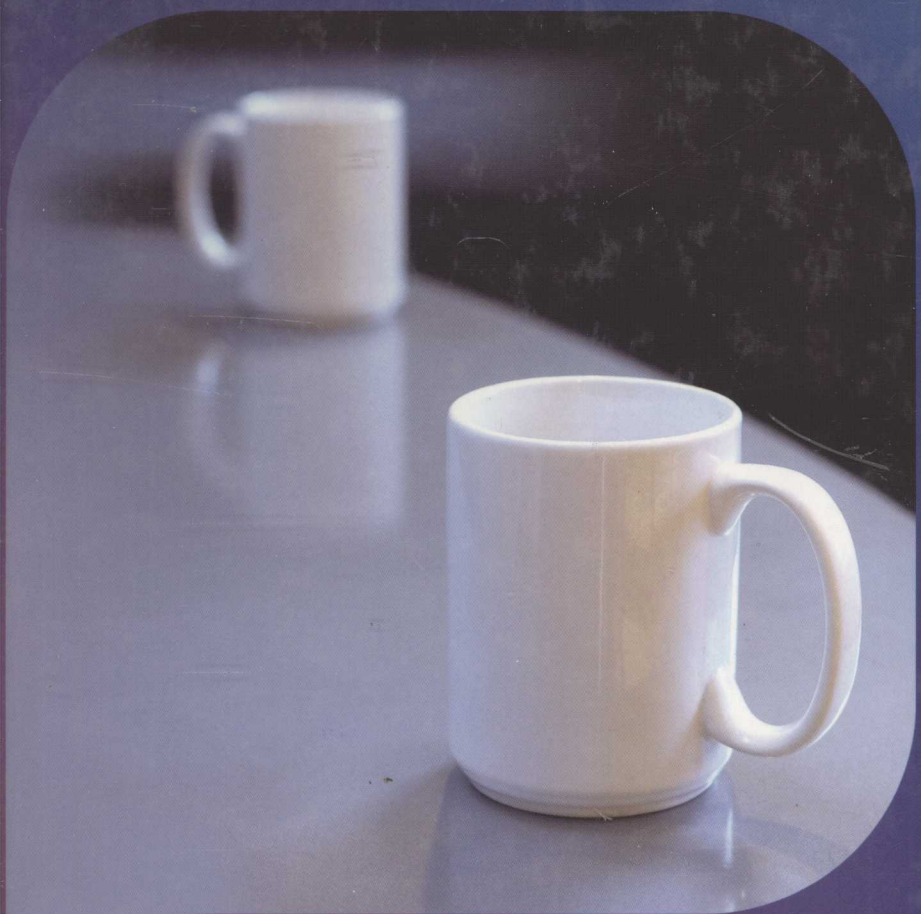

Edited by

Chris Freeman & Mick Power

HANDBOOK OF EVIDENCE-BASED PSYCHOTHERAPIES

A Guide for Research and Practice

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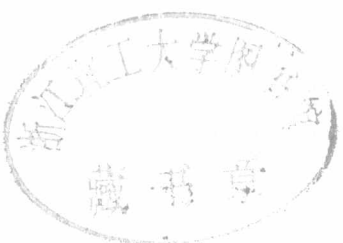

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Preface

There has never been a greater interest in evidence-based psychotherapies. Therapists more than ever wish to evaluate their clinical practice and develop their skills using such evidence in a self-reflective manner; healthcare managers want the best supported practices for psychological therapies run in a cost-efficient way and our patients and clients themselves surely also want some reassurance that there may be some support for the practices that they may be subjected to at times when they are likely to be in considerable distress.

So why might there be problems with such a noble cause? Since at least the early 1950s the general area of psychotherapy has been beset by a considerable degree of inflammatory accusation and counter-accusation that at times has felt like a religious war. The politics of established psychotherapies versus newcomers vying for position has seen considerable polarisation around the issue of which therapy tries to monopolise science while accusing other therapies of being non-scientific. All therapies, of course, are capable of using the practices of the scientific method in order to establish an evidence base and to amend, adapt and alter practices accordingly. This scientific approach is possible whatever the extreme claims of individual practitioners from the different psychotherapies might reflect. Psychoanalysis is neither more nor less capable of being evaluated than is behaviour therapy.

