensions (Van Os et al, 1997). Boundaries between disorders are, it is argued, necessarily arbitrary to some extent based on the different sets of criteria used throughout the history of schizophrenia research (Crow, 1998). Definitions that use symptoms alone have poor validity but the major diagnostic classifications of DSM-III-R and ICD-10 do appear to have high predictive validity and are claimed to provide relatively stable diagnoses (Mason et al, 1997). In contrast to this optimistic conclusion, others such as Bentall (1990) have argued that schizophrenia is a 'catch-all' category covering such a heterogenous mix of cases that it can predict neither outcome or response to treatment, and hence the classification should be abandoned in favour of a dimensonal model of mental disorder. This has already had some impact on psychological research in the shift towards studying individual symptoms within a framework of normal cognitive psychology. However, most researchers, whatever their discipline, continue to refer to standard classifications of schizophrenia in their work and so conclusions about treatment and other clinical implications tend to follow in the tradition of psychiatric models of illness.

Course and outcome

The clinical course of schizophrenia is much more varied than had been supposed by the earliest accounts of this illness (Bentall, 1988). For some sufferers the first psychotic episode is followed by complete recovery whilst in others life-long decline is apparent. For the majority, episodes of psychosis alternate with periods of remission over many years followed by partial or complete recovery. It has been estimated that as many as a quarter of patients may completely recover and 40 to 45 per cent will 'socially recover', that is, achieve economic and residential independence (Warner, 1985; Bentall, 1990). A further finding which has to be taken into account in longitudinal research studies is that, whilst deterioration occurs in both treated and untreated psychosis, it usually stabilises between two and five years and may even relent among those who deteriorate most (Jackson and Birchwood, 1996). However, studies of outcome are difficult to compare or aggregate because of wide variations in diagnosis, sampling techniques and definitions of recovery and only broad generalisations are possible.

Numerous factors have been shown to influence the course and outcome in schizophrenia. Males tend to demonstrate earlier onset of psychosis and poorer functioning, whilst patients with insidious onset also have a poorer prognosis (Larsen et al, 1996). Stress factors which have a significant impact on the course of the disease include life events, finances, housing, employment, social supports and family relationships (Falloon et al, 1996; Salokangas 1997). On a broader level of analysis, urbanisation, cultural origin, and socioeconomics have been shown to exert significant effects on the way schizophrenia develops and is expressed (Craig et al, 1997; Takei et al, 1998: Varma et al, 1997). Indeed there is evidence to suggest that social indicators of functioning are more powerful predictors of outcome than clinical symptoms (Craig et al 1997; Jackson and Birchwood 1996). This desynchrony between social factors and clinical symptomatology must be considered when looking at research into treatment outcome studies in psychiatry and psychology which often rely on clinical measures to assess change. A purely individualistic, biological model cannot adequately account for these social influences.

Stages in Schizophrenia

According to most writers, schizophrenia progresses through a series of stages and the early course consists of three phases: the premorbid period, the prodromal period and the acute psychosis. Following on from this an individual may experience a number of remissions and relapses with the latter signaled by further prodromal phases (relapse prodromes). Whilst these stages are referred to widely in the literature, and indeed a prodrome is included in the DSM-IV description of schizophrenia, they are by no means well-defined or easily detected phenomena. The prodrome phase, for example, is defined by a nonspecific set of symptoms (such as depressed mood, sleep disturbance and anxiety) and can be the early phase of a number of other psychiatric illnesses. Researchers also claim that the length of the phase can vary from days to many years. There is huge individual variation in a patient's experience of schizophrenia, with some people missing out the prodromal or onset stages all together (McGlashan and Johannessen, 1996). In spite of this astonishing heterogeneity, the prodome is still widely accepted as a valid construct in psychiatric studies.

Of course a major problem with research into the early stages of schizophrenia is that the studies have been wholly retrospective, relying on patient or carer accounts of the emergence of a breakdown. As Yung and McGorry (1996) point out, a prodrome is a *retrospective concept*, diagnosed only after the development of definitive psychotic symptoms. Analogies have been drawn between the better-studied early warning signs just before a relapse and the initial prodrome in order to arrive at models of the onset of schizophrenia, but such a comparison is beset with weaknesses as the two types of prodome are likely to be qualitatively different (Vaglum, 1996). Even the more well established pattern of remission and relapse later on in the illness is open to challenge, with some authors within psychology stating that a stage model is not adequate to conceptualise the changes in symptomatology and that a multi-dimensional aspect needs to be incorporated (Drury 1994).

Early Intervention

New developments in the early treatment of schizophrenia, including psychological approaches, are largely based on an acceptance of a stage model of schizophrenia. Such confidence in constructs like premorbidity and prodome seems unwarranted in the light of the serious shortcomings outlined above. However, within psychiatry there has been an increasing emphasis on targeting the early stages of schizophrenia in order to develop treatments which might ameliorate or even prevent further deterioration. Psychiatrists acknowledge that despite modern neuroleptics and improved care, schizophrenia remains a severe and chronic disorder for many sufferers, with treatments offering only palliative results. (McGlashan and Johannessen, 1996), and the search for earlier and better interventions is being pursued with great optimism (McGlashan, 1996). Some of the developments in research psychiatry are outlined below in order to place in context the psychological research on early intervention to be described later in this essay.

Early intervention is a loosely defined concept variously applied to; identifying and treating premorbid or prodromal individuals, early treatment once psychotic, or even treatments applied to try to forestall a relapse. This range of definitions makes it difficult to compare studies and weakens the theoretical bases on which early intervention rests. However, studies of first-episode psychosis have shown that many schizophrenia sufferers have signs of psychosis for months or years before they present for treatment. This phase of early psychosis is termed duration of untreated illness (DUP), and a long DUP is strongly correlated with poorer outcome in a number of domains (Larsen et al, 1996). This finding is a cornerstone of the argument that earlier treatment will result in better prognosis. Several studies do lend support to this proposal. There appears, for example, to be a link between earlier medication and better outcome (Sheitman et al, 1997). Other more ambitious programmes have tested a comprehensive early screening process followed by an integrated intervention package of education, family work and neuoroleptic medication (Falloon et al 1996). The authors conclude that such a programme was effective in detecting and treating the early phases of schizophrenia.

In spite of the enthusiasm for early intervention in psychiatry there is still a paucity of empirical evidence to support such treatments. Several methodological problems exist, for example, most studies of DUP are retrospective and the association between DUP and outcome remains correlational rather than causal. Also some studies define their target population by a mix of prodomal and early-episode criteria which confuses two supposedly different stages in the disease (Falloon et al,1996). Finally, prospective studies of early treatment have regarded the *non*-appearance of full blown psychosis in their at risk samples as a successful outcome, and have used historical cohorts or epidemiological data to compare rates with. This method of analysis considerably weakens the confidence with which we can conclude that early intervention alters the course of schizophrenia. Indeed, a recent British Journal of Psychiatry supplement devoted to early intervention emphasised that these "exciting possibilities must be based upon sound evidence which can only arise from well-conducted clinical research" (McGorry, 1998, p.1).

Cognitive-behavioural therapy and schizophrenia

Cognitive-behavioural therapy (CBT) is a well established treatment model for a wide variety of psychological disorders, originating of course from Beck's seminal work on depression and his development of cognitive therapy for the neuroses (Alford and Beck, 1994). However CBT has, until recent years, made little impact on the treatment of psychotic disorders. As more evidence builds to show that schizophrenia is fundamentally shaped by and interacts with psychosocial factors, cognitive-behavioural psychologists have developed an impressive body of work in this field. Indeed, Alford and Beck (1994) argue that cognitive therapy should have a special role in schizophrenia given that thought disturbances and delusional beliefs, the 'bread and butter' of CBT, are central to the psychotic disorders. There follows a brief overview of some of the developments in CBT, using studies of delusional beliefs to illustrate some of the theoretical and methodological issues pertinent to this field.

The range of psychological approaches used in the treatment of psychotic symptoms is diverse but most incorporate some cognitive-behavioural element. Stress management, social skills training, problem-solving/coping strategies, self-esteem work and family management have all been found to be useful components of care packages for individuals with schizophrenia (Sellwood et al, 1994; Barrowclough and Tarrier, 1994). Perhaps the area which has progressed most in terms of theory-based cognitive-behavioural treatments is the study of the psychotic symptoms of hallucinations and delusions. Several psychologists have argued that schizophrenia is best understood by detailed study of such component symptoms within the context of a developmental cognitive model (Bentall et al, 1988; Claridge, 1990). Research in the last ten years or so has started to identify cognitive 'errors' which characteristically underpin disturbances as hallucinations and delusions, and corresponding CBT techniques have been proposed which might improve or at least compensate for these errors of thinking. This approach is not entirely new. Meichenbaum used cognitive techniques to attempt to alter specific thinking errors of schizophrenic patients twenty-five years ago (Meichenbaum and Cameron,1973), but recent randomised case-controlled studies, such as those conducted by Kuipers and her colleagues (Kuipers et al, 1997) have added considerable weight to the evidence in favour of these CBT approaches. Many authors are enthusiastic about the potential for CBT and recommend that such treatments should be made available now for people with schizophrenia (Bentall, 1990a)

According to the DSM-IV, a delusional belief is one that, in the person's culture, would be regarded as totally implausible, for example, thought broadcasting (DSM-IV, 1994). Within a cognitive framework, delusions have been defined as "maladaptive cognitive constructions of internal or external phenomena" (Alford and Beck, 1994). Questionnaires designed to measure delusions in a standardised fashion have been developed, such as the Maudsley Assessment of Delusions Schedule (Wessely et al, 1993) and treatment usually consists of helping clients to review the evidence for their beliefs, gentle challenging, reality-testing and the presentation of possible alternative explanations. Early research has relied mostly on single case studies to assesss the merits of CBT for delusions (Fowler and Morley, 1989). Chadwick and Lowe (1994) used a multiple-baseline design to study the effects of cognitive interventions on 12 patients with a diagnosis of chronic schizophrenia. They reported that 10 participants showed reductions in their level of belief conviction as a result of several weeks of structured verbal challenging and reality testing. The authors are appropriately cautious in their conclusions pointing out methodological weaknesses and issues of validity. However, they do state that the weight of evidence indicates that delusions can indeed be modified. Unfortunately, the more recent case-controlled studies of CBT for psychosis by Kuipers, Garety and colleagues (Garety et al, 1997), which are widely quoted in support of the CBT approach, incorporated a variety of cognitive techniques and targeted not only delusions but hallucinations, self-esteem, social stigma and medication compliance. Therefore it is not clear from these studies which aspects of delusional thinking might be modified by which treatment technique.

Of course, Chadwick and Lowe (1994) acknowledge that delusions are "multidimensional phenomena" and rarely exist in isolation from other major mental health problems. Bentall's call to researchers to study (and by implication treat) individual symptoms rather than disorders (Bentall et al, 1988) is fraught with difficulties, and the idea that CBT might be relatively simply translated from one domain to another is proving premature. The more delusions are investigated the more complex they appear in terms of their pheonomenology and also their relationship to other symptoms such as hallucinations and depression. In fact the debate about the essential nature of delusions remains unresolved.

From an epistomological standpoint for example, the definition of a delusion as a false belief has been criticised as illogical and reliant on circular reasoning (Levy, 1996), whilst others have pointed out that delusions are not always or necessarily false, they are sometimes not belief but judgement (Fulford, 1993), and it is not yet shown how they may be reliably distinguished from 'normal' beliefs and ideas such as religious faith (Jones and Watson, 1997). Berrios (1991) goes further in stating that delusions are merely "empty speech acts", that is, they are simply random fragments of information, a stance which certainly precludes a cognitive model of treatment. Delusions are not only philosophically troubling but are proving difficult to conceptualise in terms of cognitive theories of information processing. Some authors argue that the delusions of psychotic individuals reflect rational cognitive processes attempting to make sense of abnormal or anomalous perceptual or emotional experiences. Research by Maher in particular lends support to this hypothesis (Maher, 1992) and he remarks that if confirmed, this makes CBT potentially very relevant, since the historical notion in psychiatry that delusions are qualitatively abnormal,and imprenetrable by reason would no longer stand. Recent studies in cognitive psychology have added to this debate by suggesting that delusional individuals do show some subtle reasoning biases such as 'jumping to conclusions' but that this may be in some senses a functional process, for example as a way of reducing cognitive demands (Dudley et al, 1997), or even as a defence against depression or low self-esteem (Bentall et al, 1994). In summary no concensus has been reached regarding either the nature, taxonomy or origins of delusions, and treatment studies are very much in their infancy.

Research into CBT and hallucinations suffers similar conceptual and methodological critisisms to those levelled at delusion studies (Bentall, 1990b). However, many psychologists are still confident that CBT approaches are effective and use them routinely in their clinical work. Workshops, textbooks and diploma courses, not only for psychologists but many other professionals, are widely available. The field is developing rapidly, and the latest area of interest is in *early* psychological interventions.

CBT and Early Intervention

Most of the original studies in CBT and psychosis used samples of people with chronic, medication-resistant schizophrenia. However, in line with the psychiatry-led research into early intervention, research psychologists have started to look at the idea that the adage "a stitch in time saves nine" might apply to CBT approaches too. This interest is not so much theoretically-driven but seems to have been aroused by the findings that the early patterns of illness (e.g. duration of untreated illness) predict long-term outcome and that earlier treatment with neuroleptic medication improves prognosis (Drury et al, 1996). Aside from the serious problems with conceptualising and defining early psychosis, an analogy between pharmocological and psychological treatments seems an unsatisfactory starting point for developing new models of cognitive therapies. Some authors have likened the early phases of a schizophrenic breakdown to a "critical period" (Birchwood et al, 1998; Jackson and Birchwood, 1996), with the suggestion that damaging and maladaptive cognitions would form in the early years. There is little empirical evidence to support the idea of a critical period in cognitive terms - too little is known about the neurological and cognitive deteriorative processes in schizophrenia to arrive at this conclusion. Treatment studies in this area form a heterogenous cluster with varying definitions of early intervention and different types of CBT applied. No studies have been identified which look at CBT in the prodromal stages of schizophrenia. Even in early psychosis most psychosocial treatments are of the stress management or coping strategies type rather than the formal CBT of interest here. The exception to this is the study by Drury et al (1996), which offered individual and group cognitive therapy to a sample of first and second-episode pyschotic patients (which is described as early intervention since it concerns the acute phase of the illness). Compared to a control group there was a significantly greater decline in positive symptoms and delusional conviction at a nine month follow-up. The study suffered weaknesses in its small samples, and lack of an adequate control treatment consisting of extra leisure activities. Nevertheless this study suggests that CBT could play an important role in early intervention.

Another area of research which addresses acute psychosis is the work of Birchwood and colleagues into what they call secondary prevention, that is the early recognition of relapse prodromes with the aim of treating to avert or ameliorate the impending relapse. Psychological studies have contributed significantly to the understanding of relapse prodomes, and the conceptualisation of an individual's prodrome as a personalised *relapse signature* has prepared the way for thinking about interventions during this phase (Birchwood et al, 1989; Birchwood and Macmillan, 1993). However, the true predictive significance of these early warning signs and the type of treatments which might be appropriate have yet to be established (Birchwood, 1992).

Early intervention may be a 'hot' new area for psychological research, but there is an urgent need to synthesise the relatively few pieces of work emerging in this field into a coherent cognitive framework. Such a theoretical base would need to take account of the findings (briefly outlined earlier) from psychiatric research into stage models of early schizophrenia and the various predictors of outcome (for example the strong effect of gender) that have been identified. Cognitive models will need to arrive at a more sophisticated phenomenology of early psychosis and offer testable hypotheses regarding the developmental aspects of these early phases before we can be confident that specific early interventions can be recommended. Roberts (1992) points out that cognitive models will need to address and synthesise the numerous findings regarding delusions and states that present theory "has yet to account for the antecedents, formation, elaboration, and perpetuation of delusion" (1992, p.299), but have focused on the pragmatics of cognitive treatment whilst assuming that general cognitive theory for example about negative automatic thoughts and dysfunctional beliefs can 'fit' delusions just as well. Aside from theoretical considerations of course, as Jackson and Birchwood (1996) acknowledge, if the early intervention paradigm has a future it will have to be demonstrated that early outcome can actually be improved and much more research is needed to achieve this.

CBT - problems and future directions

Given the complexity of the disorder schizophrenia, it is not sufficient to import mainstream cognitive therapy techniques and assumptions into the field of schizophrenia research (Birchwood and Chadwick, 1997). There are numerous features of the disease which will influence the way in which CBT should be applied, for example the impact of general cognitive deficits, negative symptoms and thought disturbances need to be considered while the social and cultural aspects of the illness will also interact with any psychological processes. Interestingly, one development in the application of CBT techniques is an increased emphasis on the therapeutic relationship, regarded as crucial to build rapport, keep patients engaged and avoid threatening the patient's self-esteem (Alford and Correia, 1994). Such modifications to the mainstream model need empirical verification. Clearly more case-controlled studies are needed with attention being paid to the generalisability of the research to typical clinical populations. It would also seem important to study in more detail the phenomenology of schizophrenic processes from a developmental perspective including those individuals who recover well or even avoid relapse. Another theme which appears to be very promising is the emphasis on *meaning* in the symptomology of schizophrenia. Many research psychologists have argued that delusional and hallucination-related beliefs are not irrational or random, but are systematic attempts to make sense of and control psychotic experiences (Kingdon et al, 1994; Birchwood et al, 1993; Yusupoff et al, 1996). This mirrors the approaches of such self-help organisations as Hearing Voices (Baker, 1997) whose members point out that many people hear voices and yet are not schizophrenic, and that what helps them is addressing the personal meaning behind the experience then applying various (cognitive-behavioural) strategies to manage them.

Taking a user perspective such as Hearing Voices groups is an important area which psychologists could develop further. Little in the way of patient satisfaction has been addressed in CBT work, and the possibility that sometimes CBT might be detrimental or even 'hazardous' has not been systematically explored. Birchwood et al, (1993) reported that acceptance of illness by patients can, especially in the early stages of illness, result in pessimism and lowered self-efficacy. Adjusting to a diagnosis of schizophrenia is traumatic for patients and the effect of CBT, with its emphasis on rational challenging, may have negative as well as positive effects. More specifically, if, as has been suggested by Maher (1992), delusions are rational strategies which help the patient cope with distressing experiences, then one might argue that there are inherent risks in challenging these strategies both in terms of distress for the patient and the effectiveness of the CBT techniques. Such risks or 'side-effects' need to be fully addressed in the debate about the usefulness of CBT approaches.

Another issue related to patient satisafaction and ensuring positive outcome is the notion of quality of life. Few studies measure this, and yet quality of life (QOL) may not necessarily improve simply because delusional beliefs, for example, are held less firmly. The World Health Organisation recognises that QOL assesssments are an important part of keeping the patient at the centre of inquiry (Orley et al, 1998) and this needs to be reflected in future research.

The research into CBT for schizophrenia is not simply an academic or clinical exercise. Fundamental ethical issues permeate this field of enquiry. The notion of 'normalising' therapy which CBT subscribes to (Kingdon et al, 1994; Birchwood and Shepherd, 1992) is contentious one. Politically this raises a dilemma for those movements such as the Schizophrenia Fellowship wishing to emphasise schizophrenia as a biological illness with less reference to family or social influences. If CBT encourages a less 'medical' view of schizophrenia,and trains a patient to take control of psychotic symptoms, this may unfairly raise expectations of both family and wider society and imply blame or responsibility on the part of the individual. There is some evidence that this reaction does occur in families (Barrowclough et al, 1994). Considering expectations, ethical questions are also raised by the promotion of early intervention programmes which recommend "dedicated support for the first two or three years" (Jackson and Birchwood, 1996). The cost of setting up such programmes has not been discussed in the literature, but it seems premature to expect that such treatments could be financed in normal community settings, especially since recent research suggests that psychosocial interventions are only effective as long as they are active (Linszen et al, 1998).

CBT could play an important part in *destigmatising* schizophrenia through the reappraisal of the traditional medical views of madness and a more collaborative, respectful approach to patients' distress. However, a particular ethical concern with the move to early intervention is the risk of labelling many young people very early on as schizophrenic or marking them out as *likely* to develop the illness. This has serious implications for the individuals concerned, not least for those who may be misdiagnosed as false positives. This issue is touched on only briefly in the literature despite the acknowledgement by researchers that identifying people at risk is still fraught with conceptual and methodological difficulties.

Some psychologists have been keen to assert that psychological treatments like CBT offer a different paradigm of care to the traditional medical model which emphasises pathology and biological determinism and should be promoted as a priority (Bentall, 1990). The claim that CBT is an "antidote to the reductionist paradigm" (Birchwood and Shepherd, 1992) is unfortunately rather too ambitious. From a social constructivist perspective, psychological conceptualisations of illness and therapies like CBT are inherently reductionist themselves and can be regarded as powerful modes of social control just as much as biomedical approaches (Harper, 1992). For example, the goal of many psychosocial programmes is explicitly to enhance medication compliance (Kuipers et al, 1997). Regardless of whether this might be good in terms of outcome, such goals say something about the philosophical paradigm within which much of the psychological research is located. As Smail has argued, taking other philosophical perspectives into account does not mean psychological approaches are redundant or unethical, but given the powerful political issues involved in an analysis of schizophrenia, psychologists need to acknowledge these debates in their work.

In the current climate of evidence-based medicine (Geddes and Harrison, 1997), clinical psychologists are well-placed by virtue of their research background to make a significant contribution to the development of better treatments for schizophrenia through well-designed replicable studies. The shift towards psychosocial accounts of schizophrenia is a welcome response to the challenge of improving the quality of life for people with the disorder. The empirical evidence for the efficacy of CBT is gradually accumulating, although case-controlled studies are still few. Early intervention and CBT is so far unsupported by firm empirical evidence and this is acknowledged even by those researchers most involved in this area (Birchwood and Shepherd, 1992; Birchwood et al, 1998). However, the researchers who advise caution in drawing conclusions about CBT and early intervention are some of those who are most active in developing training courses and producing practical guides for clinicians (for example, Tarrier and Birchwood, 1994). The concern is whether enough is known not only about the efficacy of CBT and schizophrenia, but also about what skills and training would be required to practice such techniques. Issues of professional competency are touched upon in the literature (Birchwood, 1992, Sharp, 1997) with the assumption being that clinical psychologists are best placed to teach and implement these treatments. However, the eagerness with which these approaches are being applied clinically (for example group work as described in the professional publication Forum by Mills and Whiting, 1997) appears to be premature in the light of the empirical status of the research. Clinical psychologists are understandably keen to contribute to what has historically been a medically-led domain, but ultimately new techniques must be based on what is shown to help the patient. It is not just a question of more case-controlled studies, but it is argued that clinical psychology research will be all the more valuable to patients if it takes account of the social, political and ethical dynamics which inevitably play a part in schizophrenia.

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PROFESSIONAL DOSSIER

Professional practice, training and development

Professional practice since qualification

1991 - 1992

Psychologist, Waikato Hospital, New Zealand

- assessment and treatment of adult inpatients and outpatients, including general mental health, eating disorders, neurological assessment.
- assessment and treatment of child and family outpatients including behavioural problems, family dysfunction, learning difficulties.
- group work (self-esteem/ assertiveness).
- staff training.
- liaison psychologist for multi-disciplinary ward team.

1992 - 1994

Lecturer in clinical psychology, Institute of Psychiatry, London

- provision of a clinical service to two child development centres.
- assessment and treatment of children with developmental disorders, chronic illness and other special needs.
- clinical psychology representative on multi- disciplinary child development team.
- consultation service to child health professionals.
- liaison psychologist for special needs nursery groups.
- supervision and training of trainees on the M.Sc. Clinical Psychology course.
- teaching on M.Sc. Clinical Psychology course.

1994 -

Clinical Psychologist, Surrey Oaklands NHS Trust, Surrey

- provision and development of clinical psychology service to Ewell Community Health Team including assessment, treatment, consultancy, teaching.
- development and provision of a family therapy clinic (adult mental health).
- supervision of trainees and counsellors and doctors
- clinical audit and quality assurance

Training since qualification

Professional Issues

The Psychologist's role in influencing organisations (NZ Psychological Society, 1991)
Ethics in the 1990's (NZ Psychological Society, 1991)
Supervision Issues (Waikato University, 1992)
Supervision Training (Institute of Psychiatry, 1992)
Supervision Training (University of Surrey, 1995)
The NHS and Community Care Act (Institute of Psychiatry, 1993)
Child Clinical Psychology in the marketplace (SIG Children and Young People, 1992)
Consultation: a course for psychologists (Tavistock Clinic, 1993)
Outcomes in Professional Practice (DCP-BPS, 1996)
Accountability in professional practice (CPCPD-BPS, 1997)

Clinical Issues

Predicting Dangerousness (Professor Buckmaster NZ, 1992)
Post-natal depression (Professor Kendall NZ, 1992)
Eating Disorders conference (Institute of Psychiatry, 1993)
National Family Therapy conference (NZ, 1992)

Three Workshops with David Epston (NZ, 1992)
The Family Interpreted (Deborah Luepnitz, 1992)
Family Therapy within CMHTs (Institute of Family Therapy, 1995)
Brief therapy for survivors of abuse (Brief Therapy Practice, 1994)
Adult illness and childhood sexual abuse (Surrey Oaklands Trust, 1994)
CBT with psychotic symptoms (Hazel Nelson, 1996)
CBT with Schizophrenia (Prof. Tarrier, 1998)
Introduction to psychodynamic thinking (Surrey Oaklands Trust, 1996)
Treatment of Post Traumatic Stress Disorder (Europ. Soc. for Traumatic Stress, 1998)
BABCP Annual Conference (Canterbury, 1997)
Family Research and Family Therapy (Institute of Psychiatry, 1998)

Service developments

1995 - Development of a Family Therapy Clinic for adult mental health.
1996 - Clinical Psychology input to a strategy group which set up a nurse training course in sexuality and mental health.
1996 - New service offering supervision of counsellors in primary care.
1997 - CBT supervision and training to all junior doctors in Surrey Oaklands Trust.
1997 - New service offering consultation to Health Visitors on adult mental health.
1998 - Clinical psychology input to Steering Group on documentation and multi-user file policy.

Reflections on professional development since qualification

In the first years following my training I worked with a range of different services and client groups, which allowed me to build up my basic clinical skills and develop my ideas about what interested me professionally. My experience with child services gave me a good grounding in family therapy approaches and introduced me to thinking systemically about individuals and organisations as well as families. This field also provided me with valuable experience in multi-agency liaison, as much of the child and family work involved careful negotiation with social services and educational psychology departments. This fired my enthusiasm for working within a multi-disciplinary framework. At the same time I had the professional and academic support from being part of the Institute of Psychiatry which allowed me to maintain a strong identity as a clinical psychologist. This issue of balancing of professional status with team membership has permeated my work ever since. In my current post I am a member of a community mental health team, but I am managed by a professional head of psychology services. My current post reflects the way in which my interests and skills have developed. I have specialised in adult mental health and enjoy a balance of community-based work covering the full range of mental health problems, together with the opportunity to develop some areas of specialist interest and experience. My development has followed three main themes.

Firstly I have maintained and developed skills in cognitive-behavioural therapies (CBT). The work by such psychologists as Paul Salkovskis and David Clarke on refining cognitive theory in anxiety disorders has informed my clinical practice. I am especially interested in treating obsessive-compulsive disorder, and such cases are referred on to me by my team colleagues. I have also had some training in CBT with psychosis and hope to develop this approach at a local level. In contrast to developing these specific cognitive approaches, I have also had much professional satisfaction through learning to integrate CBT work with other models. Through my experience and training in psychodynamic perspectives I have enhanced my ability to work with very troubled clients, such as those with personality disorders or survivors of abuse, particularly in terms of engaging and containing clients whilst still addressing cognitive issues. My training in brief systemic therapy has also been a valuable addition to my repertoire of skills. This approach has many similarities to CBT and provides a valuable resource of creative ideas for engaging and motivating clients.

A further area of interest is in the field of organisational issues, consultation and training. The training course at the Tavistock Clinic provided an excellent focus for developing these skills and I have had many opportunities to apply them in my work. I offer training and consultation to a variety of professional groups. My contributions to clinical audit and a policy group also allow me to apply this knowledge at a broader organisational level.

Finally my training and experience in family therapy has allowed me to develop a new specialist service in adult mental health. However, I also use this knowledge in my individual practice since I find it clinically very helpful to work with clients in the context of their family life. My consultation service to health visitors reflects this integration of adult mental health with a perspective on the importance of family factors, for many of our adult clients are of course parents too.

Professionally I hope to steer my clinical career in a number of directions which follow on from the interests and skills I have already developed. Firstly, I will continue to build upon my clinical skills in CBT work and hope to offer more specialised services in the areas of psychosis and obsessive-compulsive disorder. I am also planning with a colleague to develop a trauma service within the department as currently no such specialism exists locally. An important part of these developments will be the opportunity to carry out research into these specialist areas. Secondly, I hope to gain more organisational and management experience, and would want to complement this with appropriate training. Lastly, I will continue to co-run the family therapy clinic, and again see further formal training and research as key elements in my professional development in this field.

Developing a family therapy service in adult mental health: a review and preliminary audit of the first two years

Introduction

In common with many adult mental health services, family therapy was not available within the Adult Mental Health directorate of my Trust when I came into my post in 1994. Part of my role in my current post was to devote some of my sessions to new service developments for the clinical psychology service. With this in mind I explored the possibility of setting up family therapy services for adults. The head of the adult psychotherapy unit in the Trust also considered family therapy to be an important treatment option for adults, and was keen to work collaboratively with me to develop a service. Since we both had some expertise in family therapy approaches, a proposal was made to set up and evaluate a new family therapy clinic for adults within our Trust.

Many psychologists acknowledge that family factors play an important role in adult mental health problems. This view is borne out by the multitude of ways in which families, and particularly spouses, have been involved in the psychological treatment of adults with problems such as depression, obsessive-compulsive disorder and alcohol abuse (Baucom et al., 1998). The substantial research into the concept of 'expressed emotion' as applied to schizophrenia (Kuipers et al., 1992), but more recently applied to several other disorders such as depression and eating disorders, has also contributed to the increased awareness of family factors in adult mental health.

Baucom et al's (1998) review of empirically supported family interventions covers a wide range of adult mental heath treatments. However, most of these, including the expressed emotion work, are not based on *systemic* models of family therapy, but fall more in the categories of behavioural therapy or psychoeducation. Systemic family therapy is now a well-established approach in the field of child and adolescent psychiatry, and is even the treatment of choice with some disorders in these client groups, for example anorexia nervosa (Eisler et al., 1997). However, family therapy and the systemic approach in general has had little impact on general adult practice (Cottrell, 1989), and only a handful of reports of such work appear in the psychiatric or psychological literature. There are no case-controlled studies to support the general use of (systemic) family therapy with adults, indeed research in this area has tended to remain outside the mainstream scientific field.

Bloch et al. (1991) describes an audit of 50 families seen in an adult family therapy clinic using a Milan style of therapy, and concludes that they achieved satisfactory results using this approach. MacDonald (1994), who advocates using a brief therapy model, also found that a 'good' outcome was achieved in 70% of families treated in an adult psychiatry unit. However, these studies were not empirically robust and the authors acknowledge that information is sorely lacking on issues such as; the types of patients and presenting problems which might be most suitable for this approach, ways of conceptualising psychiatric problems in family-systemic terms, and the outcome of treatment.

The studies by Bloch et al. and MacDonald have yielded promising results, and suggest that family therapy for adults may be an effective complement to current adult mental health treatments. Thus, it was considered worthwhile to set up and evaluate such a service in our Trust. A 'needs assessment' was not carried out in the form of a survey or questionnaire, but through our discussions with colleagues throughout the adult mental health services, it was clear that many professionals felt that family factors played an important role in their patients' difficulties, and they welcomed the idea of a family approach. This support, together with our knowledge of the literature and our family therapy expertise, allowed us confidence in our view that a family service could benefit patients in our Trust. The aim of this review is to give an account of the development of the new service, and to report the results of a small preliminary audit of the first two years.

Development of the Family Therapy Clinic

Context

Planning a new service required careful consideration of the background factors and culture which prevailed in the Trust. The psychotherapist and I recognised the importance of consultation with colleagues, and considered the potential for tension and anxiety in the Trust arising from the prospect of relative newcomers implementing change in a complex organisation. No other professionals were offering adult family therapy although several, particularly learning disabilities clinical psychologists, had received some training. However, we had to work very sensitively with the Child and Adolescent services to ameliorate their initial concerns about our competence to move into what is traditionally a child and adolescent domain. Overall, most professionals and managers were supportive of our plans.

It was also important to recognise the potential for tension over ‘ownership’ of the clinic. Whilst I jointly developed and ‘owned’ the service with my colleague, it has been organisationally located in the psychotherapy unit, and is therefore seen by many staff as the consultant psychotherapist’s clinic. However, the administrative and financial support of the psychotherapy unit, and the ‘protection’ afforded by the presence of a medical consultant psychotherapist in negotiations was vital in the initial development of the clinic. The input of clinical psychology into the service is still considerable in terms of time, expertise and service developments.

Therapists’ background

I have long had an interest in family therapy, having worked in Child and Adolescent services for some years where systemic work was the ‘bread and butter’ of clinical treatments, and techniques such as using one way screens, reflecting teams and systemic consultation were standard practice. My training was largely experiential with some excellent supervision over the years by highly regarded practitioners. This was supplemented by workshops and conferences, with some particularly influential training from the originators of narrative therapy, Michael White and David Epston, whilst I worked in New Zealand. Narrative therapies are especially relevant to working with adults, both in a family context and individually, and thus I was exposed to the possibility of working systemically within my chosen field of adult mental health when I took up my latest post in a Surrey NHS Trust.

With a strong psychoanalytic background in group therapy as well as experience of structural systemic models, the psychotherapist brought very different strengths to the service. These contrasted significantly with my approach which incorporated a synthesis of brief and narrative models together with a clinical psychologist's cognitive-behavioural background. The psychotherapist and I spent some sessions discussing experiences, training and preferred frameworks, to ensure we had some shared values and conceptualisations of how we would work together.

Development Phase 1

The initial consultation and planning process took several months. Meetings with managers and professionals to establish support for the new service took much of this time. It was also crucial to discuss and clarify with the psychotherapist how we conceived the service, how decisions would be made, and our views on conducting family therapy. Much work was done on establishing our identity and the characteristics of our service. Considerable effort went into the production of information leaflets about the new service, as we wanted to be as clear as possible with referrers and clients about what we could and could not offer. We specified, for example, that where a child or young person in the family was having significant emotional or psychological difficulties, the family would be more appropriately seen by Child and Adolescent services. Because we had limited time and resources we decided, at least initially, to accept referrals only from professionals. Our overall aim was to provide family therapy input to families where one or more of the adult members were identified as having mental health difficulties, and where the referrer and family felt a family approach was relevant to the presenting problems.

Development Phase 2

We began accepting referrals in September 1995. Initially the psychotherapist and I saw families together in a hospital-based consulting room with very limited facilities. The clinic operated one morning a week only. Psychiatric colleagues provided the first referrals, which often consisted of very complex cases where many other approaches had already been tried and failed. It felt as if we were being 'tested' and we handled these cases as sensitively as possible, wishing to offer help but be clear about what was realistic to expect. Fortunately, my links with professionals through my other role as a community clinical psychologist meant I could liaise with my colleagues informally as well as formally, to encourage good relations within the Trust. We were acutely aware of the responsibility we had to work only within our competence and with appropriate support. To this end we arranged monthly supervision from a regional adolescent family therapy team which has proved invaluable both for the quality of peer supervision, and for the connections made with an important neighbouring service.

Development Phase 3

Some months into the service we felt confident enough to develop the consulting facilities, and a video link and camera were installed between a clinical psychology office and the psychotherapist's adjacent room. This required the support of the clinical psychology management, and was in some ways symbolic of the firm link now established between the two departments in the running of this service. We also wished to expand the personal resources of our small team, and so recruited a family therapist with high level training to join the clinic on a permanent basis. This post was funded from the psychotherapy budget. This arrival of course shifted the dynamics of the relationship between the clinic's two 'founding members', and we spent some time as a team exploring this to ensure we worked well together. The arrival of trainees and registrars on placement further expanded our numbers.

After a year of operation the three team members held an annual review of the service; an opportunity to reflect on how the clinic was running but also to talk about how we related both within the team and to external agencies. A steady flow of referrals had necessitated a waiting list. This was addressed by a review of the communications process with referrers and families, improvements to an opt-in questionnaire for all families, and better use of clinic time to enable reduced waiting times. We also increased the emphasis on liaison with referrers and keyworkers of the index patient referred. My work in community-based clinical psychology had given me experience of multi-disciplinary working within policy frameworks such as the Care Programme Approach. I was therefore in a good position to feed this into the family therapy team discussion and encourage clear lines of clinical responsibility in our work with families. This was vital given the complex nature of the psychological problems which many of the referrals presented and the multitude of agencies involved. In all, the review at this stage suggested that the service appeared to be highly valued by our psychiatric colleagues, but limitations on resources and session time has meant that even today the service continues to operate on only one morning a week. Hopes for community-based premises have not been realised, although the current out-patient setting does allow for helpfully close links with consultant medical colleagues whose support has been crucial. A symbol that we had perhaps been accepted by our peers at this point in our development was an invitation to present our work at a regional conference on eating disorders, a presentation which was well received.

Models of working

Throughout the short history of the family therapy clinic a recurring theme has been how the team members integrate their different training and models of therapy into a coherent, workable approach in practice. As commented on earlier, family therapy in adult mental health is very much in its infancy and lacks empirical status. Indeed family therapy itself is in reality a host of different theories and models, some quite contradictory of each other. Given this background it seemed particularly important as a clinical psychologist to be clear about the models being used, and to acknowledge the many assumptions that have to be made in the face of relatively little direct research evidence. This would challenge the team to justify formulations and interventions used and to consider ways of assessing outcome as objectively as possible. The audit described below and outcome surveys planned for the future are part of this process of evaluation.

Most family therapy teams in clinical practice are multi-disciplinary and so our mix of disciplines is not unusual. Family therapy, by its very nature of looking at systems and communication patterns, lends itself to an analysis of how the members of a team relate to each other. In our team we have debated issues brought about by the status held by the different professions, the kudos acquired by formal as opposed to informal training, and the influence of gender and culture on team relationships (I am the only female on the team and my two colleagues are white males, but we have both male and female visiting trainees who have been from a variety of different cultures). At times the different perspectives of team members are in stark contrast, for example I might take a behavioural or perhaps a brief therapy approach to a session whereas my psychotherapy colleague might highlight the emotional tone of the interactions and interpret analytically. The structure of a family therapy session with the typical break for team discussion allows us to think about these contrasts and be guided by what seems best to fit with the family being seen, rather than by our own ideologies or professional interests. All the members of the team receive some form of external supervision which is crucial for ensuring professional standards of clinical practice.

Whilst the training and experience of the team members is quite varied, there are several themes and conceptualisations which are of high heuristic value and occur a lot in our formulations, aside from the general systems principles which we obviously all share. Hayley's (1980) ideas about the 'family life cycle' and transitions in family life are often pertinent, for example when working with adults with eating problems. The concepts of family scripts and 'problem-saturated' narratives (White and Epston 1989) have also proved important in our thinking about the nature of pathology and psychiatric problems. Attachment theory has also helped us conceptualise family conflicts and distress, especially the notion of a secure family base described by Byng-

Hall (1995). These and other theoretical models or ideas are debated through team literature reviews and discussion, as part of a team commitment to continuing professional development.

A preliminary audit of the family therapy clinic

Psychology has an important role to play in clinical effectiveness (Baker and Firth-Cozens, 1998), and as a clinical psychologist, I was able to bring to the team an emphasis on issues of service evaluation, outcome assessment and the trend towards evidence-based practice in health services. Audit is a small but important part of this process of evaluation and I undertook to design and carry out an evaluation of the service. Initially, due to severe time constraints, it was agreed that the audit should address basic questions such as who refers what kind of problems and what kind of service do we then offer. To this end I reviewed the notes of all the families referred up to December 1997 and collated data on a number of different variables. The results are presented below.

Method

A retrospective review was conducted of the casenotes of all the families referred to the clinic from its inception in September 1995 to December 1997. Using a structured protocol, the following information was recorded.

(a) Source of referral
(b) Reason for referral, classified into three main themes, family relationship issues, marital problems, family bereavement. These categories follow convention in other audits in the literature, and captured the main themes of interest to our team.
(c) Diagnosis of index patient. This was derived from the referral letter.
(d) Care Programme Approach (CPA) level of index patient, as indicated in the referral letter or from medical notes. The CPA system applies to every patient in the psychiatric

service and ranges from 1 - non-complex cases requiring one or two professionals' input, to 3 - complex cases needing multi-agency involvement.

(e) The course of therapy, including waiting time, number of times seen and reason for termination.

Outcome data was not included in this particular audit. Satisfactory measures of outcome have not yet been developed for family therapy. This issue is discussed further in the Discussion section.

Results

From September 1995 to December 1997, 69 families were referred to the clinic for assessment. The referring agencies are shown in Table 1.

Table 1. Referral source

Professional	n	%
General Psychiatrist	39	57
Community Psychiatric Nurse	12	18
Social Worker	7	10
Clinical Psychologist	6	9
GP	3	4
Psychotherapist	1	1
Occupational Therapist	1	1

The majority of referrals, 35 cases (51%), requested assessment for marital difficulties, 31(45%) were for various family communication and relationship difficulties, and the remaining 3 referrals (4%) specifically related to bereavement issues.

Diagnosis of Index Patient

In 63 out of the 69 referrals (91%) an index patient with a mental health diagnosis was identified by the referrer. 21(33%) were male and 42(67%) were female. The diagnoses for those families where an index patient was identified are presented in Table 2.

Table 2. Diagnosis of index patient

Diagnosis	n	%
Depression	28	45
Anxiety disorders	9	14
Psychosis	7	11
Alcohol abuse	5	8
Eating disorder	4	6
Grief reaction	3	5
Bipolar disorder	2	3
Personality disorder	2	3
Other	3	5

The CPA levels for index patients were as follows. Level 1 - 24(38%) cases, Level 2 - 16(25%) cases, Level 3 - 3(5%) cases. In 20(32%) cases no CPA level was identified.

The course of therapy

Of the 69 families referred, 9(13%) were not accepted by the family therapy team. In most of these cases child protection concerns disqualified them from our service and they were referred elsewhere. Of the 60 families accepted for assessment (87% of referrals), 30(50%) declined an appointment, mostly by not responding to the questionnaire or follow-up letter.

Waiting times for an assessment appointment varied considerably over the audit period, from one to 20 weeks, with the average wait being 6 weeks.

Seven (23%) of the 30 families assessed were seen only once and not taken on for therapy. In 2 cases this was based on a professional decision. In 4 cases the decision was by mutual consent, and in one case on the family's initiative.

Twenty-three families were offered therapy (77% of families assessed). The diagnosis of the index patient for these families are shown in Table 3.

Table 3. Diagnosis of index patient in families offered therapy

Diagnosis	n	%
Depression	15	66
Anorexia nervosa	2	9
Anxiety disorder	2	9
Bipolar disorder	1	4
Psychosis	1	4
Personality disorder	1	4
Psychosexual disorder	1	4

Of the 23 families offered therapy, 6 were still being seen when the audit took place. For those who had been discharged, the average number of sessions given was 6, with a range of 2 to 12 sessions. Overall, 15% of sessions were cancelled and 4% were unattended with no notice given.

For the 17 families who were discharged within the audit period, 12 completed therapy and were discharged by mutual consent, 4 terminated on the family's initiative and one family was discharged by the therapist. Therapy was judged to have been terminated by mutual consent when both the family and the therapist felt the therapy had been beneficial and had reached a satisfactory conclusion. However, no objective measures of outcome were available to support this clinical judgement.

Discussion

The principle aim of this audit was to provide the family therapy team with an overview of the type of service we were offering, so that we could identify strengths and weaknesses, and opportunities for improvement. With this in mind, some key points which emerge out of the results will be presented below, followed by a consideration of the need for quality standards and further audit.

The majority of referrals came from psychiatrists, which is not surprising given that the clinic is located in an out-patient psychiatric department, and that the psychotherapist on the team liaises closely with his consultant colleagues. The very few referrals from the primary care sector are to be expected as we have not heavily publicised our service outside our own specialist Trust. While we would wish to ensure that the service is easily accessible to primary care professionals, time and human resources constraints limit our ability to respond to an increase in referrals from any source. However, we are cognisant of the fact that GPs are key purchasers of mental health services, and the new primary care groups will no doubt have an impact on future service provision. For example, local GPs may not be familiar with family therapy and its application to adult services, regarding it as only applicable to children, or as an expensive non-essential service which should not be provided locally. Good communication with all our purchasers, especially GPs, is vital to ensure that we inform them of the nature of the service, our audit results and the published evidence for the effectiveness of family therapy with adults.

The range of reasons for referral reflected the generic nature of a family therapy service, but with an emphasis on requests for couples or marital therapy, often in the context of a relationship where one partner had a long-term serious mental health problem. Such referrals are appropriate given the emerging research evidence supporting the use of systemic work with couples such as that conducted by Julian Leff and colleagues at the Institute of Psychiatry which showed good results in the treatment of depression (Leff, 1998).

The diagnosis of the index patients covered a range of serious problems though depression predominated, particularly in the couples referrals. Seven patients with a psychotic illness had been referred which may reflect the increasing awareness of family factors in psychotic illnesses such as psychosis. The significant proportion of patients with a serious mental illness was also reflected in the number of cases at CPA levels 2 and 3 which amounted to 30% of the index patients. This has significant implications for the service as such complex cases are more time-consuming and demanding on the therapist, and involve liaison with several professionals and agencies.