The main issues for the current volume are therefore, first, to accept that there is a need for an evidence base for the psychotherapies but, second, to cast a highly critical eye on many of the assumptions about the collection of that evidence base and to be critical of the so-called evidence base itself. We have therefore asked our contributors not simply to sing the praises of their preferred approach nor to over-state the strength of the relevant evidence base but rather to maintain a critical and honest stance regarding the strengths and weaknesses of the claimed evidence. We have also sandwiched the overview of psychological therapies and the disorder-by-disorder reviews between an introductory chapter that provides a critical starting point or perspective from which to approach the subsequent chapters and a concluding chapter that provides a critical framework for the evaluation of approaches to the evidence base together with a way forward for the future.

Our hope therefore is that the challenges that face the philosophical and practical issues of the concept and collection of evidence in psychotherapy are all debated in this volume in a manner that will be useful and enlightening to everybody involved in research on or the delivery of psychotherapy. We hope, too, that our clients and patients may also benefit from these debates for otherwise our efforts will be hollow.

Chris Freeman and Mick Power

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PART I

Overview of Therapies

CHAPTER 1

Introduction

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INTRODUCTION

There has been a recent surge of interest in evidence-based medicine, which has led one or two sceptics to question what medicine was based on before. The answer is, of course, that medicine has always been based on 'evidence' but maybe not on evidence as we now know it. There was always 'evidence' that the world was flat (you just had to look at the Earth), that the sun rotated around the Earth (you just had to look at the sun moving through the sky) and that blood letting was an effective form of treatment for maladies (some patients did actually recover who might otherwise have died). In a world where we seek to confirm rather than disprove our beliefs there is always evidence to be cited in favour of multiple and contradictory viewpoints. Given the ambiguous nature of evidence, therefore, one of the key questions for a book such as this has to be a consideration of the concept of evidence itself. What counts as evidence? Do different approaches take different views of what counts as evidence? What of apparently contradictory sets of evidence? How can such contradictions be resolved or explained?

There is now a major growth industry in medicine that examines the nature of evidence and draws evidence together in order to produce expert clinical guidelines for the treatment and management of all possible disorders. Most of this work draws on quantitative and qualitative methods for the assessment and grading of evidence in addition to methods for combining evidence across studies. Earlier evidence review methods were of a qualitative nature but there are now quantitative review methods, for example in the forms of meta-analyses and mega-analyses (see later) that allow the combination of evidence from different studies. The increasingly influential Cochrane reviews (named after the epidemiologist, Archie Cochrane, in particular for his influential 1972 book) categorise evidence from different studies into levels: high-quality meta-analyses and randomised controlled trials (RCTs), case-control or cohort studies, case reports or case series, and expert opinion.

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Cochrane reviews and other clinical guideline groups (such as the SIGN groups in Scotland and the NICE groups in England) pool together all published studies that satisfy basic inclusion criteria, but thereby suffer from the *publication bias* problem that positive results are more likely to be submitted and accepted for publication than are negative results. It is possible to estimate the extent of this bias based on the standard error of published findings (see, for example, Begg & Mazumdar, 1994), although this is rarely done when such reviews are carried out. However, an equally important issue that keeps many psychotherapists awake at night is whether or not the RCT view of evidence is the appropriate one for psychotherapy. Although RCTs provide the gold standard for evidence in many areas of medicine, a number of the requirements for a high-quality RCT are difficult to meet in psychotherapy research. For example, although in theory patients and raters in pharmacotherapy trials may be blind as to whether the person is in the placebo group or the active drug group of the trial, it is nearly impossible to blind patients (and therefore raters) about which arm of a psychotherapy trial they are in. Part of the purpose of this chapter and a number of subsequent chapters will therefore be to consider some of the limitations of the current evidence-based approach to avoid uncritical acceptance of a flawed and complex evidence base while also avoiding the need for its complete rejection.

EVIDENCE AND PSYCHOTHERAPIES

One of the classic and most destructive uses of evidence was in Eysenck's (1952) claim that psychodynamic psychotherapy was no more effective than leaving people to recover spontaneously. Apart from the controversy that Eysenck sparked, he also led to a generation of psychotherapy researchers determined to improve the science of psychotherapy outcome research, subsequent summaries of which argued that Eysenck had considerably over-estimated rates of spontaneous recovery and under-estimated psychotherapy change (for example, Luborsky, Singer & Luborsky, 1975; Smith & Glass, 1977). These research efforts focussed primarily on analyses of the immediate outcome of therapy and, by-and-large, led to the conclusion that all therapies were equally effective because of the operation of 'common factors' such as the therapeutic relationship. Although there is some truth in such a conclusion, and there is no question of the importance of a positive therapeutic relationship in relation to outcome (Hubble, Duncan & Miller, 1999), the chapters in this book will testify that such a conclusion must be significantly qualified in relation to specific disorders and different individuals.