The audit data on various aspects of the course of therapy raise several issues, not least the high attrition rate of referrals pre-assessment. It is obviously not possible from this audit to deduce what caused 50% of families to decline to be seen. It may be that the families, and for that matter referrers, were not fully informed about the service or clear about the rationale for family therapy. Other family members may not have been fully consulted. Further enquiry is clearly needed to address this issue.

An average waiting time of six weeks for an assessment is relatively short compared to many psychotherapy waiting lists, and is within the Trust's quality standard of a maximum of eight weeks for an out-patient appointment. However, at times the wait was much longer, and the team were concerned that waiting for several weeks is stressful for families in need of help. Limited resources and a relatively small team mean that it is difficult to absorb fluctuations in referral rates to the clinic, and so this problem is unlikely to be resolved in the short term.

Families attended an average of six sessions which is in line with usual family therapy practice, but slightly longer than the audit reported by Bloch et al (1991) whose average was four sessions. This may reflect the higher proportion of serious mental illness seen in our clinic. The cancellation and non-attendance rates are reasonable for a psychotherapy service (MacDonald, 1994).

The majority of families seen completed their therapy to the satisfaction of both the therapist and family. The actual outcome of therapy and the reasons why some families terminated prematurely are important issues to address in future audits.

In general, this audit provided some useful preliminary information and pointed to some issues which need attention, such as attrition rates and premature termination. Audit can be seen as a systemic approach in itself, consisting of a feedback with questions posed, information received and changes made in practice (Bruggen and Pettle, 1993). The information here is currently being discussed by the team with a view to implementing improvements, followed by further auditing to establish whether clinical practice has been enhanced by these changes. The following issues are being considered.

a) Information given to referrers and families about our service.
b) Quality of liaison with other professionals and agencies
c) Waiting times to assessment
d) Database to allow systematic collection and monitoring of audit data
e) Audit of therapy process and outcome to complement basic audit topics.

In relation to the last point, two types of questionnaire have been devised, one for referrers and another for families. These will assess satisfaction with the service and views on outcome. Such satisfaction surveys are commonly used in clinical practice, and have the advantage of explicitly involving users in a service, a requirement of good practice in the NHS (DoH, 1991). However, the conclusions that can be drawn from such self-report surveys are limited, for example in how well they correlate with other measures of outcome. It is inherently difficult to audit outcome of family work where the goals and experience of the therapy may be different for each family menber, and where it is problematic to operationalise theoretical (systemic) objectives. Outcome in family therapy is the source of much recent debate (MacDonald, 1994). However, the team recognises that ethical, evidence-based practice demands assessment of outcome. With this in mind, the team need to consider how we might begin to operationalise what we do and what we aim to achieve, so that we can more objectively judge the success or otherwise of family therapy interventions. The audit presented here has provided a focus for developing these ideas further, so initiating a process of evaluation which will help our practice and allow us to be more accountable to those who use our service.

The Family Therapy Clinic: Conclusions and Future directions

The family therapy clinic appears to be well utilised and received by referrers and families alike, and initial evaluations suggest that the service is potentially a worthwhile complement to standard adult services. Many of the index patients continued to be seen by their psychiatric team within the Trust after finishing family therapy, which may of course have influenced the feedback given, and thus we need to be cautious about this 'approval'. The clinic is not without its problems of course. There is the continual pressure of limited resources and time, but perhaps more fundamental is the inevitable tension for the organisation as a whole which accompanies the assimilation of systemic ideas into traditional psychiatric and psychological practice. Systemic theory and family therapy models operate within a different paradigm to the medical model of mental illness. For example, there is no simple cause and effect approach to pathology, and disturbance is seen as located within the family rather than an individual. Moreover, interventions such as narrative therapies and brief therapies (based on social constructivist principles) work with families to *challenge* the orthodox stories of pathology which predominate in psychiatry, and so may be seen by other professionals as subverting the framework of psychiatric care within which the clinic is located. Family therapists such as Haley (1975) and more recently Lieberman (1995) have acknowledged the difficulty of offering family therapy in general psychiatry, and point out that one cannot just 'add on' this approach or keep the clinic separate from other parts of the service. The institution itself has to be encouraged to 'think' systemically through good liaison at every level of the service including managers. Training is a vital part of this process too, and our inclusion of keyworkers, trainees and junior doctors in the clinic contributes to better understanding of systemic approaches. While at an epistemological level, family therapy approaches might be in opposition to medical models, in practice our team has found that we *can* work within the organisation and its psychiatric structures such as the CPA and Mental Health Act procedures. By being clear with each other, the referrers and the family about our approach, boundaries and responsibilities, we have been able to establish a workable collaboration with our colleagues.

Whilst the debate about different paradigms within psychiatry and psychology will inevitably continue, there has been a shift to an empirical emphasis in family therapy in recent years, particularly as highly respected psychiatrists and psychologists within traditional academic institutions have started to research and publish in this field. A recent conference at the Institute of Psychiatry in 1998 entitled 'Family Research and Family Therapy' highlighted the increased interest and respectability that family therapy now enjoys. In the current climate of evidence-based practice and clinical effectiveness, this more 'scientific' profile can only help family therapy make inroads into adult mental health. However, the empirical emphasis may also obscure or over-simplify the reality of doing systemic work in general psychiatry with the inherent tensions outlined above. As a clinical psychologist I have had to struggle with these tensions at an individual level, in terms of reconciling my empiricist training with the challenges of different philosophies of science which underpin systemic ideas. This informs not only my family therapy practice but my adult mental health work too.

The family therapy clinic is now an accepted part of the psychotherapy unit, and in future we would hope to be able to expand the service to allow more time to see families but also to offer more consultation and training. There are opportunities to offer additional specialist services if increased resources were available. For example, the psychotherapist is involved in the strategic planning of a new adult eating disorders service in the Trust. Family therapy is considered an important part of this. The increased attention paid to family factors in schizophrenia has raised the issue of whether our clinic should offer family management as developed by Kuipers, Leff and colleagues (Kuipers et al., 1992). This approach, which reduces relapse rates in sufferers of schizophrenia, is supported by several research studies. However it would require a substantial increase in resources and a great deal of collaboration with community mental health workers to apply this to local services. The approach is not without its critics. Some have questioned the ethics of an intervention which emphasises that schizophrenia is biological illness and appears to promote a view of mental distress which is stigmatising (Johnstone, 1993). Others have pointed out the inherent contradictions between family management (which offers psychoeducation and behavioural interventions) and systemic family therapy (Burbach, 1996). These are issues which our team must continue to discuss and resolve in terms of how we develop our clinic for the future.

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RESEARCH DOSSIER

Perfectionism, guilt and responsibility in obsessive-compulsive disorder: an investigation of cognitive and emotional styles

Abstract

Recent cognitive models of obsessive-compulsive disorder (OCD) propose that there are specific thinking and appraisal biases evident in sufferers of the disorder. The cognitive traits of perfectionism, and responsibility have both been linked to OCD, while guilt, an affective trait, has also been hypothesised to play a key role in the condition. This study measured perfectionism, responsibility and guilt in 21 patients with OCD compared to 26 patients with other anxiety disorders. It was hypothesised that OCD patients would show significantly higher levels of these traits. However this hypothesis was not wholly supported by the results. OCD patients did not differ from anxiety disorder patients in terms of the three traits described. Correlational analyses across the two groups suggested that only perfectionism was related to obsessionality when the effects of anxiety and depression were partialled out. The current status of cognitive theories of OCD are discussed in the light of these results.

Introduction

The nature of obsessive-compulsive disorder

DSM-IV describes the essential features of obsessive-compulsive disorder (OCD) as recurrent obsessions or compulsions which are severe enough to be very time-consuming or cause significant distress or impairment. Obsessions are defined as ‘persistent ideas, thoughts, impulses, or images that are experienced as intrusive and inappropriate and that cause marked anxiety or distress’, whilst compulsions are defined as ‘repetitive behaviours...or mental acts...the goal of which is to prevent or reduce anxiety or distress' (APA, 1994). Clinical accounts of OCD have appeared in the literature for well over a hundred years but until relatively recently the condition was considered to be an extremely rare illness with a poor prognosis. Epidemiological studies in the last few years have indicated that the prevalence of OCD is approximately 1% to 2% (Rasmussen and Eisen, 1992; Bebbington, 1998). Thus OCD is about as twice as common as schizophrenia though less prevalent than depressive disorders.

OCD is classified in DSM-IV as a form of anxiety disorder. Thus, OCD is assumed to share common attributes with the other anxiety disorders such as panic disorder and generalised anxiety disorder, whilst still demonstrating discriminant validity as a clinical category in its own right. Some researchers have challenged this system of classification. Enright and Beech (1990) and Enright (1996) have argued that OCD is misclassified as an anxiety disorder and is more closely aligned to the schizophrenia spectrum of disorders. More recently, Hollander controversially argues that OCD is one of a range of related conditions belonging to the 'obsessive-compulsive spectrum disorders' which includes such diagnostic subgroups as hypochondriasis but also non-anxiety disorders such as impulse control disorders, autism, tic disorders, eating disorders and dissociative disorders (Hollander, 1998). Others have pointed to the extensive overlap found in the mood disorders and anxiety disorders and propose a more general challenge to the categorical classification within anxiety (Brown, 1996). OCD for example shows a marked overlap in symptomatology with depression (Rasmussen and Eisen, 1992), and thus the notion of a separate entity called OCD properly located within the anxiety disorders is called into question. Problems with diagnostic classification have led researchers, particularly in the cognitive-behavioural field, to argue for *dimensional* approaches to classification and this is especially pertinent for OCD since much of the cognitive-behavioural research into this disorder has looked at dimensions of obsessional-compulsive behaviours across different populations rather than the clinical condition per se. There is a great deal of evidence that obsessive-compulsive symptoms lie on a continuum in the general population, rather than OCD being a qualitatively different experience (Gibbs, 1996). This way of conceptualising the symptomatology of OCD underlies much of the psychological research into OCD, such that analogue studies using student and other non-clinical samples are frequently used and then generalised to a clinical population. This issue is important in reviewing theory and research in this field, and will be returned to again later. The point being made here is that, even for such a distinctive illness as OCD, much controversy still exists regarding its nature, nosology and its relationship to other disorders.

Theories of etiology and maintenance in obsessive-compulsive disorder

Some of the first hypotheses about the origins of OCD were psychodynamic theories in which obsessions were regarded as defensive psychological responses to unconscious impulses (for example, see Jenicke's [1986] summary of the Freudian position on OCD). However, these theories suffer the criticism levelled at psychodynamic theories generally, that is, they cannot be falsified and therefore are not open to empirical evaluation. In addition, there is no evidence that psychodynamic treatments work with sufferers of OCD (Jenicke, 1986).

Many different biological theories have been proposed which postulate some sort of neurological deficit model based on neuroimaging studies, neuropsychological studies of cognitive deficits and the effectiveness of serotonergic drugs in the treatment of OCD (Pigott et al., 1996; Tallis, 1997; Saxena et al., 1998). However, there is little consensus about the nature of the neurological deficit or which part of the brain is functioning abnormally. Many of the distinctive symptoms and responses to psychological treatments in OCD cannot be accounted for in neurobiological models, and neuroanatomical abnormalities are found in only a proportion of the samples studied. Normal controls or control samples with other psychopathology have rarely been used (Saxena et al., 1998). As Salkovskis (1996) and others have pointed out, *all* behaviours have biological substrates and to this extent will have neurophysiological correlates, so the apparent division between biological versus psychological theories of etiology are misleading. Indeed recent research is moving towards a synthesis of biological and behavioural perspectives and is beginning to address the reciprocal relationship between brain and behaviour in OCD, for example looking at the way psychological therapies induce changes in cerebral function (Schwartz, 1998). A further biological perspective on OCD, that of genetic factors has been researched for some years. Genetic studies have on the whole been inconclusive. Pauls et al. (1995) concluded from their study of over a hundred probands with OCD and their relatives, that the disorder is a heterogeneous condition with some cases being familial, but in other cases there was no evidence of a familial relationship.

Behavioural models have been the most influential in terms of theorising about the development of OCD, particularly Mowrer's two-stage theory of fear and avoidance (Mowrer, 1960). In the first stage a neutral stimulus acquires anxiety-evoking properties through classical conditioning, and in the second stage, behaviours which reduce the anxiety associated with the stimulus are reinforced. In OCD these behaviours might be both passive avoidance by withdrawing from the stimulus, or active avoidance of the anxiety through fear-reducing compulsions and rituals. Rachman and his colleagues (1980) conducted several experimental studies to examine this model in OCD, and found that elicitation of the obsession resulted in increased anxiety, rituals and compulsive behaviours rapidly reduced the levels of anxiety, and if the rituals were blocked or delayed then the initial increases in anxiety were followed by a slow decrease (spontaneous decay). These studies provided the foundation for behavioural treatments such as exposure and response prevention (see Rachman and Hodgson, 1980). Behavioural models thus assume that OCD is an anxiety disorder which, in common with other anxiety disorders such as phobias, has its origins in associative learning or conditioning through which obsessional thoughts or impulses have become associated with anxiety.

Behavioural models seem intuitively reasonable, and are well grounded in empirical research. The success of behavioural treatments has provided considerable support for behavioural models, though in themselves such studies do not of course directly test the etiological aspects of the theory. A recent review of behavioural treatments concluded that exposure and response prevention had a “robust effect” on OCD (Stanley and Turner, 1995, p. 169), and many other researchers have reached similar conclusions (e.g. Steketee, 1994; de Haan et al., 1997). However, many authors have noted limitations of behavioural models, and their status as an explanation of the etiology of OCD is open to criticism. A key drawback is that there is little evidence that critical traumatic episodes precede the onset of OCD. That is, people with OCD rarely recall a particular learning experience which could account for the association between a specific stimulus and conditioned anxiety. Tallis (1994) described two case summaries where there appeared to be key events in the patients’ past which triggered obsessional symptoms. However, a study by Jones and Menzies (1998), which assessed retrospectively the onset of OCD in a group of 23 OCD patients, found a low rate of associative learning. Only three of the patients could recall a specific event which precipitated their obsessional concerns.

Behavioural treatments, whilst greatly improving the prognosis for OCD sufferers, work only for a proportion of patients. Indeed, when drop-outs and treatment refusers are taken into account, only around 50% of patients could be characterised as treatment responders (Stanley and Turner, 1995), and many responders still suffer significant residual impairment in terms of social and occupational functioning. Some types of OCD such as obsessional ruminations, are particularly refractory to behavioural treatments (O’Kearney, 1993). Thus, in contrast to treatment outcome for other anxiety disorders such as specific phobias or panic disorder, behavioural methods in OCD have major limitations. The theoretical and treatment shortcomings summarised above have contributed to some dissatisfaction amongst researchers and clinicians with purely behavioural models. An increasing emphasis on the *cognitive* aspects of OCD has led to the development of cognitive-behavioural theories of both the etiology and the maintenance of obsessive-compulsive symptoms. These theories are explored in some detail in the next section.

Cognitive-behavioural models of obsessive-compulsive disorder

Most obsessive-compulsive behaviour is evoked by intrusive thoughts, images or impulses (Parkinson and Rachman, 1981). These are perceived by the sufferer as automatic, irrational, unpleasant and ego-dystonic. The key feature of cognitive-behavioural models is an emphasis on the *appraisals* which are triggered by these intrusive thoughts; that is, the faulty thinking patterns, beliefs and assumptions which are automatically activated and which serve to generate anxiety. Compulsive behaviours then develop in an effort to reduce anxiety and prevent further occurrences of the unpleasant intrusive thoughts. The assumption is that these appraisal biases are learned in early childhood, although there has been little research attention paid to the etiology of faulty cognitions in OCD. As Rachman (1997) points out, a complete answer to the question of where cognitive biases originate from would require "a better grasp on the very nature of human thinking." (p. 797).

There has been an enormous amount of research in the last few years into the phenomenology of obsessions and the particular patterns of faulty thinking that distinguishes OCD from other disorders. An important impetus to this research has been the now well-established finding that intrusive thoughts are not a pathological symptom in themselves but constitute "a universal human phenomenon" (Salkovskis, 1996). A comprehensive review by Gibbs (1996) of studies using non-clinical populations concluded that "normal" obsessions are similar in form and content to clinical obsessions, but the latter are more enduring, intense, discomforting, and difficult to dismiss. The finding that intrusive thoughts are normal has had two distinct influences on the research. Firstly, it has been used to justify the use of non-clinical populations in a large proportion of the studies, from which extrapolations are made about the nature of OCD (e.g. Freeston et al., 1992) and, perhaps because of this, there is a relative paucity of cognitive research using OCD patients. Secondly, the question of interest has shifted from why intrusive obsessional thoughts occur (since they are now regarded as a normal part of the stream of consciousness), to what causes these intrusive cognitions to become so distressing and debilitating in some individuals and not others. There is evidence from a number of studies that appraisal processes are closely associated with the frequency and controllability of intrusive thoughts, and that individuals with OCD use different appraisals and cognitive strategies in response to intrusions compared to normal controls. (Parkinson and Rachman, 1981, Purdon and Clark, 1994). Moreover, it appears that the personal meaning or significance of the intrusive thoughts is of crucial importance in obsessional problems and differentiates obsessional thoughts from other cognitions such as worry (Clark and Claybourn, 1997).

The burgeoning literature on cognitive processes in OCD reflects the wealth of interesting ideas and theories about which dysfunctional appraisals underlie the disorder. However, there is little consensus on what cognitive biases are the most important in the development of OCD. Numerous traits, processes and beliefs have been implicated in the disorder. In a major review of recent theories of OCD, the Obsessive Compulsive Cognitions Working Group (1997) summarised the belief domains they believed to be of most importance in development and maintenance of OCD. Six groupings of faulty beliefs and related appraisals were devised. These included overestimating the importance of thoughts, excessive concern about the need to control thoughts, overestimation of risk and severity of threat, intolerance for uncertainty, inflated responsibility for harmful events and lastly, perfectionism. Thus the range of cognitive biases purported to be crucial in OCD continues to grow, but the development of a cognitive theory of OCD which links all these factors in a logical fashion is noticeable by its absence. Some authors have developed theoretical models which emphasise particular cognitive biases and their relationship to obsessive-compulsive symptoms, and two of these will be described in more detail since they seem to offer coherent, though different, accounts of the possible cognitive foundations of OCD.

McFall and Wollersheim's model

McFall and Wollersheim (1979) were some of the earliest writers to offer a specific cognitive theory of OCD. Their model proposes that unrealistic threat appraisals are the key characteristic of OCD. They draw together a number of 'primary appraisals' which are hypothesised to increase the likelihood of making unrealistic subjective appraisals of danger following an obsessional thought, and thus generate anxiety. Examples of such primary appraisals or beliefs include (1) one must always be competent and adequate (perfect) to be worthwhile; (2) failure to live up to these expectations will result in punishment; (3) one is powerful enough to initiate or prevent the occurrence of disastrous outcomes; and (4) certain thoughts and feelings are unacceptable and could lead to a catastrophe. McFall and Wollersheim hypothesised that deficits in 'secondary appraisal processes' follow this first appraisal of threat, and are characterised by unreasonable beliefs about one's ability to cope with the threat. Such distorted appraisals include beliefs that if something is or may be dangerous, it will be upsetting; it is easier and more effective to carry out a magical ritual than it is to confront one's feelings or thoughts directly; and it is intolerable to have feelings of uncertainty and loss of control. Although obsessions and compulsions are themselves distressing, patients perceive these symptoms as more tolerable than the distress and guilt associated with the feared outcomes. As the authors describe it, "symptoms are *their best option* for reducing distress" (p. 336: italics in original).

McFall and Wollersheim describe cognitive approaches which would address these irrational primary and secondary appraisals, and particularly recommend Rational Emotive Therapy (RET). Ellis (1994), who created RET, has his own model of OCD which holds that people with OCD are born and reared with cognitive, emotive and behavioural deficits. Like McFall and Wollersheim, Ellis proposes that OCD patients have primary irrational beliefs relevant to the obsessional thoughts, such as a need for certainty and perfectionist beliefs, and that they then also construct further disturbances about their cognitive distortions such as " If I can't function better than I do function, I'm a *worthless person*." (p. 123; italics in original).

The theory proposed by McFall and Wollersheim has not been subjected to direct empirical testing, in that the constellation of cognitive distortions supposed to co-exist in OCD has not been specifically investigated. However, as outlined above, much pragmatic research has been carried out into cognitive traits and patterns which might relate to OCD, and some of this research offers indirect support for McFall and Wollersheim's ideas. Two particular traits, perfectionism and guilt, which play an important part in their theory and have been subject to some empirical study, will be explored in more detail.

Perfectionism

Perfectionism has long been associated with psychopathology. Pacht (1984) argues that perfectionist thinking underlies a wide variety of psychological disorders, and quotes Burns' (1980) definition of perfectionism which, although not specifically referring to obsessive-compulsive problems, does encapsulate an obsessional quality. Burns wrote that perfectionists are:

"those whose standards are high beyond reach or reason, people who strain compulsively and unremittingly toward impossible goals and who measure their own worth entirely in terms of productivity and accomplishment. For these people the drive to excel can only be self-defeating." (1980, p. 34).

The key features of perfectionism are a desire to achieve the highest standards of performance coupled with excessively critical evaluations of one's performance (Frost et al., 1990). However, recent studies have investigated the construct more precisely, and developed multidimensional definitions of perfectionism which have been empirically tested. Having thoroughly reviewed the literature on perfectionism, Frost et al. (1990) hypothesised that the trait is in fact comprised of six dimensions: (1) excessive concern over making mistakes, (2) high personal standards, (3) perceived high parental expectations, (4) perceived high parental criticism, (5) doubt about the quality of one's actions and (6) a preference for order and organisation. The multidimensional perfectionism scale (MPS) was then developed on the basis of this definition of perfectionism, and the six subscales corresponding to the dimensions above are; Concern over Mistakes, Personal Standards, Parental Expectations, Parental Criticism, Doubt about Actions and Organisation. Other perfectionism scales have also been developed which posit a different factor structure and which are also widely used. Hewitt and Flett (1991) identified three components of perfectionism: self-oriented, other-oriented and socially-prescribed, and also called their scale the Multidimensional Perfectionism Scale (MPS-H). There is considerable overlap between these two scales (Frost et al., 1993), although some of the dimensions are not related. This allows some integration of studies which use one or other of these measures.

Perfectionism, as measured by these scales, has been empirically linked to clinical depression and anxiety disorders (Hewitt and Flett, 1991) but rather less research has been conducted in the area of OCD, despite the emphasis placed on perfectionism by many cognitive theorists. Several studies have reported high levels of perfectionism and related traits amongst the *parents* of OCD patients, which lends support to the model of Frost et al. (1994) and its factors of parental criticism and expectations. For example, Rasmussen and Tsuang (1986) found that a significant proportion of parents of OCD patients had perfectionist traits, while Frost et al. (1994) reported that parental perfectionism, rigidity and criticism was associated with obsessive compulsive symptoms in their non-clinical sample, but only with regard to fathers. In contrast to these findings, Merkel et al. (1993), in their study of 320 OCD patients, found no evidence to support the hypothesis that parents are likely to exhibit criticism, high expectations and perfectionist traits themselves, and remark that explanations of straightforward links between OCD sufferers and parental characteristics are too simplistic. Similarly, Vogel et al. (1997) found that a diagnosis of OCD was not related to parental rearing practices and comment that it is necessary to take into account the contributions of other psychopathology (such as depression) when evaluating differences between patients with OCD and healthy controls. They also warn against relying solely on subclinical student populations.

A number of studies have looked directly at the link between perfectionism and obsessive-compulsive problems. However, most of these have relied on non-clinical populations. Frost and his colleagues (1994), in a study of several personality traits in "subclinical" obsessive-compulsive volunteers, found that subclinical obsessive-compulsives were more perfectionist than a noncompulsive sample. The study involved two separate samples of university undergraduates who were screened and divided into subclinical and noncompulsive groups on the basis of scores on three obsessive-compulsive measures. The groups differed significantly in terms of total perfectionism scores on the MPS. Separate analysis of the two samples revealed that, for the first study which had a lower cut-off for obsessive-compulsive group membership, Concern over Mistakes, Personal Standards and Doubt about Actions were the only MPS subscales significantly higher in the obsessive-compulsive group. In the second analysis, involving a higher cut-off with a second sample of students, the obsessive-compulsive group scored more highly on all subscales of the MPS, indicating to the authors that the relationship between perfectionism and obsessive-compulsive problems is "even more apparent at higher levels of compulsivity" (1994, p.54). The conclusions drawn in this study are limited by the use of relatively small samples of students, at least in terms of generalising to OCD. But more importantly, no attempt is made to measure or control for levels of anxiety or depression, mood states known to correlate with measures of perfectionism. It is not possible therefore, from this study to assess whether perfectionism was *specifically* related to obsessive-compulsive symptomatology rather than other psychopathology which might accompany obsessive-compulsive problems (remembering that OCD has high co-morbidity with anxiety and depression, [Rasmussen and Eisen, 1992]).

A further problem with this study relates to use of the MPS with obsessive-compulsive populations. Frost and his colleagues, in their development of the MPS (Frost et al., 1990 and 1991), point out that the Doubt over Actions subscale is composed of items taken from the Maudsley Obsessive Compulsive Inventory (MOCI, Hodgson and Rachman, 1977). This is a widely used scale designed to measure *obsessive-compulsive* symptoms in both non-clinical and clinical populations. That is, doubt about the quality of ones actions is considered by Frost et al. to be a specific dimension in perfectionism whilst also constituting a key feature of OCD. Later studies like that done by Frost et al. (1994), which utilise the MPS to measure perfectionism in obsessive-compulsive samples, encounter a serious problem since the two scales will inevitably overlap through the Doubt over Actions subscale. To compound the problem, Frost et al. used the MOCI to identify their obsessive-compulsive group. The Doubt over Action subscale cannot logically be used to measure perfectionism in OCD samples, and yet such an analysis appears in several studies in this field.

Gershuny and Sher (1995) used the MPS in their study of compulsive checkers. However, they noted the problem of content overlap with the MOCI, and used only the dimensions of Concern over Mistakes, Personal Standards and Parental Expectations to arrive at a total score, since they judged these to represent a more general expression of the trait of perfectionism. Utilising a population of undergraduates, screened using the MOCI checking subscale, they found that checkers were significantly more perfectionist than a group of anxious controls and a group of non-anxious controls. Since the checking group did not differ from the anxious group on measures of state-trait anxiety or depression, the authors argue for a specific relationship between perfectionism and obsessive checking. Once again the conclusions are limited in their generalisability by the use of a non-clinical population and, in this case the inclusion of checking problems only. However, as the authors point out, there is evidence that obsessive-compulsive problems lie on a continuum and so they argue that non-clinical studies of tightly defined psychological phenomena can lead to a better understanding of obsessive-compulsive disorder.

Ferrari (1995) included a clinical sample in his study of perfectionism and obsessions and compulsions. Using the Perfectionism Cognitions Inventory (PCI), this study found an association between perfectionist thinking and obsessive-compulsive tendencies. However, as the author notes, there are several limitations to the research. In particular, the clinical sample was self-selected from a support organisation, and there was no control for anxiety or depression. As Ferrari points out, the PCI is related to anxiety disorders in general, which weakens the evidence for a specific link with obsessional problems.

Rhéaume and colleagues (1995) suggested that perfectionism has an important role in OCD, noted the lack of empirical evidence, and reported on a study which measured several traits including perfectionism and obsessionality in a large sample of nonclinical students. The results showed a relationship between MPS scores and obsessional-compulsive symptoms measured by the Padua Inventory. The Doubt about Actions and Concern over Mistakes MPS subscales were most highly correlated with obsessive-compulsive symptoms. The only subscale not correlated with the Padua Inventory was Organisation, a subscale often omitted from MPS research as it does not appear to be a core component of perfectionism (according to the scale's own authors, Frost et al., 1990). A hierarchical regression analysis was also conducted which suggested that even when other variables such as responsibility were partialled out, perfectionism still explained a significant part of the variance of obsessive-compulsive symptoms. In common with many other studies, this work used a student sample which weakens its generalisability to OCD, and again, no controls for anxiety or depression were included. The authors also comment on the limitations of the MPS in some detail. Their factor analysis of the scale suggested that it had a more unstable factorial structure than previously reported. Furthermore, they question the relevance of many of the subscales to a definition of perfectionism (for example the developmental nature of the two parental subscales) and conclude that multidimensional scales such as the MPS do not adequately address the fundamental nature of perfectionism that leads to psychopathology that is, a self-referential belief in, and search for, the perfect state. This study concluded that perfectionism plays a significant and underestimated role in obsessional problems, and the authors assert that perfectionism should be corrected in clinical treatments of OCD. However, it also stressed that "perfectionism may not be specific to OCD, but may represent a necessary but insufficient trait for development of OCD" (Rhéaume et al., 1995, p.793).

Frost and Steketee (1997) addressed the need for clinical populations and control groups in their recent study of perfectionism and OCD. They compared 35 clinically diagnosed OCD patients with a group of 14 patients diagnosed with panic disorder with agoraphobia (PDA) and a group of 35 community controls. Using a compulsive activity checklist and the MPS, this study found that both OCD and PDA patients scored higher in perfectionism than community controls but that the two clinical groups did not differ from each other in terms of total perfectionism scores. Indeed the only MPS subscale which distinguished OCD from PDA patients was Doubt about Actions. The authors make minor reference to the confound problem with this particular subscale but omit to point out that it consists of MOCI questions, and still suggest that it represents some feature of perfectionism that differentiates OCD patients from other anxiety disorders. Overall, this study supports only a non-specific relationship between OCD and perfectionism, and goes some way to refuting the model of McFall and Wollersheim which places perfectionism in such a prominent role in the condition. However, the conclusions are limited by small sample size and lack of control for mood state.

Frost and Steketee's study is important in being the first to include well defined clinical groups with different disorders for comparison. Antony and colleagues (1998) also compared a range of clinical diagnoses, including OCD, in their study of perfectionism across the anxiety disorders. Specifically, patients with social phobia, OCD, panic disorder and specific phobia and a nonclinical control group were compared on measures of perfectionism and depression. The results replicated Frost and Steketee's (1997) findings with OCD being associated with higher scores relative to the anxiety groups (except for social phobia) on Doubt about Actions. However, when compared with the non-patient control group OCD patients differed only on Concern over Mistakes and Doubt about Actions. This is in contrast to much of the previous research which shows a more robust difference in perfectionism between OCD and normal controls. Antony and his co-workers point out that further research is needed to better understand the precise relationships between different dimensions of perfectionism and the anxiety disorders. The study was limited in that, while OCD was diagnosed according to DSM-IV, no standardised measure of obsessive-compulsive symptoms was administered to the clinical groups. Also, the study measured levels of depression and found that the groups differed on this measure, but did not then control for this finding. Thus the results are somewhat inconclusive.

The research on OCD and perfectionism suggests that the link between the two is far from simple. Several studies show that people with OCD are more perfectionist than the normal population but there is, as yet, no clear evidence for a specific relationship. The research in this field suffers many limitations, and so further studies are required to properly assess the part perfectionism plays in obsessive-compulsive problems.

Guilt

Guilt is widely viewed as characteristic of patients with obsessive-compulsive problems, and it has long been thought that associated traits such as moral rigidity or sensitivity underpin a variety of obsessive-compulsive concerns such as safety, cleanliness, control of taboo or blasphemous thoughts and so on (Rachman and Hodgson, 1980; Rachman, 1993; Steketee et al., 1991). Klass (1987), who has researched the emotional trait extensively, defines guilt as;

"an aversive conscious emotion that involves self-reproach and remorse for one's thoughts, feelings, or actions and a sense of wrongdoing, as if one has violated moral principle" (1987, p. 36).

Whilst Klass has not researched guilt and OCD directly, she has developed an assessment tool for measuring guilt, the Situational Guilt Scale (Klass, 1987), which is widely used in cognitive-behavioural research. Guilt is regarded as an affective trait but also encompasses cognitive features, such as beliefs about right and wrong. In the present study, guilt will subsumed under the heading of a cognitive trait for ease of discussion and comparision with the two other traits (perfectionism and responsibility) being considered.

McFall and Wollersheim (1979) afford guilt an important part in their cognitive-behavioural model of OCD. They posit that primary appraisals made in OCD include the belief that certain thoughts and feelings are unacceptable, and should be punished. Experiencing these unacceptable impulses then leads to guilt, and compulsions are, at least in part, a way of reducing or avoiding the risk of being found culpable for potentially catastrophic outcomes. More recently, van Oppen and Arntz (1993) have suggested a similar process exists in OCD whereby the sufferer tries to avoid a depressive position of feeling guilty, worthless or a failure, by performing rituals.

The importance placed on guilt in OCD is reflected in the inclusion of guilt as an 'associated feature'in the DSM-IV description of the disorder (APA, 1994). However, there is a paucity of research demonstrating such a relationship. It is well documented that high levels of guilt are associated with other forms of psychopathology, particularly depression (Jarrett and Weissenburger, 1990), but the evidence for a link with OCD is mixed. Steketee, Grayson and Foa (1987) compared a group of OCD patients with a group of anxiety-disordered patients on a number of dimensions including guilt. Patients were required to choose applicable adjectives from a list of self-descriptors such as 'inadequate, stupid, guilty'. No standardised measures of guilt were used. Whilst the authors claimed that their OCD group showed a greater tendency towards guilt and self-blame than the anxiety-disordered group, the results are far from conclusive. The groups differed on several important dimensions such as depression which was not taken into account in the analysis, and the finding that patients with Generalised Anxiety Disorder had *higher* levels of checking and similar levels of guilt compared to the OCD group weakens the conclusions still further. Steketee, Quay and White (1991) also used clinical populations to investigate religion and guilt in OCD patents. Using the Situational Guilt Scale they found that OCD patients were not significantly more guilty than other anxious patients. They did, however, find that guilt was positively correlated with greater obsessive-compulsive symptoms, but not with moodstate, arguing for some specific association between guilt and OCD. Unfortunately they did not look at symptom severity and guilt in the anxiety group to assess the specificity of the relationship. A final clinical study provides some indirect evidence for a link between guilt and OCD. Amir, Cashman and Foa (1997) compared patients with OCD with non-patient controls and found that self-punishment strategies (an example being 'I get angry at myself for having the thought') were the strongest discriminators of OCD patients and controls. Clearly the notion of self-punishment is closely related to guilt, though no direct measures of the trait were used.

Studies using non-clinical populations have provided some supportive evidence for the role of guilt in OCD. Niler and Beck(1989) examined the relationship between intrusive thoughts and guilt in a college sample. They found that the frequency of intrusive thoughts, their dismissability and levels of distress were best predicted by guilt rather than levels of anxiety or depression. Reynolds and Salkovskis (1991) attempted to replicate these findings, but their study, using a large sample of students, found that guilt did not independently predict intrusive thoughts. Instead, anxiety and depression were the strongest predictors. Freeston et al. (1992), in another non-clinical study, examined the relationships between several dimensions of cognitive intrusions (including guilt), and compulsive, depressive and anxious symptoms. The only significant predictor of compulsive activity scores was a factor the authors describe as 'Evaluation'. This factor incorporates the dimensions of responsibility, disapproval and guilt. Finally, Frost et al. (1994) found that 'subclinical obsessive-compulsive' students (defined by scoring above an arbitrary cut-off on OCD measures) scored significantly higher on the Situational Guilt Scale than a noncompulsive sample. This study points to an association between guilt and obsessive-compulsive symptoms, but is limited by the absence of controls for anxiety and depression and, in common with all the non-clinical studies, its generalisation from student samples to obsessive-compulsive disorder.

In summary, there is a small but growing body of research which indicates that there is some sort of relationship between guilt and OCD, though the evidence is conflicting. These studies, however, are limited by numerous methodological problems, and have failed to determine whether guilt is specific to obsessive-compulsive symptoms or if it accompanies other anxiety states as well.

Research on perfectionism and guilt lend some support to the hypotheses proposed by McFall and Wollersheim in their model of obsessive-compulsive disorder. However, both traits need a great deal more investigation before their role in OCD is properly understood.

Salkovskis' responsibility model of obsessive-compulsive disorder

McFall and Wollersheim's model of OCD has been criticised by Salkovskis, a prominent researcher and author in the field of obsessive-compulsive disorder. Salkovskis (1985) describes this model as an attempt at 'bridging the gap' between behavioural and psychoanalytic theory. He is critical of what he sees as psychodynamic concepts such as the 'preconscious' (although McFall and Wollersheim distinguish between this and the unconscious of psychoanalytic theory). He also remarks that the primary and secondary appraisal processes are not elaborated upon in terms of what cognitive or behavioural manifestations the appraisals might predict. Thus, Salkovskis' main criticism is that the model of McFall and Wollersheim does not distinguish between OCD and other anxiety disorders. He has proposed and developed his own theory which claims to explain the wide range of clinical phenomena associated with OCD (Salkovskis, 1985).

Salkovskis (1985, 1996) has proposed a cognitive model of OCD which starts from the premise that people suffering from obsessions do so because they make negative appraisals of their intrusive thoughts, images or impulses. This part of his model derives from general cognitive models of anxiety whereby a negative emotional response to a stimulus (thought, situation or event) results from a person's negative misinterpretation of that stimulus. In particular, Salkovskis proposes that OCD sufferers interpret the occurrence and/or content of their intrusions as indicating that they are in danger of bringing harm to themselves or others. That is, they believe that they are potentially *responsible* for such harm. Salkovskis uses the term responsibility in a specific way in the context of OCD and defines it as follows:

"The belief that one has power which is pivotal to bring about or prevent subjectively crucial negative outcomes" (Salkovskis, 1996).

This appraisal of personal responsibility has a number of important effects. Salkovskis proposes that it precipitates increased discomfort, anxiety and depression, results in efforts to control or suppress the intrusive thoughts and, crucially, leads the person to engage in "neutralising" responses in an attempt to avoid or discharge their perceived responsibilities. Neutralising includes both overt behaviours such as checking and washing, and covert responses such as mental checking and restitution responses (e.g. putting things right by repeatedly saying prayers or 'good thoughts').Thus Salkovskis argues that compulsive behaviours in OCD are neutralising responses which reduce discomfort in the short term, but which increase preoccupation and the frequency of further intrusive thoughts in the long term. He argues that neutralising causes intrusive thoughts to become more salient and frequent, evoking further discomfort and prompting even more neutralising efforts.

This 'responsibility appraisal' is afforded a crucial role in Salkovskis' model. It is claimed to be a defining characteristic of obsessive-compulsive disorder. The responsibility component "distinguishes obsessional cognitions from anxious and depressed cognitions" (Salkovskis, 1996, p. 112). The concept has caught the imagination of many researchers in the field and is afforded a key position in the OCD Cognitions Working Group (1997) document on important beliefs in OCD. A pathological sense of responsibility is also included in the DSM-IV description of OCD, being considered an associated feature of the disorder (APA, 1994). Unfortunately there is little direct evidence that responsibility appraisals do indeed characterise obsessive-compulsive problems. Even in a very recent paper which presents this model in some detail (Salkovskis, 1998), empirical evidence is noticeable by its absence. There are, however, some studies which have addressed the notion of inflated or excessive responsibility, and these will be described here in brief.

Tallis (1994), in common with many authors (Rachman, 1993), asserts that clinical observations support the significance of responsibility in obsessional phenomena. He provides case reports of OCD patients in which inflated responsibility (as well as guilt and thought-action fusion) appear to be central themes in the presentation. Purdon and Clark (1994) used self-report questionnaires and a student sample to explore the relationship between intrusive thoughts, and cognitive appraisals. They found that concern that one's intrusive negative thought would come true had a significant association with the thought's controllability and frequency. The authors concluded that this supports the role of inflated responsibility in obsessional intrusions, as they argue that people's belief in their capacity to act on an intrusive thought is an important and necessary aspect of inflated responsibility. This indirect evidence is weakened by the same study's findings that their measure of responsibility was *not* an important predictor of controllability. The authors suggest that the problem lies in the wording of their questionnaire item on responsibility, regard responsibility as a difficult feature to assess, and thus conclude that individuals may hold "rather idiosyncratic views on what it means to be responsible" (p. 281).

Rhéaume, Ladouceur, Freeston and Letarte (1994) developed a self-report questionnaire, using a semi-idiographic design, to explore perceived responsibility in personally relevant situations. The development of the responsibility questionnaire (the RQ) was an attempt to empirically test the validity of a definition of responsibility which closely matches that of Salkovskis. In this case the definition is "the belief that one possesses pivotal power to provoke or prevent subjective crucial negative outcomes" (p. 266). Responsibility was measured along a number of domains including severity, probability, influence and pivotal influence. Measures of anxiety and depression, as well as obsessive-compulsive symptoms, thoughts and beliefs were also included. A large sample of student volunteers took part in the study, which found that responsibility was most strongly correlated with obsessive-compulsive symptoms but only weakly correlated to anxiety and depression. The authors concluded that the RQ showed good convergent and discriminant validity, and that the results support Salkovskis' cognitive model of OCD. Some of the results, however, weakened this conclusion, as the RQ did not correlate with beliefs about obsessions or compulsive activities. The use of a non-clinical sample was justified by the research on analogue studies in OCD, but the authors do comment on the inherent difficulty in accessing responsibility beliefs in non-clinical samples when these beliefs are by definition associated with clinical disturbance. A further study by the same authors, utilising the same student sample, looked at the RQ in more detail and concluded that pivotal influence (one of the RQ domains) was the best predictor of the overall responsibility score (Rhéaume et al., 1995). This 'responsibility schema', represented by the pivotal influence finding, is therefore considered central to OCD. The study is limited again by its reliance on a non-clinical sample. The authors comment that these findings need to be replicated, especially with a clinical population, to confirm the presence of a responsibility schema.

Another study by Rhéaume and his colleagues (Rhéaume et al., 1995) looked at the relationship between responsibility, perfectionism and obsessive-compulsive symptoms in a sample of 245 students. No controls for mood were included. They found that two measures of responsibility were moderately related to obsessive-compulsive symptoms. However, they regard the results as only partially supportive of Salkovskis' model of OCD, since they found perfectionism to be independently important in predicting obsessive-compulsive symptoms and this trait does not feature in Salkovskis' analysis of OCD.

A study by Scarrabelotti and colleagues (Scarrabelotti et al., 1995), which addresses responsibility in a slightly different context, looked at the relationship between obsessive-compulsive symptoms, depression, responsibility (measured using a scale developed by the authors for this study) and Eysenckian personality dimensions. Using a student sample, the authors found that neuroticism and responsibility each accounted for a significant amount of variance in predicting obsessive-compulsive symptoms, over and above depression. They also conducted the same study on a small clinical sample of 20 patients diagnosed with obsessive-compulsive disorder, with the same results. Interestingly, they propose that responsibility is a dimension of psychoticism (in Eysenckian terms) and as such, the two traits should be inversely correlated. The studies described here lend some support to this notion, but this conceptualisation of responsibility has not been followed up by other researchers in the field. It is difficult to draw strong conclusions about the role of responsibility in this study, as the scale used here was developed by the authors according to a wider definition than that proposed by Salkovskis (1985). The scale includes items relating to beliefs about thought control and thought-action fusion as well as responsibility for harm. However, the study is one of the few to measure responsibility in a clinical population, and thus offers some tentative support for Salkovskis' model.

Lopatka and Rachman (1995) took a different approach to testing the responsibility hypothesis, by direct experimental manipulation of levels of responsibility in a clinical sample. They sought to test a central notion in Salkovskis' model, that perceived responsibility has a direct effect on compulsive checking (i.e. checking is a form of neutralising behaviour initiated in response to responsibility appraisals about intrusive thoughts). Patients were exposed to situations in which they normally checked or cleaned and responsibility was manipulated by explicit instructions and a 'contract' which emphasised that high responsibility was placed on the patient, or in another condition, on the therapist. The study was carefully designed with 'blind' pre- and post-assessments, structured clinical interviews and control conditions. The results confirmed that the experimental manipulation produced significant shifts in responsibility in the expected direction, but only when responsibility was decreased (in which case the urge to check went down). Increased responsibility did *not* result in an increased urge to check. Cleaning compulsions were less influenced by responsibility manipulations overall than checking. The authors suggest that their study provides partial support for Salkovskis' model, but point out that there is a need for a dependable measure of responsibility in future research, which will clarify the nature of responsibility, particularly with regard to whether it is a stable trait or better construed as a situation-specific response.

Following on from this study of directly manipulated responsibility, Shafran (1997) proposed that a more 'naturalistic' manipulation could arise by leaving participant alone during exposure conditions (high responsibility) compared to exposure with the experimenter present (low responsibility). Her study involved 40 volunteers diagnosed with OCD entering both high and low responsibility conditions. Verbal analogue scales were used to determine how responsible each participant felt in the exposure situation. The results supported Shafran's hypothesis that perceived responsibility would be higher when alone and would lead to greater discomfort and urges to check. The author points out however, that other cognitions may have been manipulated in the study and could arguably account for the findings. For example, increased estimation of probability of risk or danger expectancies may have been confounding variables.

Whilst these studies provide some support for the role of responsibility in OCD, other research has challenged this notion. O'Kearney (1993) describes a case report of a woman with obsessional thoughts where beliefs about responsibility were not in evidence, but fear of losing control of her thoughts was predominant. O'Kearney concludes that other psychological factors may be important in the etiology and maintenance of OCD, and cautions against placing too much emphasis on responsibility. He also argues that the definition of responsibility and its links to neutralising require further clarification. Other studies too, have noted complex relationships between obsessional thoughts and neutralising responses, which challenge Salkovskis' model. Freeston and Ladouceur (1997) found, in an analysis of strategies used by OCD patients in response to their obsessional thoughts, that only a proportion of the strategies were attempts to decrease perceived responsibility. The authors argue that many types of appraisals are implicated in the development of OCD, for example need for control and perfectionism, as well as responsibility. Other researchers have made the same point, that there is empirical evidence for other OCD-related appraisals which may be just as crucial as responsibility. Rachman et. al., (1995) consider that thought-action fusion, as well as guilt and responsibility, are key factors in OCD, whilst Jones and Menzies (1997) argue that their more recent study supports a danger-based model of OCD. In their research a group of OCD patients were exposed to a contamination task and cognitive and behavioural measures were made of the patients' responses to the exposure. Responsibility was assessed by a single question about how responsible they would feel if harm befell them or others following 'contamination'. Danger expectancy was the only mediating variable which correlated with increased neutralising behaviours. The study was important in its emphasis on experimental method and use of robust behavioural measures, but the conclusions are severely limited by the way responsibility was measured, and the lack of controls for anxiety and depression.

Research on the responsibility model of OCD presents a mixed picture. Clinical observations and case reports strongly support the idea of excessive responsibility in obsessive-compulsive patients, and there is some empirical evidence to suggest that the trait does show a relationship to obsessive-compulsive symptoms. However the research is limited by a number of drawbacks. Researchers have used differing methodologies, and many of the responsibility measures devised have not been standardised. The use of non-clinical samples, lack of control or comparison groups are further problems with recent research. Salkovskis claims that inflated responsibility is specific to obsessive-compulsive problems, although other writers have questioned this view (Jakes, 1989), and yet there is no study which tests this basic assumption by comparing perceived responsibility in different disorders. The proposal that responsibility is *central* to the development of OCD is also as yet unproven. As outlined above, some studies suggest that other cognitive biases may be equally or more important than responsibility in explaining obsessive-compulsive symptoms. There is clearly an urgent need for empirical research to address this issue.

Current status of cognitive-behavioural models of OCD

There is a wealth of literature on postulated cognitive deficits or biases in obsessive-compulsive disorder, but little consensus on a specific theory which can encompass all these findings. The place of responsibility in cognitive-behavioural models of OCD remains unclear. Salkovskis is critical of the appraisals proposed by McFall and Wollersheim (1979) since he claims they do not distinguish OCD from other disorders, and yet his ideas about responsibility overlap considerably with their 'primary appraisals' in OCD. The primary appraisal that "one is powerful enough to initiate or prevent the occurrence of disastrous outcomes" (McFall and Wollersheim, 1979, p.335), corresponds closely to Salkovskis' notion of responsibility being "power which is pivotal to bring about or prevent subjectively crucial negative outcomes" (1997, p.212). Jakes (1989), in his own critique of Salkovskis' cognitive-behavioural formulation, comments that one might argue that McFall and Wollersheim did anticipate most of Salkovskis' arguments. However, McFall and Wollersheim proposed that there are *several* cognitive appraisals forming a 'constellation of unreasonable ideas', which contribute to obsessive-compulsive thinking, whereas Salkovskis places primary importance on responsibility.

The literature tends to support a multi-factorial model in terms of explaining the variety of cognitive processes found to be related to OCD. Whether McFall and Wollersheims' set of appraisal biases is the most parsimonious formulation remains to be established by empirical testing. Their particular model has not been subject to empirical evaluation, however, the notion that a set of traits whose *inter-relationship* might best explain the development and maintenance of OCD, is considered by many writers to be a useful way of conceptualising the condition. Rhéaume et al. (1995), for example, propose that responsibility *and* perfectionism are key cognitive biases in OCD, whilst Rachman (1997) considers that responsibility and guilt as well as other cognitive processes such as thought-action fusion, play a role in the condition. The precise 'mix' of cognitive styles and the relative importance of each factor is far from clear. Such a sophisticated analysis requires much empirical research and theory-building. However, McFall and Wollersheims' model gives some direction to researchers in that it highlights those appraisals which intuitively seem related, and which have at least some empirical support. Thus one would predict that perfectionism, guilt and responsibility, which are closely linked to their four primary appraisals, would be related to obsessive-compulsive symptoms and should distinguish people with OCD from those with other types of disorder. There have been very few studies which, in assessing these traits, have included a clinical comparison group to begin to address the question of specificity in OCD cognitions. Just as importantly, few studies have consistently included measures of anxiety and depression and controlled for these, despite the fact that these factors are known to relate to many cognitive traits independently of levels of obsessive-compulsive symptomatology (Reynolds and Salkovskis, 1991; Scarrabelotti et al., 1995).

The study presented here was conducted in order to compare the levels of perfectionism, responsibility and guilt in two clinical samples. It was predicted that these appraisal biases would be present to a greater degree in OCD patients compared to patients with other anxiety disorders. The study was also designed to address the relationship between these three traits and levels of depression, anxiety and obsessive-compulsive symptoms across all patients, and it was hypothesised that levels of perfectionism, responsibility and guilt would be positively related to obsessive-compulsive symptoms.

Method

Participants

Participants in the study were 47 patients who ranged from 19 to 68 years of age. All were attending a specialist mental health centre for treatment.

The OCD group contained 9 males and 12 females diagnosed with OCD. Ten patients suffered mainly with contamination fears, 8 with checking symptoms and 2 with other types of obsessive-compulsive symptoms. The anxiety disorder (AD) group contained 6 males and 20 females diagnosed with an anxiety disorder. This included 10 patients with panic disorder, 8 with generalised anxiety disorder, 6 with specific phobia and 2 patients with social phobia. A 'normal' control group of people without any anxiety diagnosis was not included in this study, since the specific focus was on comparing different clinical groups to establish whether they differed from each other in the expected direction on the traits of interest.

All patients had been assessed and received a diagnosis by the mental health professional in charge of their care. To ensure that diagnoses conformed to DSM-IV criteria, all patients' casenotes were reviewed. In addition, the professional keyworker involved with each patient was interviewed to confirm that the assessment and diagnosis given in the notes fulfilled the relevant DSM-IV criteria. All eligible presenting patients were recruited until target numbers were achieved. Patients were invited to participate in the study if their main diagnosis was either OCD or one of the following anxiety disorders: social phobia, panic disorder (with or without agoraphobia), generalised anxiety disorder, or specific phobias. Patients with evidence of psychosis, substance misuse or learning difficulties were not included in the study. All participating patients gave their consent according to ethical committee guidelines.

Regarding sample size in this study, a power analysis was performed at the design stage to assess the sample size required. For *t*-test analyses, a total sample of 42 would ensure a power value of 0.7, whilst for correlational analyses a minimum total sample of 49 would be required to achieve the same degree of power. Because of the clinical nature of this research, there were limits on the number of appropriate patients who could participate, and therefore the final sample size obtained was 47. Thus, *t*-test analyses were sufficiently powerful, whilst the correlations approached a satisfactory level for the purposes of this study.

Measures

Participants were asked to complete the following measures.

The Hospital Anxiety and Depression Scale (HADS, Snaith & Zigmond, 1994). The HADS is a 14-item questionnaire consisting of two subscales, one measuring generalised state anxiety (HAD-A), and one measuring depression (HAD-D), which are scored separately. The HADS is quick and easy to complete and has good validity and reliability. The scores for each subscale range from 0 to 21, with interpretive categories of normal (0 to 7), mild (8 to 10), moderate (11 to 14) and severe (15 to 21).

The Maudsley Obsessive Compulsive Inventory (MOCI, Hodgson & Rachman, 1977). This is a 30-item true/false measure of obsessive-compulsive symptoms, widely used in OCD research and demonstrating good reliability and validity. A total score measuring obsessional-compulsive complaints was analysed. The possible range of scores was 0 to 30, with a high score indicating a more severe degree of obsessional-compulsive symptoms.

The Multidimensional Perfectionism Scale (MPS, Frost et al., 1990). The MPS consists of 29 items which are scored on a five-point scale. A total perfectionism score is calculated with a possible range of 29 to 145, with a higher score indicating a higher level of perfectionism. As well as a total score, subscores are obtained for the following five subscales. Concern over Mistakes reflects negative reactions to mistakes, Personal Standards relates to the setting of very high standards and the excessive importance placed on these, Parental Expectations reflects the perception that ones' parents set very high standards, Parental Criticism relates to excessive criticism from parents, and Doubts about Actions reflects the extent to which people doubt their ability to accomplish tasks. It has been noted in several studies that the Doubt Over Actions subscale of the MPS has considerable overlap with items on the MOCI, and may reflect checking symptoms in OCD (e.g. Frost and Steketee, 1997; Gershuny and Sher, 1995). To avoid overlap, and possible statistical problems of multicollinearity, the Doubt Over Actions subscale was not included in the total perfectionism score in the correlation analyses.

The Situational Guilt Scale (SGS, Klass, 1987). The SGS is a 22 item questionnaire which assesses degree of expected guilt feelings in specific, naturally occuring situations. It yields scores for total guilt, and for three subscales of guilt over interpersonal harmdoing, norm violation and failures of self-control. Total possible guilt scores range from 88 to 440, with a higher score indicating higher levels of guilt. All three subscales were included here. Validation studies support the reliability and validity of the SGS.

Responsibility Questionnaire (RQ, Rhéaume et al., 1994). The RQ consists of a 14-page booklet, each page outlining a different OCD-relevant target situation. The patient is required to describe a possible negative outcome and then rate this outcome on six dimensions: severity, probability, influence, pivotal influence, responsibility and relevance. Each dimension is scored on a 9 point scale. The responsibility subscale is the dimension of particular interest in the present study, and, in common with other studies, the responsibility total score is the only one to be reported here. The range of possible scores on this subscale is 14 to 126, with a higher score indicating a higher dgree of sense of responsibility. The RQ is a semi-idiographic measure which has demonstrated reasonable reliability, and concurrent and discriminant validity.

Procedure

Participants were given a pack containing the five questionnaires and an information sheet, and were asked to complete the measures in their own time. The pack was then returned by post or by hand to the researcher. All those patients who were asked to participate agreed to complete the measures.

Data Analysis

Reliability analyses for all scales in the study were computed using Chronbach's alpha coefficient. The two samples (OCD group and AD group) were compared in the following way. Since gender and medication measures are nominal in nature, 2×2 Chi-square tests were used to examine differences between the groups on these variables. For the remaining demographic variables, the distribution of scores were examined to assess degree of skewtosis and kurtosis and approximation to a normal distribution. Age was then analysed using *t*-tests, while a Mann Whitney U test was applied to education and duration, as these variables did not conform to a normal distribution. All questionnaire variables were acceptably close to a normal distribution, and so *t*-tests were used to compare samples on anxiety, depression, obsessive-compulsive symptoms, perfectionism, responsibility and guilt. Tests of significance were two-tailed in all cases.

Associations between anxiety, depression, obsessive-compulsive symptoms, perfectionism, responsibility and guilt were analysed across samples using Pearson correlation coefficients. To examine further the relationship between obsessionality, perfectionism, responsibility and guilt whilst controlling for levels of anxiety and depression, partial correlations and a sequential linear regression analysis were performed combining patients from both samples.

Results

Reliability Analysis

A reliability analysis was performed on each of the questionnaires used in the study, combining patients from both samples, and Cronbach's alpha coefficients for each measure are reported in Table 1.

Table 1. Reliability (Cronbach's alpha) for all measures

Questionnaire	alpha
HAD(total)	0.90
MOCI	0.84
MPS(total)	0.95
SGS(total)	0.97
RQ	0.89

It can be seen from Table 1 that all these alphas are high, indicating that all the scales were internally reliable.

Group Data Analysis

Analyses were conducted to check that the two patient groups were equivalent on certain demographic and medical history variables. The Chi-square analysis for gender is presented in Table 2, and the analysis for medication is shown in Table 3. The two groups did not differ significantly in terms of proportions of males and females, or the proportion on psychotropic medication (in all cases the medication involved was a form of anti-depressant).

Table 2. Distribution of men and women in each group

	OCD group	AD group	χ^2	corrected χ^{2*}	df	p
male	9	6	2.09	1.28	1	0.26
female	12	20				
Total	21	26				

* = Correction for continuity when df = 1

Table 3. Proportion of those on medication in each group

	OCD group	AD group	χ^2	corrected χ^2	df	p
medication	14	10	3.70	2.66	1	0.10
no med	7	16				
Total	21	26				

Table 4 presents means and standard deviations for the two groups for the demographic variables of age, education and duration of illness. Age is reported in years, education is measured in years of full-time education, and duration in months since diagnosed with illness.

Table 4. Demographic characteristics of the two groups

Variable	OCD group (N =21)		AD group (N = 26)	
	Mean	SD	Mean	SD
Age	43.1	13.6	36.7	12.8
Education	12.2	2.9	12.3	2.1
Duration	189.5	174.7	105.5	108.1

The two groups were not significantly different in terms of age ($t = 1.64$, df 45, $p < 1.11$), education level (Mann-Whitney U = 240.00, $p < 0.46$), or duration of illness (Mann-Whitney U = 193.00, $p < 0.09$).

Differences between the two groups on the psychological measures were evaluated using *t*-tests. (Whilst several t-tests were performed here, it was not considered necessary to use a correction as the number of tests is not excessive, and the differences of interest in this study are relatively large). Means, standard deviations and *t*-test results for the questionnaires and their subscales are given in Table 5.

Table 5. Questionnaire results for OCD and Anxiety Disorder groups

Variable	OCD group (N = 21)		AD group (N = 26)*				
	Mean	SD	Mean	SD	*t*	df	*p*
HAD-A	13.6	3.9	13.2	4.7	0.35	45	0.72
HAD-D	9.5	5.0	7.5	5.5	1.29	45	0.20
MOCI	**15.2**	**5.4**	**10.4**	**5.3**	**3.07**	**45**	**0.00**
MPS - total	91.1	21.2	82.6	20.2	1.40	45	0.17
Concern over mistakes	27.4	8.6	25.3	7.4	0.91	45	0.37
Personal Standards	22.9	5.9	22.4	4.9	0.33	45	0.74
Parental Expectations	14.2	5.3	12.7	4.6	1.10	45	0.28
Parental Criticism	11.6	4.7	9.6	4.1	1.50	45	0.14
Doubt About Actions	**14.8**	**2.5**	**12.5**	**4.1**	**2.30**	**45**	**0.03**
SGS - total	316.8	62.5	293.3	65.9	1.24	45	0.21
Interpersonal Harm	128.2	26.1	120.7	25.7	0.99	45	0.34
Norm-violation	78.1	17.0	76.1	18.3	0.50	45	0.62
Self-control failure	82.1	17.9	71.6	20.9	1.83	45	0.07
RQ	79.0	22.4	76.0	20.6	0.47	44	0.64

* N = 25 for the AD group on the RQ scale as one patient omitted an item on this questionnaire.

There were no significant differences in levels of anxiety or depression between the two groups. As expected, the OCD group reported significantly more obsessive-compulsive symptoms on the MOCI than the Anxiety Disorder group. The two groups did not differ on the responsibility scale or any of the guilt subscales. There was no significant difference between the groups on total perfectionism scores, but they did differ on one perfectionism subscale, Doubt Over Actions.

To explore the relations among constructs in more depth the two groups were combined for the following analyses.

Correlation Analyses

Table 6 presents Pearson correlations between levels of obsessional-compulsive symptoms, anxiety, depression, responsibility, perfectionism and guilt. (The Doubt Over Actions subscale is omitted from the MPS total scores reported from now on since these items overlap with items on the MOCI. The reliability analysis of the revised MPS scores based on 25 items was alpha = 0.94, and the scores approximated to a normal distribution).

Table 6. Pearson correlations for the six questionnaire scales

	HAD-A	HAD-D	MOCI	MPS	SGS
HAD-A					
HAD-D	0.66**				
MOCI	0.58**	0.49**			
MPS	0.32*	0.37*	0.51**		
SGS	0.42**	0.29*	0.33*	0.45**	
RQ	0.46**	0.38**	0.46**	0.55**	0.58**

N = 47, except for RQ correlations where N = 46
** correlation is significant at the 0.01 level (2-tailed)
* correlation is significant at the 0.05 level (2-tailed)

The correlation matrix reveals that all the scales were significantly positively inter-correlated. A further correlation analysis presented in Table 7 examined the relationship between obsessionality (MOCI) and the three traits of interest: perfectionism (MPS total and subscales), guilt (SGS), and responsibility (RQ), with the effects of anxiety and depression partialled out. The MPS subscales are included to allow a more detailed analysis of the relationship between the particular dimensions of perfectionism and obsessionality. The dimensions are Concern over Mistakes (MPS-CM), Personal Standards (MPS-PS), Parental Expectations (MPS-PE), Parental Criticism (MPS-PC).

Table 7. Partial correlation coefficients controlling for HAD-A and HAD-D (df = 42)

	MOCI	MPS	MPS-CM	MPS-PS	MPS-PE	MPS-PC	SGS
MOCI							
MPS	0.40**						
MPS-CM	0.10	0.85**					
MPS-PS	0.40**	0.78**	0.60**				
MPS-PE	0.46**	0.86**	0.53**	0.53**			
MPS-PC	0.45**	0.81**	0.51**	0.41**	0.86**		
SGS	0.10	0.37*	0.36*	0.23	0.29	0.35*	
RQ	0.25	0.47**	0.42**	0.28	0.45**	0.40**	0.47**

** correlation is significant at the 0.01 level (2-tailed)

* correlation is significant at the 0.05 level (2-tailed)

With the effects of anxiety and depression controlled for, obsessionality correlated significantly and positively with perfectionism, but not with responsibility or guilt. Looking at the subscales of the MPS, obsessionality correlated highly with Personal Standards, Parental Expectations and Parental Criticism but was not related to Concern over Mistakes.

Sequential Multiple Regression Analysis

Tables 8 and 9 present the results of the regression analysis performed on both groups of patients combined. Obsessionality (MOCI) was the dependent variable. As anxiety was considered likely to be of greatest theoretical importance in terms of predicting obsessionality this was entered as the first independent variable. Depression correlated highly with anxiety and so to avoid problems of multicollinearity and reduced power, depression was not entered into the regression. At the second point of entry, the perfectionism subscales, responsibility and guilt were entered as one block of independent variables. Since Parental Expectations and Parental Criticism were highly correlated these variables were combined before entry into the analysis. The combined is coded as MPS-P. Colinearity and residuals diagnostics indicated no problems with outliers or colinearity for the final variables included.

Table 8. Model summary (Dependent Variable: MOCI)

Model	Variables entered	Adjusted R Square	F	df	Sig.
1	HAD-A	0.33	23.36	45	0.000
2	MPS-CM MPS-PS MPS-P SGS RQ	0.51	8.70	45	0.000

Table 9. Regression Coefficients for Models in Table 8

Model		Standardised Coefficients Beta	t	sig.
1	HAD-A	0.59	4.83	0.00
2	HAD-A	0.55	4.30	0.00
	MPS-CM	-0.31	-1.96	0.06
	MPS-PS	0.32	2.25	0.03
	MPS-P	0.36	2.66	0.01
	SGS	-0.10	-0.70	0.50
	RQ	0.14	0.97	0.34

The first step in the analysis reveals that anxiety was a significant predictor of levels of obsessionality, and accounted for a large proportion of the variance as indicated by the significant Adjusted R Square value. At the second point of entry in the analysis, two perfectionism dimensions (Parental Expectations/Criticism and Personal Standards) were the only further variables which made a significant contribution to the prediction of obsessionality.

Discussion

This study explored the role of perfectionism, guilt and responsibility in obsessive-compulsive disorder. The findings supported the hypothesis that some aspects of perfectionism are related to obsessional symptoms, but the results suggested that guilt and responsibility may not be distinctive features of the condition. The three traits will be addressed in turn, followed by a discussion of methodological problems, implications for cognitive models of OCD, and recommendations for future research.

The results of the present study are consistent with reports in the literature suggesting that there is a relationship between OCD and perfectionism. However, this relationship is not a straightforward one. When comparing clinical groups, OCD patients did not differ significantly from anxiety patients on this trait (the significant difference on the one subscale Doubt about Actions is disregarded due to major confound problems identified in the introduction). The levels of perfectionism in the two clinical groups are very similar to those obtained by Frost and Steketee (1997) in their study of perfectionism which also found no differences between an OCD sample and an anxiety sample, but much higher levels in both groups compared to community controls. Thus the OCD patients here appeared to suffer excessively high levels of perfectionism, but so did the patients with other anxiety disorders.

The correlation analyses allowed a closer look at the relationship between traits without the problems associated with comparing clinical groups, and found that perfectionism was correlated with obsessionality, even after controlling for anxiety and depression. The MPS subscales which were related to obsessionality were parental criticism, parental expectations and personal standards. The regression analysis confirmed that these subscales did indeed account for a significant amount of variance in levels of obsessionality. These findings build on previous studies which showed similar patterns of relationships (Frost et al., 1994; Gershuny and Sher, 1995; Rhéaume et al., 1995). These studies all showed a significant relationship between the MPS subscales of parental expectations, criticism and personal standards and measures of obsessionality. However, they also found that concern over mistakes was significantly related, which was *not* the case in the present study. Most studies of perfectionism utilise non-clinical samples and omit controls for mood state. The present study was careful to address these issues, and thus the findings reported here represent a useful addition to the evidence for a specific role for perfectionism in OCD.

The results on perfectionism support McFall and Wollerheim's model of OCD which emphasises the role of this trait in the development of the disorder. Furthermore, the pattern of significant MPS subscales obtained here might reveal clues about the particular form which perfectionism may take in obsessive-compulsive problems. Rachman (1997) has argued that people with OCD suffer "moral perfectionism" whereby a strict and rigidly moralistic upbringing leads to excessively harsh self-judgement. Tallis et al (1996) use the term "moral sensitivity" in discussing obsessional traits which, whilst not exclusively relating to perfectionism, again points to a concern for high standards along a moral dimension. Thus perfectionism in OCD might be more to do with moral standards than with concern over, say, achievement. The cluster of personal standards, parental expectations and parental criticism fits with Rachman's observations that people who have developed a “tender conscience”, perhaps through a morally strict upbringing, are prone to obsessional experiences (Rachman and Hodgson, 1980; Rachman, 1998). There is. of course, only limited evidence to support the notion of critical or perfectionist parenting in OCD patients, but these findings suggest that this parental influence aspect of perfectionism may be worth further exploration. The finding that personal standards rather than concern about mistakes is related to obsessionality supports the idea that *self-referent* as opposed to socially-referent perfectionism is of most relevance to OCD. Rhéaume et al. (1995) have proposed that it is the evaluation made by oneself that is the ultimate criterion for judging one’s actions against one’s perfectionist beliefs. They argue that few measures of perfectionism capture this aspect of the construct, although the present study suggests that the MPS Personal Standards subscale may tap into this to some degree.

Despite the importance placed on guilt in cognitive models of OCD (Rosen, 1975; Rachman, 1993) this trait did not appear to be related to obsessionality in the present study. When considering the two clinical groups, there was no significant difference in levels of guilt when comparing OCD patients with anxiety patients. The correlation analyses also failed to show an association between scores on the Situational Guilt Scale and the MOCI measure of obsessionality. The scores obtained by the OCD patients are very close to those reported by Frost et al. (1994) for their group of "subclinical obsessive compulsives" and higher than the norms reported in this and other papers. However, the anxiety patients, though showing a trend to score lower on the SGS, did not differ significantly, and so it seems that this study indicates pathological levels of guilt in all patients but no specific relationship with obsessive-compulsive symptoms.

As other researchers have noted (Steketee et al., 1991), it is possible that the measure of guilt utilised in this study may not have adequately captured the type of guilt experienced by OCD sufferers. Obsessional guilt may have a different focus to normative guilt, for example it may particularly relate to breaking personal or idiosyncratic moral rules. Ellis (1994) and Clark and Claybourn (1997) have both commented on the idiosyncratic nature of OCD patients' constructs relating to personal failings. Clark and Claybourn point out that it is the interpretation of obsessional thoughts in terms of "what the thought content may mean about one's personal character" that differentiates obsessions from worry (1997, p. 1141). Some authors have suggested that a guilt scale by Mosher (1966) might get closer to the type of guilt in OCD, since it contains subscales measuring Sex Guilt, Hostile Guilt and Morality-Conscience Guilt. However, this scale was not used in this study as it is not well validated or widely used. The majority of research into guilt in OCD have utilised the SGS which allows comparision across studies. Clearly refinement of guilt measurement in obsessive-compulsive problems and a closer analysis of the precise characteristics of this trait are urgently needed before conclusions are drawn about the role of guilt in OCD.

The findings of this study did not support Salkovskis' responsibility model. Levels of responsibility did not differentiate OCD patients from anxiety patients. Neither did the correlation analyses reveal any specific relationship between responsibility and obsessionality. Because this study did not include a normal control group it is difficult to determine how 'excessive' the patients' responsibility ratings were. Both clinical groups scored several points higher than the normal group used by Rhéaume and colleagues in their standardisation of the RQ (Rhéaume et al., 1995). This indicates that both OCD and anxiety patients may have pathological levels of responsibility, but there is still no evidence to support Salkovskis' proposal that the trait distinguishes OCD from other disorders. Indeed, some researchers have proposed that excessive responsibility is a feature of some other anxiety problems. For example, Abramowitz and Foa (1998) found that the comorbidity of generalised anxiety disorder with OCD was associated with higher levels of pathological responsibility than in OCD alone. The present study was unable to address this issue of the relationship of responsibility to specific anxiety problems, because of the small numbers of particular diagnoses within the anxiety disorder group, but there is clearly an urgent need for empirical studies of responsibility in a range of clinical problems, not just OCD.

However, there are alternative reasons for the lack of an association between responsibility and OCD in this study. Firstly, much of the literature on responsibility in OCD is concerned with theoretical ideas and clinical observations. There is little research on operationalising the concept of excessive responsibility and its expression in obsessive-compulsive problems. There is no one agreed definition of inflated or excessive responsibility and no generally accepted standardised measures. Researchers have typically used their own variation of Salkovskis' description of responsibility and developed scales accordingly. Thus, some researchers (Purdon and Clark, 1994) have argued that in order to truly assess the significance of responsibility in OCD, much research is still required to establish the defining characteristics and ways of capturing and measuring the trait in both normal and clinical populations. Lopatka and Rachman (1995) comment that we need to establish "whether perceived responsibility approaches a stable, psychological trait, or whether it is better construed as a situationally-specific reaction" (p. 683). The RQ, used in the present study, was the best available at the time, with some evidence of reliability and validity and a theoretical underpinning which related to Salkovskis' concept of responsibility. However, the questionnaire had many limitations, not least its very limited use in published empirical studies and lack of data on clinical samples. The scale is cumbersome and was found by some participants to be difficult to understand. This suggests that caution is required when considering the interpretation of this scale in the present study.

As other commentators have noted, the role of responsibility in OCD is likely to be a complex one in which particular forms of the trait interact with other factors such as guilt and perfectionism, to form a constellation of cognitive traits. It may be that only in combination with other traits does responsibility become a potent factor in obsessional problems. Tallis (1995), for example notes that the interplay between inflated responsibility, associated guilt and high moral standards may be important in cognitive models of OCD. Salkovskis' apparently simple model, which positions responsibility as the central and distinctive feature in OCD, does not seem adequate to explain the emerging empirical findings which suggest a more complex and multi-faceted picture of cognitive biases in this disorder. Clearly, from clinical observations, responsibility is a potent factor in the presentation of some patients with OCD. However, as O'Kearney has noted; 'it is important both clinically and theoretically, then, that the recent attention paid to "feelings of responsibility".... do not obscure the contribution of other psychological factors to the etiology and maintainance (*sic*) of obsessions.' (O'Kearney, 1993, p.364).

In considering the present study, there are some general design and methodological issues which present limitations to the confidence that can be placed in the findings. The study is cross-sectional and as such does not enter the debate about the etiology of OCD. More importantly the clinical samples were not formally randomised, and participants were not subject to a structured standardised interview to confirm diagnosis. However, patients were not simply allocated a diagnosis by self-report, which is a weakness of some cognitive studies, but had had at least one independent clinical assessment by an experienced professional to ascertain their condition. A key difficulty with community clinical studies is access to sufficient numbers of relevant patients. In the present study the sample size limits the power of the statistical analyses, although the numbers of patients included compares favourably with much published research in clinical psychology.

The mainstay of this research was the use of self-report questionnaires, a valid quantitative method but not without its drawbacks. Firstly, whilst all the scales used here had good face validity and reliability, their construct validity is less certain. Some of the questionnaires such as the Hospital Anxiety and Depression Scale and MOCI have been well standardised and show good correlations with other measures of these mood states. Others like the Responsibility Questionnaire, have not been analysed with regard to construct validity. Thus, one has to be cautious about the extent to which these cognitive questionnaires are truly capturing the trait of interest. As Antony et al. (1998) point out with regard to perfectionism, there is a need for research into *behavioural* correlates of cognitive traits which would allow us to go beyond self-report measures in exploring the way people's thinking relates to their affect and behaviour.

A key limitation in the present study was the heterogeneity inherent in the two clinical groups. Heterogeneity limits the generalisability of the results to specific clinical populations and reduces power in the analysis. Here, the OCD group contained patients with varying profiles of the disorder, including predominantly checkers, predominantly washers and some with obsessions only. Some researchers argue that these are distinctive subtypes within OCD (Steketee et al. 1984, Rachman, 1993, 1997) and that each subtype may differ with respect to their etiology and function (Hodgson and Rachman, 1980).Other studies challenge this idea by pointing out the vast amount of crossover in OCD symptomology, with the majority of patients showing more than one type of obsession. Rasmussen and Tsuang (1986) argue that since many checkers become cleaners and vice versa, a common underlying manifestation for these diverse symptoms is more likely. This provides support for using a patient sample with mixed types of OCD, as in the present study. Looking at responsibility specifically, Salkovskis argues that people with checking rituals will tend to suffer high levels of responsibility and anxiety whilst washers would tend more to guilt and depression (1997, personal communication). The picture is confused since other experts in the field have focused more on pure obsessions and appear to apply the responsibility model specifically to this subtype (Rachman, 1997, 1998). The possibility that different types of OCD profile may display different types of cognitive bias remains unresolved. The research published so far does not allow conclusions to be made concerning what sort of OCD sample would be most appropriate when examining different cognitive biases. However, the heterogenous nature of the OCD in this study does mean that associations between specific types of OCD problem and particular appraisal styles may have been obscured. It is evident that research with larger samples of OCD patients, with careful analysis of particular symptom profiles would contribute greatly to clarifying this confusion.

A similar problem of clinical heterogeneity applies to the anxiety disorder sample in the present study. A mixture of anxiety diagnoses was included which, whilst mirroring much of the other research in this field, is clearly not ideal when trying to tease out possible relationships between different styles of thinking which may manifest themselves differently in different disorders (Riskind, 1997). The traits studied here have all been claimed to be distinctive of OCD and so should still differentiate between OCD patients and any other anxiety patients. Of course the clinical assessment of each patient, established that no participant in the anxiety disorders group also had a diagnosis of OCD. However, there is little research to establish the level of obsessional problems which might be found in other anxiety disorders.Whilst the anxiety disorders group in this study was significantly less obsessional than the OCD group there were clearly *some* obsessional features reported, since the anxiety disorders group did not score zero on the MOCI questionnaire. Differences in cognitive style and their relationship to obsessional problems may have been more apparent if homogenous groups of anxiety problems were considered separately. This would also allow a more detailed analysis of the extent to which different types of anxiety disorder might be more strongly linked to particular appraisal biases. From the research literature reviewd in this study it is clear that some appraisal biases are found across the whole range of anxiety disorders. For example, it is expected that any clincal anxiety group might show higher than normal levels of perfectionism. Antony and colleagues (1998), found higher levels of perfectionism in patients with social phobia and panic disorder compared to non-anxious controls. Larger groups of homogenous anxiety problems would have allowed a closer look at patterns of cognitive bias, and showed more clearly the *degree* to which different types of disorder exhibit the appraisal biases of interest here. As a final point on limitations of this study's design, a 'normal' control group, whilst not necessary in terms of the specific hypotheses being tested, would have provided useful additional information about the severity of symptoms and cognitive distortions in the clinical groups as compared to a non-clinical population.

Overall, the present study lends only limited support to a cognitive model of OCD such as that of McFall and Wollersheim and provided no evidence to support Salkovskis' model. This mixed picture reflects the general status of research in this field where the enthusiasm for cognitive theory is not yet matched by empirical evidence. Intuitively, and from clinical observations, styles of thinking in OCD are powerful mediating factors in the expression of the disorder. However, there is little consensus in the literature about what features are of prime importance in a model of the maintenance of the symptoms; rather we have several categories of possible contenders for key position, three of which, perfectionism, guilt and responsibility were investigated in the present study. There is clearly a pressing need for more empirical research into cognitive factors, with more emphasis on the details of the processes and phenomenology underlying OCD. Some of the current research, including the present study, provides some clues as to what might be future areas to focus in on within the field. Certainly researchers are arguing for more emphasis on the *meaning* attached by OCD sufferers to certain concepts like perfectionism, responsibility or guilt, meanings which may go beyond the usual definitions ascribed to these terms and used in standard questionnaires. Such an analysis might be conceptualised as addressing the difference between 'hot' and 'cold' cognitions. Teasdale (1993), in his theory of cognition and emotion, distinguishes between specific and more holistic levels of meaning ('hot' or holistic level meanings are exemplified by our use of poetry and metaphor, and are claimed to be of primary importance in emotion production). Whilst Teasdale pays particular attention to depression, the model appears equally relevant to OCD, where sufferers so commonly complain that they 'know' that they are not particularly bad or immoral, but their intrusive thoughts lead them to feel evil or malevolent to a most profound degree. The theme of moral sensitivity and malevolence repeatedly comes up in the literature and is only obliquely captured by traits such as perfectionism, guilt and responsibility. The paradox faced by OCD sufferers of a sense of enormous power to do harm coupled with an exquisitely sensitive conscience might be a worthwhile focus for future research.

As part of the increasing interest in applying cognitive theory to OCD, many authors have written on the subject of cognitive *treatments* for this disorder. As Salkovskis and others have pointed out (Salkovskis, 1996; James and Blackburn, 1995), behavioural therapy is successful for many OCD sufferers but relapse and drop-out rates are high. Cognitive therapy was regarded by Salkovskis as a potentially powerful adjunct to behaviour therapy which might address these treatment failures and improve the overall efficacy of psychological treatments. Several writers have gone on to recommend specific cognitive techniques, and dependent on which cognitive biases the writers give pre-eminence to, the treatment protocol will emphasis tackling that particular style of dysfunctional thinking. So for example, Salkovskis (1998) advises challenging ideas about pivotal responsibility, whilst Jones and Menzies (1997a) suggest targeting danger-related cognitions. Treatment studies in themselves are not a direct test of cognitive theories of OCD, but of course if cognitive techniques were shown to enhance the success rates of behavioural treatments of OCD, then this would be powerful supporting evidence for the role of cognitive factors in the disorder. Unfortunately, there are few controlled studies of cognitive therapy with OCD, and these show little evidence of significant improvement when cognitive treatments are added to existing behavioural techniques (James and Blackburn, 1995). That is not to say that cognitive factors are unimportant in the treatment of OCD, more that well-designed studies are urgently needed to address this issue. Thus the value of cognitive therapy for OCD still cannot be judged, and the treatment research does not yet offer support for cognitive models of OCD.

As this present study confirms, our understanding of cognitive processes in obsessive-compulsive disorder is very limited. It is quite clear that much more empirical research is urgently needed to explore what cognitive features operate in OCD. However, just as importantly, the research in this area needs to be rigorously aligned to good theory-building. The rapid rise of cognitive theory in explaining psychological distress has been attacked by many scientists for its apparent lack of a sound scientific base. As far back as 1978, Wolpe argued that cognitive formulations add little to an understanding and treatment of psychological disorders. More contemporary writers have also questioned the scientific basis of cognitive theory. Teasdale (1993), in a paper which challenges Beck's cognitive model, quotes Ross (1991) in arguing that cognitive therapists employ "neither the researchers' methods or their principles. What they do employ is some of the terminology of cognitive psychology.....Such terms are usually called on to serve as *post hoc* rationalisations when *ad hoc* clinical procedures seem to have worked." (Ross, 1991, p.743). Such challenges have, of course been hotly debated and refuted by researchers in the field (Beck, 1979). However, the point being made here is that if researchers wish to apply cognitive models to OCD, the search for cognitive biases and dysfunctional beliefs needs to be underpinned and driven by scientific theory as well as by clinical observation and intuition. The present study made a small step towards this by relating the literature and empirical findings to two existing models of cognition in OCD. However, for real progress to be made in our understanding of this field, further refinement of research, in terms of developing hypotheses and better methodology, is required.

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MATERIAL REDACTED AT REQUEST OF UNIVERSITY

THE DEVELOPMENT OF A THEORY OF MIND IN AUTISM:

A SEVEN YEAR FOLLOW-UP

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ABSTRACT

THIRTEEN AUTISTIC SUBJECTS WHO WERE ORIGINALLY GIVEN TESTS OF FIRST-ORDER AND SECOND-ORDER BELIEF ATTRIBUTION ABILITY IN ORDER TO ASSESS THEIR THEORY OF MIND, WERE RE-TESTED IN 1990, SEVEN YEARS LATER. THE PURPOSE OF THE FOLLOW-UP WAS TO ESTABLISH IF THERE IS ANY DEVELOPMENT IN THEORY OF MIND ABILITY WITH TIME. IN ADDITION, INFORMATION ABOUT THE SUBJECTS' CURRENT COGNITIVE FUNCTIONING WAS OBTAINED IN ORDER TO EXPLORE THE RELATIONSHIP BETWEEN MENTAL AGE VARIABLES AND A THEORY OF MIND.

THIS STUDY CONFIRMED THE PRESENCE OF A SEVERE DELAY IN ALL SUBJECTS, WITH NO OVERALL IMPROVEMENT IN THEORY OF MIND ABILITY OVER SEVEN YEARS. AS FOUND IN 1983, NO SUBJECTS WERE ABLE TO MAKE SECOND-ORDER BELIEF ATTRIBUTIONS. CHRONOLOGICAL AGE AND NON-VERBAL MENTAL AGE WERE NOT RELATED TO ABILITY TO PASS THE THEORY OF MIND TESTS, BUT VERBAL MENTAL AGE WAS AN IMPORTANT FACTOR IN AN ABILITY TO MAKE FIRST-ORDER BELIEF ATTRIBUTIONS.

CONTENTS

LIST OF TABLES

INTRODUCTION

The Autistic Syndrome

Since Kanner (1943) first described the characteristics of autistic children over forty years ago, there has been a wealth of research into the diagnosis, understanding and treatment of autism. At the same time, however, there has been much controversy over the issues of validity and definition. While some psychiatrists did not accept that autism was a distinctive syndrome, others applied the diagnosis far too widely, and yet others interpreted Kanner's summary of the features of this syndrome far too narrowly (Wing, 1978). To add to this confusion, for many years, there was a continuing debate as to how far autism was meaningfully different from such conditions as childhood schizophrenia, general mental retardation and specific developmental disorders of speech and language (Rutter, 1978; Rutter and Gould, 1985). Much of this controversy arises from the fact that the abnormalities of behaviour and impairment of function that may be seen in autism and related conditions are so numerous and so varied that a great deal of research was needed to pinpoint those behaviours that constitute specific and necessary features of autism.

During the 1970s, considerable progress was made in describing the taxonomy of the disorder. Rutter, in his discussion of the evidence (Rutter, 1978), concluded that to avoid further ambiguity, investigators should adopt the following criteria to define autism:-

1. Onset before the age of 30 months
2. Impaired social development which has a number of special characteristics and is out of keeping with the child's intellectual development.
3. Delayed and deviant language development which also has certain defined features and which is out of keeping with the child's intellectual level.
4. Insistence on sameness, as shown by stereotyped play patterns, abnormal preoccupation or resistance to change.

These diagnostic criteria have been widely adopted and were incorporated in the third edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-III) published by the American Psychiatric Association in 1980, and revised as DSM-III-R in 1987. Here autism is classified as an instance of a Pervasive Developmental Disorder.

Primary Deficits in Autism

While a clear consensus about the validity of the autistic disorder has emerged, and important advances have been made in diagnosis, it was still necessary to understand more about the underlying pathology of the disorder. Further research was needed into the defining characteristics of autism, that is, the primary or core deficits which are specific to the disorder. Researchers were, of course, also attempting to understand the course of the disorder.

As mentioned earlier, there is immense individual variation in children diagnosed as autistic. The boundaries are unclear and the disorder overlaps with or co-occurs with other handicaps (Bishop, 1989). Wing (1988) has argued for the concept of an *autistic continuum* to account for the variation. This concept is largely due to the epidemiological studies of Wing and Gould

(1979), and is based on the empirical finding of a common and invariant constellation of three features in autism, regardless of additional handicap. This constellation has become known as 'Wing's Triad', and is widely regarded as an accurate description of the core features of autism. The Triad comprises:-

1. Social impairment
2. Communicative impairment
3. Impairment of imaginative activity, with substitution of repetitive activity

These core impairments give rise to different kinds of behaviour at different ages and at different levels of ability. They will be described in more detail here.

Social Impairment

Social disabilities are increasingly recognised as a major defining characteristic of autism, and they appear to be qualitatively different to social disturbances in other disorders.

Social abnormalities are evident early in life with deviant gaze behaviour, eye contact and facial recognition, inappropriate affectionate behaviour and impaired attachments (Rutter, 1978; Wing, 1976; Volkmar, 1987). In older children, the abnormalities are reflected in a basic failure to form normal social relationships. This is seen in their lack of response to others people's emotions, rare use of appropriate greetings, no peer friendships, inability to modify their behaviour according to social situations and inability to use posture, gesture and facial expression to regulate social interactions (Rutter and Schopler, 1987). Wing and Gould (1979) have characterised three forms of social deviance: aloof, passive, and active-but-odd, which show how patterns or clusters of social impairments may exist in autistic individuals. Social difficulties persist into adulthood even with higher functioning autistic individuals (Szatmari et al, 1989).

Experimental research into social understanding provides evidence of a wide range of deficits in this field. Autistic children show a striking lack of ability in conceptual role-taking (Baron-Cohen, Leslie and Frith, 1985; Dawson and Fernald, 1987), imitation of abstract gestures (Bartak, Rutter and Cox, 1975), and comprehension of emotions (Hobson, 1986a, 1985b). These experimental studies have consistently shown that the deficits lie in the realm of social cognition and in particular, awareness of other people's thoughts, beliefs and feelings.

In summary, these studies document a chronic social deficit in autism, and point to a persistent inability to participate in two-way reciprocal social interaction.

Communication Impairment

It has long been recognised that deviant language is one of the hallmarks of autism. While some have viewed the deficits as simply a product of other aspects of the syndrome (Bettelheim, 1967), many investigators regard language impairment as a core or primary symptom of autism (Paul, 1987; Rutter and Schopler, 1987).

However, through numerous studies of autistic individuals' language, it has emerged that, rather than a general language delay, there is a striking developmental asynchrony between form and function (Tager-Flusberg, 1989). In other words, the acquisition of the phonological system, of words and their meanings and of grammatical forms do not prove to be fundamental problems specific to autism. Instead, there appears to be a basic deficit in the use of language for social communication, also described as a *pragmatic deficit* (Tager-Flusberg, 1985; Baron-Cohen, 1988). The communication abnormalities including such deficits as a lack of pointing, showing or turn-taking, both before and after language acquisition (Ricks & Wing, 1975; Bartak, Rutter and Cox, 1975). Infant communication appears to be limited to functions that seem not to require joint attention and reference (Mundy et. al., 1987; Mundy & Sigman, 1989). Older autistic children show impaired ability to adapt to discourse roles, to follow politeness conventions in conversation and to distinguish given from new information (Baltaxe, 1977). Higher functioning autistic children often have a pedantic formal style of speech and show difficulties in judging appropriate amounts of information to include in an utterance (Paul and Feldman, 1984). Prosodic deficits (the use of intonational cues) are also frequently cited (Fay & Schuler, 1980).

The literature on pragmatic deficits in autism presents a consistent picture of severely impaired functioning on almost all aspects that have been tested. It is the social use of communication that is primary in the communication impairment in autism.

Impairment of Imaginative Activity

This third feature of Wing's Triad refers essentially to the fact that autistic children frequently show impoverished pretend or symbolic play (Wing et al., 1977). They may be able to manipulate objects and show functional use of playthings, but true symbolic play which typically develops during the end of the second year of life, is noticeably absent. Instead, autistic children tend to engage in repetitive, stereotyped activities (Gould, 1982).

The immature play observed in autistic individuals can be understood in terms of a general deficit in symbolic thought and, as such, has close conceptual links with the deficits in social understanding and communication which seem also to suffer from an inability to think abstractly and interpretatively about situations. However, the lack of imaginative activity remains a distinct and primary abnormality in its own right.

Theoretical Accounts of Autism

Throughout the 40 years since Kanner's original diagnostic observations, efforts have been made to develop comprehensive models of the psychological processes underlying the autistic disorder, and a number of very different hypotheses about the nature of autism have been proposed.

Bettelheim (1967) claimed that parental rejection was at the heart of autism, while Tinbergen and Tinbergen (1983) suggested that a fear of social contact was to blame. Current evidence does not support either of these concepts for, while abnormalities in rearing can lead to serious social problems, the nature of the social abnormalities in such cases differs markedly from those found in autism.

At one time, it was thought that autism was primarily a language disorder (Rutter, Bartak and Newman, 1971), but, again, although autism does seem to be very similar to the more severe developmental disorders of receptive language, it is also markedly different in a number of ways (Rutter, 1979), these being the core impairments associated with autism, described earlier.

Autism differs in so many ways from most emotional and behavioural disorders of childhood that its distinctiveness is undisputed. More recent theoretical models have sought to address that distinctiveness, and provide parsimonious explanations of the social, pragmatic, imaginative deficits characteristic of autism.

It is now agreed by most investigators that biological factors are involved in the pathogenesis of the disorder (Coleman & Gillberg, 1985) and research will continue to address the issue of localising brain dysfunction in autism (Baron-Cohen, 1990). At the same time, theoretical models must attempt to specify and account for the psychological mechanisms that represent that original dysfunction. Two such models are described here in more detail.

The Affective Theory

This theory proposes that the social and communicative deficits are primarily affective. A detailed version of this theory has come from Hobson who states that,

> "on a number of levels, autism should be counted a disorder of affective and social relations and irreducibly so." (Hobson, 1989)

He proposes that in autism, there is an innate inability to interact emotionally with others, and that, while in normal infants biological prewiring allows them to be sensitive to and comprehend directly without inference the emotions of other people, autistic infants lack this "non-inferential empathy".

Hobson goes on to propose that the development of a symbolic capacity and of a conceptual role-taking ability are both directly derived from the infant's affective relationships, and the theory thus claims to account for the social, pragmatic and symbolic deficits in autism.

However, there are a number of difficulties with this theory. There is, for example, evidence that emotion-recognition deficits are not specific to autism but found in people with mental handicap too (Ozonoff et. al., in press), and some emotional responsivity is frequently present in autism (Volkmar, 1987). Also, it is difficult to see (and Hobson is unable to specify) how a child's sophisticated concepts of people's mental states, such as their thoughts and symbolising ability, would derive directly from a primitive sensitivity to others' emotions.

The Meta-Representation Theory and Theory of Mind

Refinements in our understanding of the precise nature of autistic deficits led some researchers to suggest that these impairments had a primarily cognitive basis. The types of communicative, social and imaginative deficits were those which seem to be linked in their reliance on the capacity to conceptualise other people's mental states and to abstract and use symbols. Baron-Cohen, Leslie & Frith, (1985), put forward a cognitive explanation for the pathogenesis of the autistic disorder. They suggested that the basic cognitive deficit in autism is the inability to conceive of others mental states and that this stems from a dysfunction in the capacity for *meta-representation* (Leslie, 1987).

Leslie described how, from the outset of development, infants need to be able to perceive and store knowledge about the world and objects in it. This ability to represent or have concepts and beliefs about the physical world is termed primary representation. However, children go on to develop representations not just of objects and events, but also of representations. In other words, they come to gain an explicit concept of representation and a theoretical understanding of how of how the mind relates to the external world. This ability to represent mental representations is termed meta-representation. As Perner (1988) describes it,

> "(a child) has to build a model of the external situation and another, meta-representational model of the mental state, which is itself a model of the external situation." (Perner, 1988, p.151)

Meta-representational ability allows the child to attribute to other people mental states such as beliefs, desires and knowledge, that is, cognitive or volitional states, and not simply emotional or perceptual states. This then allows the child to make sense of people's behaviour. If a child understands that people act not according to the situation but according to their mental representation of the situation, they can then not only predict likely behaviour, but also appreciate such concepts as pretence or false belief which involve strange or aberrant behaviour and which might otherwise be highly perplexing. This aspect of meta-representation has been called a *theory of mind*. Premack and Woodruff first used this phrase and define it as follows;

> "In saying that an individual has a theory of mind, we mean that the individual imputes mental states to himself and to others... A system of inferences of this kind is properly viewed as a theory, first, because such states

> are not directly observable, and second, because the system can be used to make predictions, specifically, about the behaviour of other organisms.... Purpose or intention is the state we impute most widely." (Premack & Woodruff, 1970, p.515).

Leslie and Happe (1989) hypothesise that normal development rests on a 'theory-of-mind module' (Leslie, in press) which is innate and is an 'engine' for the development of later manifestations of theory of mind. Thus very early communicative behaviours such as joint-attention which is evident in infants around the end of the first year, appear to involve meta-representation (Baron-Cohen, 1989a). Pretence, which appears around the end of the second year, can be viewed as a more complex meta-representational-based theory of mind task (Leslie, 1987), while yet more complex abilities such as understanding false belief emerge at about 4 years of age (Wimmer & Perner, 1983).

The developmental stages of a theory of mind are still somewhat speculative. Perner suggests that pretence, for example, is better conceptualised as an instance of a midway stage between primary and secondary representation (Perner, 1988; Baron-Cohen, in press). Others have questioned the formulation of very early behaviours as being meta-representational (Mundy & Sigman, 1989). Despite these controversies, it is clear that a theory of mind is firmly established by the age of about 4 years in normal children.

Baron-Cohen, Leslie and Frith (1985) proposed that autistic children lack meta-representational ability and therefore do not develop a theory of mind. This hypothesis would predict that only those skills requiring meta-representational capacity should be impaired in autism. Most of the core impairments described earlier can indeed be conceptualised in this way.

Social skills such as role-taking and appropriate social interaction require the ability to appreciate other people's mental states. Pragmatic skills would similarly be impaired, for to communicate socially, a speaker must be aware of the listener's mental state, and the listener will hold certain beliefs about the speaker's intentions such as the intention to be informative, relevant, truthful, etc. (Grice, 1975).

The theory of mind hypothesis can also explain why pretend or imaginative play is lacking in autism, for such play it claims, requires an awareness of the mental state 'pretend' (Leslie, 1987; Baron-Cohen, 1987). An impaired meta-representational capacity can, it seem, account for the impairments in Wing's Triad, and is therefore a parsimonious account of the primary deficit in autism (Leslie and Frith, 1990). It is less clear, however, how it might account for some other features of autism such as attention disturbances and arousal self-regulation (Mundy and Sigman, 1989), emotional role-taking (Rutter, 1986), and repetitive behaviours (Boucher, 1989). Several investigators while accepting the importance of a cognitive account of autism, still believe that a primary affective deficit is involved at an early stage as well, and that the cognitive and affective systems interact in an inseparable way (Ungerer, 1989; Paul, 1987). Hermelin and O'Connor (1985) propose such a system which they call the "logico-affective" state. A better understanding of the very earliest deficits in autistic children is required before such differences are resolved (Leslie, 1987).

Experimental Studies of the Theory of Mind Hypothesis

Baron-Cohen, Leslie and Frith (1985) tested their hypothesis directly, using an adaptation of Wimmer and Perner's (1983) test of normal children's understanding of *false belief*. These authors developed an ingenious paradigm to demonstrate that an explicit theory of mind is well within the capacity of the normal four-year-old. They devised a task where the child's own belief is different from someone else's belief. In order to succeed on the task, the child has to be aware that different people can have different beliefs about a situation. Hence, this case provides the strongest evidence for the capacity to conceive of mental states (Dennett, 1978).

This test, hereafter called the Sally-Anne test, is used in the present study and is described in detail in the next section. It involves a story told with two dolls (Sally and Anne), during which an object is transferred while its owner is absent. The inference that has to be made is that this person (doll) will falsely believe the object is still in the old place, since that was where she put it. To pass this test of a theory of mind, the child must attribute a belief to the owner, and this level of ability is termed, "first-order belief attribution". Baron-Cohen et. al., (1985) gave the test to people with autism, as well as to a group of people with

Down's Syndrome, and a group of normal children. They found that whilst 80% of the subjects with Down's Syndrome, and 85% of the normal children passed this test, only 20% of the subjects with autism did so, and this was so despite this group having a higher mental and chronological age than the two comparison groups. Instead, 80% of the subjects with autism indicated the doll would look for her object at the location to where it had been moved.

This study then, lent preliminary support to the hypothesis that in autism, there is a failure to develop a theory of mind. Over the past five years, this pattern of results has been replicated and extended using picture sequencing tasks (Baron-Cohen, Leslie and Frith, 1986), true belief tasks (Leslie and Frith, 1988), appearance versus reality tasks (Baron-Cohen, 1989b), a gift-choosing paradigm (Dawson and Fernald, 1987), tests of other mental states like desire and imagination (Baron-Cohen, in press), and tasks using real people (Perner, Frith, Leslie and Leekam, 1989).

This last study included a test of false belief which used a "deceptive appearance" paradigm, whereby the subject is misled into thinking a Smarties box contains sweets whereas it in fact contains a pencil. The subject then has to predict what someone else would say was in the box when shown it in the same deceptive way. This test is also used in the present study and is referred to as the Smarties Test. Essentially, Perner et. al., (1989) obtained similar results to Baron-Cohen et. al. (1985). However, they did suggest that the Smarties test might be slightly easier than the Sally-Anne test as subjects may be helped in predicting others' response by having experienced the same mistake themselves. This suggestion has not yet been assessed by direct comparison of the tests.

These studies all confirm the existence of a theory of mind deficit in autism. This deficit cannot be attributed to the general effects of mental retardation, or of language problems as the inclusion of high-functioning autistic subjects and /or the use of various matched control groups preclude this.

The Specific Developmental Delay Hypothesis

Experimental studies of theory of mind in autism present remarkably consistent results. Compared to matched controls, autistic individuals have a specific and severe deficit in this area of ability.

However, the impairment is by no means absolute. Several studies show that a small percentage (ranging from 20 to 35%) of high functioning autistic children pass the theory of mind tests. In Baron-Cohen et. al's (1985) study using the Sally-Anne test for example, 20% of subjects with autism were able to make a first-order belief attribution. Leslie and Frith (1988) and Prior, Dahlstrom & Squires (1989) also reported a minority of autistic individuals who were able to pass first-order false belief tasks.

One hypothesis that has been advanced to account for the sub-group of individuals who pass is that there may be a *delay* in the development of a theory of mind. The specific developmental delay hypothesis derives its name first from the evidence that autistic children's impaired theory of mind is not related to *general* developmental delay, and secondly, from the possibility that some older autistic children may eventually develop a theory of mind at the lowest level, many years after it would normally be present.

Baron-Cohen (1989c) investigated this hypothesis further by looking at whether those autistic children who passed a first-order belief attribution test (the Sally-Anne test) were, nevertheless, unable to use a more advanced theory of mind.

Normal 6 - 7 year old children are able to make what are termed "second-order belief attributions" of the form, 'Mary thinks that John thinks the ice-cream van is in the park' (Perner and Wimmer, 1985). Perner and Wimmer's paradigm was used by Baron-Cohen (1989c) to investigate this ability in autistic children, this test being especially useful in distinguishing unambiguously whether a subject is making first or second-order belief attributions. The test involves a story, illustrated with a model village and toy people, about two children's beliefs regarding the whereabouts of an ice-cream van. This was the third test that was is used in the present study and is described in the next section in detail. It is hereafter referred to as the Ice-Cream Van Test.

Baron-Cohen (1989) found that *none* of his sample of high functioning autistic individuals were able to make second-order attributions. In contrast, non-autistic control (Down's Syndrome) children with a lower mental age *were* able to attribute beliefs at this more advanced level. These results, therefore, supported his prediction that these autistic children were specifically delayed in the acquisition of a more complex theory of mind.

Baron-Cohen (in press), investigated this model of a delay further in a series of experiments which tested understanding of five different mental states (i.e. not just belief), in a group of people with autism. Their ability to understand mental states of increasing complexity followed a clear developmental sequence with imagination being easier than pretence which in turn was easier than belief. This study provides further evidence for the delay hypothesis.

Factors in the development of theory of mind:

Investigators have analysed the role of background variables, most notably MA and CA, in the performance of autistic subjects on belief tasks, in an attempt to identify possible contributory factors for the development of a theory of mind.

With respect to CA, Baron-Cohen (1989c), Leslie and Frith (1988), Prior et al. (1989) and Eisenmajer & Prior (1990), all found that subjects who passed first-order belief tests were not significantly older than those who did not. However, these studies did reveal a general tendency for the "passers" to be the older children of the group. Baron-Cohen (1989c) has suggested that CA is a necessary factor but is not a sufficient condition to account for those subjects with autism who pass. In these studies cited above, no child below the age of 8 years 7 months was able to pass a false belief task.

The evidence regarding MA is similar to that for CA. Although non-verbal MA does not appear to be linked to theory of mind ability, the same studies mentioned above found that verbal MA was an important factor for success on the belief tasks. While Baron-Cohen (1989c), Leslie and Frith (1988) and Prior et. al. (1989) did not find a significant difference in verbal MA between "passers" and "failers", they did show a trend of higher verbal MA being necessary for passing. Eisenmajer and Prior's (1990) study was the first to report a significantly higher verbal MA in those subjects who passed the false belief task. However, as with CA, verbal MA is not sufficient to account for passing, in that all the studies describe children of relatively high verbal MA who were unable to pass the tasks.

Limits of theory of mind development in autism:

The majority of autistic children are unable to make first order belief attributions. The developmental delay hypothesis allows for the possibility that these individuals might progress in some way to enable them to succeed later on. The only published study so far to look at advanced theory of mind (Baron-Cohen, 1989c), found that no subjects were able to make second-order belief attributions, even those with relatively high verbal MA and CA. This raises the question of whether there exists a *ceiling level* of ability beyond which no individual with autism will develop. It is too early to draw this conclusion as there have not been enough studies of second-order belief attribution, and there is also a lack of research into the theory of mind in autistic adults. However, Harris (1989) has postulated that the clinical picture will best be explained through the notion of a delay superimposed on neurological damage. Thus, *even once started*, progress will be slower than in normal children, with an increasingly marked gap with age.

In addition, Bowler (in press), in a study of second-order beliefs in individuals with Asperger Syndrome, found that 73% of this group did pass the Ice-Cream Van test. The subjects had much higher non-verbal and verbal MA's and were adults with an average CA of 26.67. If, as some researchers suggest (Wing 1981), Asperger Syndrome shares the same underlying dysfunction as autism, this study provides new evidence for the development of advanced theory of mind in the most able autistic subjects. How this finding will be accommodated in the theory of a meta-representational deficit in autism remains unclear.

Aims of the Study

The majority of theory of mind studies have been of a cross-sectional design. As many researchers have noted, questions about the *development* of a theory of mind in autism cannot be properly answered without longitudinal studies (Baron-Cohen, 1989c; Eisenmajer & Prior, 1990; Harris, 1989).

The present study aims to address this problem through a seven year follow-up of the autistic children originally tested in the study by Baron-Cohen, Leslie and Frith (1985). In this study, twenty autistic children between the ages of 6.1 and 16.5 years formed the test sample. They were assessed in 1983 for verbal and non-verbal MA, and for ability to pass the Sally-Anne test. Only 20% (four) of the subjects could make the first-order belief attribution and none of these four were able to pass the Ice-Cream Van test of second-order belief attribution when tested (in 1987) in a separate study (Baron-Cohen, 1989c).

The purpose of the follow-up was to map the changes in the MA and theory of mind ability over seven years through re-administration of the same tests used in 1983. Also, it was decided to collect further information about theory of mind ability by using the Smarties test. On the basis of the recent research reviewed above, several points of particular interest were investigated in this study.

1. The specific developmental delay hypothesis allows for changes in theory of mind ability within individuals, and so one might predict that some autistic children previously failing the Sally-Anne task might later be able to pass. It is in principle possible that those who passed the Sally-Anne test may have developed sufficiently to pass the Ice-Cream Van test.

2. The use of the Smarties test, whilst not able to show change over time, could provide interesting reliability data on first-order belief attribution ability. It is tentatively predicted that those passing the Sally-Anne test should also pass the Smarties test, whilst, if the latter is easier as has been suggested, some subjects may be able to pass this but fail the Sally-Anne test.

3. The relationship between MA and CA and theory of mind ability was also investigated. Research indicates that a minimum CA, and relatively high verbal MA are necessary to pass even first order belief attribution tests, and it was expected that the current study would concur with this

4. It was felt that information about the abilities of teenage and adult autistic individuals would be a useful addition to research in this area. Longitudinal studies of cognitive and social development in autism suggest only limited development in most areas of functioning, particularly social understanding (DeMeyer et al., 1973). Also, while a very few individuals show a marked improvement in adolescence (Kanner, et al., 1972; Szatmari et al., 1989), a substantial number may sadly be expected to deteriorate at or after puberty (Gillberg & Schaumann, 1981; Waterhouse & Fein, 1984).

METHOD

Subjects

Of the twenty autistic children who participated in the 1983 study, seven were untraceable. The subjects who were finally recruited into the present study were ten boys and three girls who had been diagnosed as autistic according to established diagnostic criteria (Rutter, 1978).

The group were assessed for receptive verbal mental age (MA) using the same tests as in 1983. These were the British Picture Vocabulary Scale (BPVS) Long Form (Dunn, Dunn, Whetton & Pintilie, 1982), and non-verbal MA using the Leiter International Performance Scale (Arthur, 1952). Details of these subjects are presented in Table 1, together with their CAs and MAs obtained in the original study in 1983.

It is worth noting here that the cognitive tests used in 1983 were re-administered in the present study for ease of comparisons of performance across time. The BPVS and the Leiter Scale are both for use up to age eighteen, and seven of the subjects were older than this ceiling age. However, as their non-verbal and verbal MAs were expected to lie well below their CA, it was still considered appropriate to apply these tests to all subjects.

TABLE 1: Chronological Age (CA) and Mental Age (MA) of subjects in the experiment

STUDY	n		CA	Verbal MA*	Non-VerbalMA+
1983	13	MEAN	11.3	5.1	8.8
		S.D.	2.8	1.7	1.6
		RANGE	6.5 - 15.8	2.8 - 7.4	5.3 - 10.6
1990	13	MEAN	18.3	6.8	9.9
		S.D.	3.3	3.9	2.8
		RANGE	13.1 - 23.1	2.8 - 15.2	5.8 - 15.1

*BPVS; + Leiter

Details of the seven subjects in the orginal study who were not included in the present experiment are summarised in Table 2.

TABLE 2: CAs and MAs of untraced subjects (1983)

n		CA	Verbal MA*	Non-Verbal MA+
7	MEAN	12.6	5.9	10.0
	S.D.	3.6	1.0	2.0
	RANGE	6.1 - 16.5	5.1 - 7.3	6.6 - 15.7

*BPVS; + Leiter

The subjects who were not traced did not differ significantly in CA (t = 0.90, 13 df), Verbal MA (t = 1.5, 18df), or Non-Verbal MA (t = 1.2, 18 df), from those included in the present study (p   0.05).

All but one of the subjects were attending some form of training centre or special school. Testing was carried out at these centres except in one case, where the testing was done at home.

Since the focus of the present study was on cognitive and social development *within* a group of autistic individuals, no control groups were included. It is clear from Table 1 that despite a seven year change in CA, Ma only increased by, on average, one and a half years. This gives a preliminary idea of how much change over time might be possible.
Nevertheless, mean MAs were over six years which is the age level at which normal children would pass the Ice-Cream Test.

Design

Each subject was tested on one occasion only. The tests were administered in the following order for all subjects: BPVS; Smarties Test; Sally-Anne Test Trial 1; Leiter Scale; Sally-Anne Test Trial 2.
The Ice-cream Van Test (two trials) was administered only if the subject passed one or more trials of the Smarties or Sally-Anne tests.

The Sally-Anne Test and the Ice Cream Van Test are exact replications of the tests administered in 1983. The Smarties Test was not used originally but was included in the design of this study to provide extra information on theory of mind ability.

Test Descriptions

SMARTIES TEST

The experimenter produced a Smarties box from her bag and asked the subject "What's in here?". All the subjects answered with "Smarties" or "sweets". The experimenter opened the box, showing the subject that there was a pencil only inside, and stated, "No, it's a pencil". She put the pencil back into the box, closed the box, and asked two prompt questions:

Reality Prompt: "What's in here?"

Own-Response Prompt: "When I first asked you, what did you say?".

Then the subject was told that his teacher, or careworker, (who was named) would be coming in later to fetch him and was told "S/he hasn't seen this box. When s/he comes in, I'll show her/him this box just like this and ask: "(Name). What's in here?".

Prediction Test: "What will (Name) say?".

Reality Question: "Is that what's really in the box?". (If answer is, "No"): "What is really in the box?".

Own-Response Question: "Do you remember when I took the box out of my bag (experimenter re-enacts that episode) and asked you what was in it, what did you say?".

At the end of the scenario, the subject's responses to the prompt questions, prediction test, and control questions were noted down. A pass on this test was scored if the subject answered both the prediction test and the control questions correctly.

SALLY-ANNE TEST

Materials: Two dolls which differed in hair colour and clothing were used. Both were approximately 25 centimetres in length. Two plastic containers (boxes) were also used, differing in colour and size. This differs slightly from the original scenario which used a basket and a box. A marble was chosen as the object to be hidden.

Procedure: The procedure is illustrated in Figure 1. The two dolls, Sally and Anne, were introduced to the subject, who was then required to name each doll (Naming Question). If the subject failed to remember, s/he was reminded until able to do so correctly.

Sally first placed a marble in her box. Then she left the scene, and the marble was transferred by Anne and hidden in her box. Then, when Sally returned, the experimenter asked the Belief Question: "Where will Sally look for her marble?". If the subject pointed to the previous location of the marble, then s/he passed the Belief Question by appreciating the doll's now false belief. A pass was only scored if two control questions are answered correctly: "Where is the marble really?" (Reality Question), and "Where was the marble in the beginning?" (Memory Question).

A second trial of the Sally-Anne Test was carried out using a new location of the marble. In Trial 2, the experimenter took the marble and placed it in her pocket while Sally was away, so that now there were three different locations that the subject could point at (the two boxes and the pocket).

To ensure that subjects did not unwittingly give a correct answer by favouring a position or giving an echolalic response, the order of the questions were such that no two correct responses using the same word followed one another. On Trial 1, the correct answers were: Sally's box, Anne's box, Sally's box. On Trial 2, they were: Sally's box, pocket, Sally's box.

For half the subjects, Sally was positioned to the subject's left, and for the other subjects, Sally was positioned to the right. This allowed an additional control for position preferences.

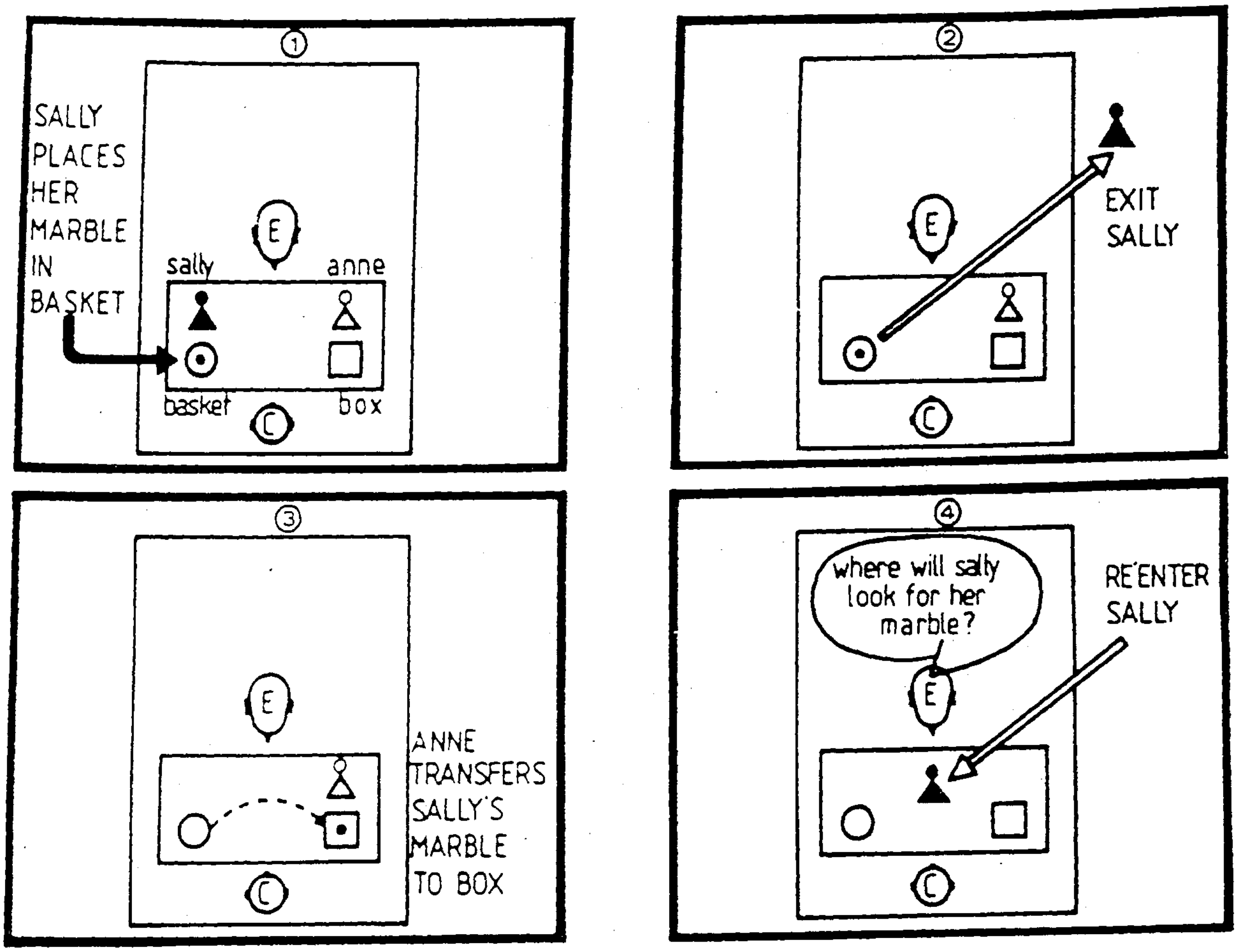


FIGURE 1: Scenario of a simple test of a theory of mind (Reproduced from Cognition, 21, 37 - 46)

ICE-CREAM VAN TEST

Materials: The test employed a toy village which comprised two houses, a church, a fence to separate the park and the road, four 'playpeople' (about 3" high) and an ice-cream van. In addition, there was a row of trees, so that it was not possible for the story characters to 'see' the church or John's house from the park (or vice versa). The buildings were about 5" high. The whole village fitted onto a table-top 2' square; this is shown schematically in the first picture of Figure 2. The subsequent six pictures in Figure 2 depict the experimental scenario. All the materials were commercially available in a children's toy-shop.

Procedure: The experimenter laid out the toy village on the table in front of the subject. The experimenter then told the following story, moving the characters (dolls) and the ice-cream van accordingly:

> This is John and this is Mary, they live in the village.
>
> *Naming question: Which is John/Mary?*
>
> Here they are in the park. Along comes the ice-cream man. John would like to buy an ice-cream but he has left his money at home. He is very sad, "Don't worry" says the ice-cream man, "you can go home and get your money and buy some ice-cream later. I'll be here in the park all afternoon... "Oh, good" says John, "I'll be back in the afternoon to buy an ice-cream".
>
> *Prompt question (1): Where did the ice-cream man say to John he would be all afternoon?*
>
> So John goes home. He lives in this house. Now, the ice-cream man says, "I am going to drive my van to the church to see if I can sell my ice-creams outside there".

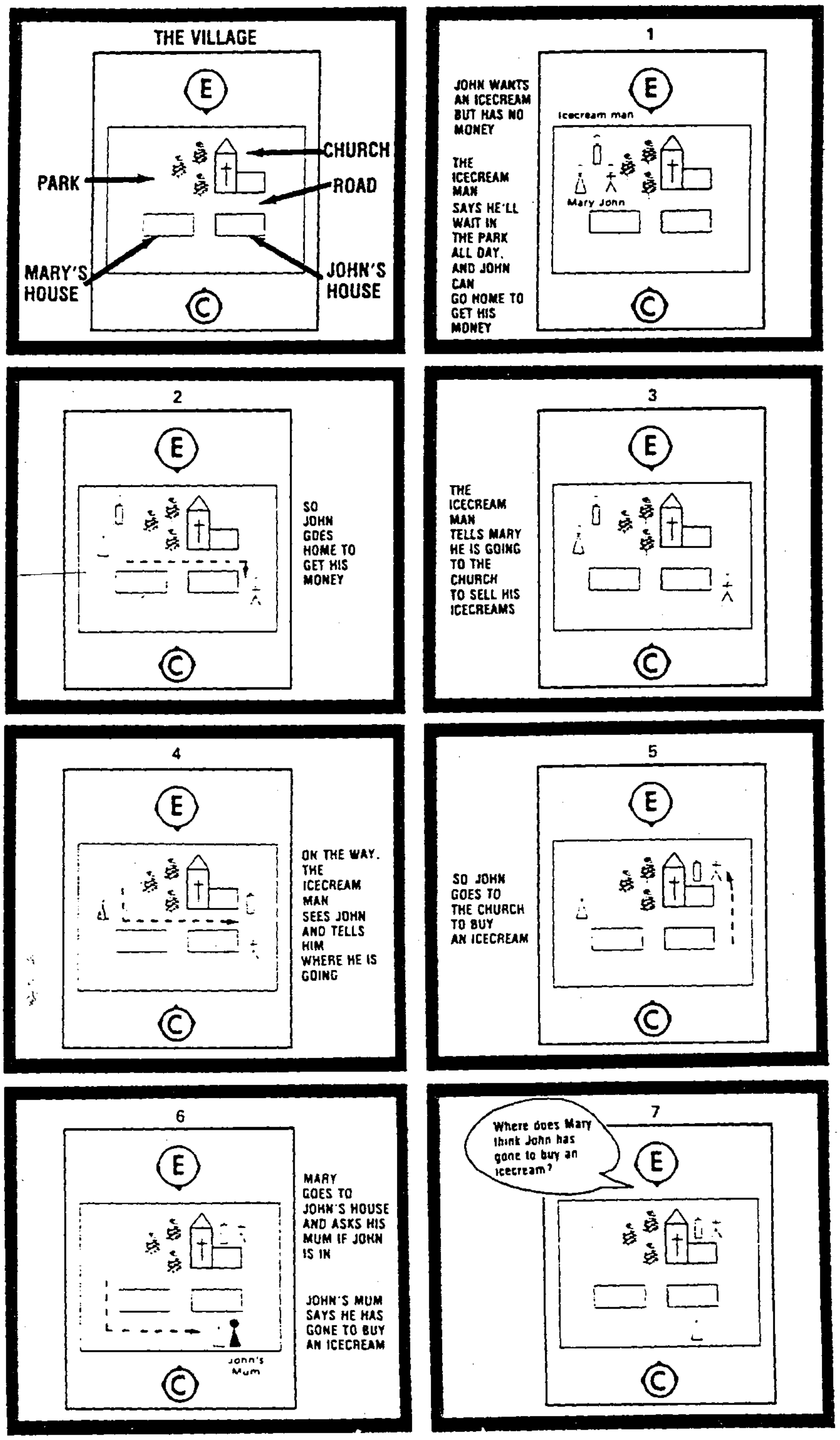
Prompt question (2): Where did the ice-cream man say he was going?
Prompt question (3): Did John hear that?

The ice-cream man drives over to the church. On his way he passes John's house. John sees him and says, "Where are you going?". The ice-cream man says, "I'm going to sell some ice-cream outside the church". So off he drives to the church then John follows to buy an ice-cream. The John follows to buy an ice-cream.
Prompt question (4): Where did the ice-cream man tell John he was going?
Prompt question (5) Does Mary know that the ice-cream man has talked to John?

Now Mary goes home. She lives in this house. Then she goes to John's house. She knocks on the door and says, "Is John in?". "No" says his mother, "he's gone out to buy an ice-cream".
Belief question: Where does Mary think John has gone to buy an ice-cream?
Justification question: Why?
Reality question: Where did John really go to buy his ice-cream?
Memory question: Where was the ice-cream man in the beginning?

This story is shown in schematic form in Figure 2.

FIGURE 2: Scenario of test of second-order belief attribution.
Reproduced from J.Child Psychol.Psychiat. 30(2), 285-297,1989)



At the end of the story, the subject's responses to the five prompt questions, the belief question and the three control questions (the justification, reality and memory questions) were noted down. Then the whole experiment was repeated (Trial 2), this time reversing the locations. The three control questions were necessary to ensure that the subject had both knowledge of the real location of the object and an accurate memory of its previous location, as well as providing evidence of which level belief attribution the subject was making. In the version of the story given above, a pass on the belief question was scored if the child pointed to or said "the park".

RESULTS

SMARTIES TEST

Two (15%) of the thirteen subjects passed the *Smarties Test*. Two subjects answered the control questions correctly but failed on the Prediction Test. The remaining subjects failed on one or more of the control questions and/or the Prediction Test.

SALLY-ANNE TEST

Only one subject (8%) passed both trials of this test, while two subjects (15%) passed on just one of the trials. The remaining subjects failed on one or more of the control questions and/or the Belief question.

ICE-CREAM VAN TEST

The *Ice-Cream Van Test* was administered to those four subjects who passed one or more of the first-order belief attribution tasks (*Smarties*, *Sally-Anne I*, *Sally-Anne II)*. None passed this test, with two subjects failing on the Belief question only and two failing on control questions in addition to the Belief question.

The pattern of performance on the theory of mind tests is summarised in Table 3. The one subject who passed both the *Smarties* and the *Sally-Anne* tests passed both trials in the latter, and hence, was the only subject to consistently pass on all first-order tests.

TABLE 3: Performance on first-order belief attribution tests

		SALLY-ANNE TEST	
		PASS (I and/or II)	FAIL
SMARTIES TEST	PASS	1	1
	FAIL	2	9

CA & MA VARIABLES

The means and standard deviations of "Passers" and "Failers" of the first-order belief attribution tests on CA and MA variables are shown in Table 4. Here, a "Passer" is defined as a subject who passes at least *one trial* of the *Smarties* and *Sally-Anne* tests. On this criterion, four subjects were "Passers" and nine were "Failers".

TABLE 4: Means and standard deviations of CA and MA variables for "Passers" and "Failers".

VARIABLE		PASSERS	FAILERS
CA	Mean	17.7	18.6
	S.D.	3.2	3.4
Verbal Ma	Mean	11.2	4.8
	S.D.	3.4	1.9
Non-verbal MA	Mean	10.7	9.5
	S.D.	1.7	3.2

The minimum CA in the group of "Passers" was 13 years 3 months, while the minimum Verbal MA was 8 years 1 month, and Non-Verbal MA was 8 years 6 months.

"Passers" and "Failers" were compared on CA and MA. No significant differences between "Passers" and "Failers" were found on CA ($t = 0.42$, df 11) or Non-Verbal MA ($t = 0,72$, df 11). Both $p >$ .0.05. However, the two groups did differ significantly on Verbal MA ($t = 4.37$, df 11, $p < 0.002$).

FOLLOW-UP RESULTS: Theory of Mind Tests

In an analysis of the changes over time in theory of mind test performance, only the *Sally-Anne Test* is considered. This is because the *Smarties Test* was not used in the 1983 study, and no subject passed the *Ice-cream Van Test* in 1983 or 1990.

Of the thirteen subjects re-tested in the present study, four had originally passed both trials of the *Sally-Anne Test* in 1983. The remaining nine subjects had failed on both trials.

Of the four subjects who passed in 1983, only one was able to pass both trials on the re-testing; one subject passed just one trial and the remaining two original passers failed on both trials in 1990. In contrast, there was one subject in the present study who did pass the trial of the *Sally-Anne Test* who had previously failed in 1983. These results are summarised in Table 5.

TABLE 5: "Passers" and "Failers" on the Sally-Anne Test in 1983 and 1990.

		1990	
		PASS	FAIL
1983	PASS	2	2
	FAIL	1	8

Using McNemar tests for correlated proportions, performance on the *Sally-Anne Test* in 1983 was compared to that in 1990. No significant differences were found in the proportions passing when using either the original 1983 criterion of a pass (both trials passed), ($z = 1.73$, $p > 0.05$), or the more lenient criterion of a pass on at least one of the trials ($z = -0.58$, $p > 0.05$).

FOLLOW-UP RESULTS: CA and MA variables

Between 1983 and 1990, 69% of subjects showed an increase, and 31% a decrease in Verbal MA. Similarly, 69% of subjects showed an increase, and 31% a decrease, in Non-Verbal MA. No significant difference was found between subjects tested in 1983 and 1990 on mean Non-Verbal MA ($t = 1.91$, df 12). However, there was a significant increase in mean Verbal MA ($t = 2.22$, df 12).

The relationship between *change* in ability of subjects to pass the *Sally-Anne Test* between 1983 and 1990, and MA variables was investigated using Pearson *r* correlations. No significant correlation was found for Verbal MA ($r = 0.45$, df 11, $p > 0.05$) or Non-Verbal MA ($r = 0.17$, df 11, $p > 0.05$).

DISCUSSION

The present experiment confirms findings from other studies that only a small minority of autistic individuals are able to pass tests of a theory of mind. In this study, four subjects (31%) passed a first-order belief attribution task.

However, the present study goes beyond current cross-sectional research to provide longitudinal data on theory of mind ability in autism. When the group's performance on belief tasks in 1983 is compared with that in 1990, it is clear that there has been no obvious development in their ability to pass such tasks, even after seven years, This is evident in comparisons of rates of "Passers" and "Failers" in the two studies, which showed no significant differences overall in either direction.

An analysis of individual performance reveals that for the four subjects who originally passed a first-order belief task (the *Sally-Anne Test*) in 1983, three deteriorated in performance on re-testing and one remained the same. While the majority of subjects failed the tests on both occasions, and therefore showed no change at all in ability, one subject did appear to have improved from failing the *Sally-Anne Test* in 1983 to passing one of the trials on re-testing.

In terms of second-order belief attribution, no subject in 1983 or 1990 was able to pass the *Ice-cream Van Test*. This concurs with previous studies, using the same test, which also found that no autistic subjects passed the higher-order task.

From the results of the present study then, the general pattern of development was as follows. Those subjects who did not pass the *Sally-Anne Test* in 1983 did not improve in their first-order belief attributions. Those subjects who *did* pass the *Sally-Anne Test* originally, did not in the intervening period, develop in the ability to make second-order belief attributions.

Additional information of theory of mind ability was obtained from the results of the *Smarties Test*. It has been suggested that this test may be slightly easier than the *Sally-Anne Test*, though both are still tests of first-order belief attribution, and following a developmental perspective, one would expect that some autistic subjects might be able to pass the *Smarties Test* but not the *Sally-Anne Test*, and that those who passed the latter should also pass the former. These predictions were not supported in this study, there being no clear relation between ability to pass the two tests, and the findings.

The relationship between theory of mind ability and CA and MA variables was investigated in this study. Like other studies, it did not show a significant relationship between CA and ability to pass the theory of mind tests. However, as the autistic subjects in this study were on average much older than subjects in previous studies, it was not possible to draw conclusions about a *minimum* CA which might be necessary for passing theory of mind tests.

Whilst Non-Verbal MA was not significantly related to theory of mind ability, "Passers" and "Failers" were significantly difference on Verbal MA. The "Passers" in the 1990 study had a relatively high mean Verbal MA of 11 years 2 months, and a minimum of 8 years 1 month. This finding agrees with trends reported in many others studies (e.g. Eisenmajer and Prior, 1990), and confirms the importance of Verbal MA in ability to pass theory of mind tests. In this study, the two subjects who passed a first-order test for the first time in 1990 were those who had the largest increases in Verbal MA over the follow-up period, further highlighting the link between Verbal MA and theory of mind.

In terms of *changes* in cognitive functioning over the seven years, results are similar to other studies. A mixed picture emerged whereby some subjects improved markedly on Non-Verbal MA and Verbal MA; others changed very little, and several subjects showed a deterioration on these variables. Overall, however, mean Non-Verbal MA did not change significantly, but the mean Verbal MA improved, with an average increase of 1 year 2 months over the seven year period. Thus the findings generally suggest only very limited development in cognitive functioning, but at the same time do not point to any substantial deterioration as some outcome studies have shown when looking at adolescent and young adult autistic individuals (Waterhouse and Fein, 1984).

A number of issues concerning the notion of a *specific developmental delay* and the pattern of results obtained in this study can now be raised, against the background of information on CA and MA variables discussed above. First, the pattern of results on the theory of mind tests raises the question of whether some subjects show a deterioration in their theory of mind. While the group as a whole performed no worse in 1990 than in 1983, three of the four subjects who originally passed the *Sally-Anne Test*, performed less well on re-testing. Also, a suggestion of deterioration comes from the general inconsistency of performance on the theory of minds tests. Many subjects made errors on both control and belief questions, and those who passed one *Sally-Anne Test* trial did not necessarily pass the other. These findings are contrary to those reported in other studies where passing and failing tended to be on an "all or nothing" basis (Baron-Cohen, 1989c).

The group's failure to develop in their theory of mind ability over time, and the trend towards inconsistent and possibly worsening performance for some subjects, cannot be accounted for by changes in verbal or non-verbal MA since neither or these variables correlated significantly with changes in performance on the theory of mind tasks. Moreover, the increase in mean Verbal MA without a corresponding increase in "Passers" is further evidence for the claim by several researchers that relatively high Verbal MA may be necessary for theory of mind but is not *sufficient*. In all, the *specificity* of the theory of mind delay in autism is confirmed by these findings which show that a theory of mind remains very much independent of levels of general cognitive functioning in adolescence and adulthood.

Poor performance on the theory of mind tests may possibly be the results of anxiety, poor motivation or 'negativism'. These factors could account for some of the inconsistency in performance, but, as the cognitive tests did not appear to be affected (co-operation and concentration being satisfactory for most subjects) such an explanation is inadequate.

The failure to find any subject able to pass the second-order belief attribution test, even after seven years, provides further evidence for the idea that a 'ceiling' of theory of mind ability in autism might exist at the level of first-order attributions. This would partly explain the limited development of theory of mind in this study (but does not, of course, account for the lack of improvement at levels *below* this ceiling). However, it is in principle possible that a much *longer* follow-up might reveal improvements even up to second-order attributions, and further longitudinal research is required to assess this.

Lastly, there are a number of methodological limitations and flaws in the present studies which taken together may account for some of the results obtained, or at least prohibit the drawing of firm conclusions. These limitations of the study are described below.

1. Reliability data on the theory of mind tests used here is not available. Inter-rater and re-test reliabilities are of particular importance in the present study.

2. Hobson and Lee (1989) have questioned the use of the BPVS as a test of receptive language ability. They found that autistic individuals had specific impairments in passing particular BPVS items (such as emotion-related items) and so its use as a benchmark of purely verbal ability may be inappropriate.

3. The investigations of change in all types of performance were limited by the absence of follow-up data on the original matched control groups. It is difficult, for example, to properly assess the significance of the changes in MA without controls for comparison.

4. It is suggested that the Smarties Test may not be as simple as it appears. The own-response question could be interpreted as a Belief task in its own right as it probes awareness of the subject's own mental state. This form of questioning has been used explicitly to test belief. (Baron-Cohen, in press) and may well confound the results of the Smarties Test. Further investigations of the pattern of responses in Smarties Tests studies are required to clarify this.

CONCLUSIONS AND RECOMMENDATIONS

The results of this study are consistent with the hypothesis that autistic individuals, while not altogether lacking a theory of mind, are specifically delayed in acquiring one. The study also provides information concerning theory of mind and cognitive functioning in autistic adolescents and young adults extending the essentially child -oriented research in this area.

The longitudinal data show that development of a theory of mind is severely limited and that the pattern of this development is not clearly related to general cognitive development, although a high Verbal MA appears to be an important factor. The possibility of a 'ceiling' at the level of first-order belief attribution is supported by this study.

It is clear from this research that the type of delay in autism is not one of a simple chronological delay such that, although autistic children might start to develop a theory of mind much later than normal children, they then move in the same direction and with the same rate of progress as normal children. The lag in their understanding of mental states in adolescent autistic individuals is clearly not fixed - with increasing age, the gap becomes more marked as shown in this study. As Harris (1989) has argued, it seems more likely that the impairment in a theory of mind stems from a developmental delay superimposed on initial permanent neurological damage. This would explain the increasing gap between normal and autistic individuals, and the present study supports this extension of the specific developmental delay hypothesis.

Our understanding of theory of mind impairment in autism is rapidly changing as new research continues to appear. From the initial discovery of a primary deficit in the ability to conceive of mental states, a number of increasingly sophisticated models of that impairment have been proposed. The notion of a specific developmental delay has been confirmed by several studies, but the precise nature of the delay and its relationship to normal development remains unclear. The present study is the first to investigate the natural history of a theory of mind in autism through longitudinal research. While the results suggest a gloomy prognosis with regard to long-term development, more comprehensive and methodologically refined research is required before firm predictions can be made about the course of development which a theory of mind is likely to take in autism. With this point in mind, a number of recommendations for future research are presented:-

1. More research is needed into the reliability of current theory of mind tasks, for while their internal and face validity appear to be quite high, studies have not addressed the issues of reliability over time or between different experimenters.

2. More longitudinal research is required to follow the development of a theory of mind over longer periods of time and for a variety of age ranges. In addition, follow-up of the original control groups, absent in the present study, would provide important comparative data.

3. Follow-up studies of meta-representational abilities other than understanding belief are required to properly evaluate the delay hypothesis. Longitudinal studies of early abilities such as joint attention and pretence, and their relationship to consequent performance on theory of mind tasks will provide important evidence for or against the existence of a developmental sequence in a theory of mind.

4. Investigation of factors beyond verbal and non-verbal MA is needed to obtain a clearer understanding of the necessary and sufficient conditions for a theory of mind to develop. Measures of social competence, or pragmatic skills, for example, and their relationship to the development of theory of mind ability have been investigated cross-sectionally but not yet longitudinally.

5. The clinical implications of the research on theory of mind need to be investigated. Can the development of a theory of mind be modified or hastened by appropriate intervention? Might, for instance, an emphasis on pragmatics or social-communicative skills early in life encourage improvement in meta-representational ability? The prognosis for autistic individuals with or without intervention remains inconclusive and as yet under-researched; it is important therefore to continue generating systematic data on what can be expected of autistic children when they 'grow up' as this sort of information will be vital to both parents and clinicians working with autistic people. A clearer understanding of the development of theory of mind, will provide a basis on which to advise and support parents and professionals in the most appropriate ways.

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having completed the approved course of study and passed the examinations as an Internal Student in the Faculty of Science has this day been admitted by the Senate to the Degree of

MASTER OF SCIENCE

in Clinical Psychology

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Vice-Chancellor

Michael Peckham

Director, The British Postgraduate Medical Federation

17 October 1990