One of the issues that any account of the psychotherapies must deal with and explain is the continued development of new psychotherapies; perhaps the focus on common factors may partly explain this continued development, but nevertheless it provides an enormous challenge if the evidence base is to keep pace with the rate of development even simply in terms of the number of new approaches. Herink (1980) documented over 250 varieties of therapy. This number had increased to about 400 by the early 1990s (Norcross & Goldfried, 1992) and the latest estimates put the number at about 500. Indeed, somewhere in California there is probably another therapy being christened at this very moment. The question that must be asked of this diversity is whether 500 different therapies need to operate by 500 different mechanisms, or whether, alternatively, there exist common factors that can offer some unification of the diverse theories and practices that occur under the label 'psychotherapy'. These common factors might apply irrespective of whether or not the therapies or therapists

are effective, so a more specific question must also be asked: 'Does the *good* cognitive therapist share anything in common with the *good* behaviour therapist or the *good* dynamic psychotherapist?' There is, in fact, a growing belief that, whatever the brand name, good therapeutic practice cuts across the artificial boundaries that therapies place around themselves in order to appear distinct from their competitors.

Some of the impetus for the exploration of integrative approaches to psychotherapy has arisen from the failure of many studies of the effectiveness of different therapies to find significant differences in outcome, as noted above. Stiles, Shapiro & Elliott (1986) have labelled this the paradox of 'outcome equivalence contrasted with content non-equivalence'. That is, it is clear from analyses of the content of therapy sessions that therapists of different persuasions do different things in therapy that are broadly consistent with the type of therapy to which they adhere (De Rubeis *et al.*, 1982; Luborsky *et al.*, 1985). Stiles, Shapiro & Elliott (1986) further argue that outcome equivalence applies not only to areas such as depression but also to areas where 'clinical wisdom' might suggest otherwise; for example, such wisdom would suggest that behavioural and cognitive-behavioural methods are more effective than other forms of therapies for the treatment of phobias. However, the evidence for this proposal arises from analogue studies with sub-clinical populations (primarily students), but they argued that it is less clear-cut from clinical trials.

The current book will provide an important update on issues such as whether or not all therapies really are equal and whether it really does not matter what the content of therapy is because outcomes are very much the same. We hope to show that, although this conclusion has some truth, in particular in its focus on the need for a positive therapeutic relationship, at the level of specific psychological disorders that range from simple phobias to severe psychoses there is evidence of differential effectiveness of therapies – that some things help and that some things do not.

EXAMPLES OF THE EVIDENCE BASE

The subsequent chapters in this book will provide numerous specific examples of studies that provide evidence one way or another for the use of particular therapies with particular disorders, but it is worth considering one or two such studies briefly, then considering one or two of the meta-analyses and mega-analyses in order to illustrate some of the more general points that we wish to make about the evidence base.

One of the most famous and most expensive therapy outcome studies was the National Institute of Mental Health (NIMH) Treatment of Depression Collaborative Research Program, which will be considered in order to illustrate the problems that have arisen from the general failure to find differential effectiveness of therapy outcome (see Elkin *et al.*, 1989; Elkin, 1994) but also to illustrate other issues about the evidence base. This trial was the largest of its kind ever carried out. There were 28 therapists working at three sites; eight therapists were cognitive-behavioural, 10 were interpersonal therapists, and a further 10 psychiatrists managed two pharmacotherapy conditions, one being imipramine plus 'clinical management', the second being placebo plus 'clinical management'. Two-hundred-and-fifty patients meeting the criteria for major depressive disorder were randomly allocated between the four conditions. The therapies were manualised and considerable training and supervision occurred both before and throughout the trial by leading authorities for each

therapy (see Shaw & Wilson-Smith, 1988, for a detailed account of this process). Elkin *et al.* (1989) reported that all four groups improved approximately equally well on the main symptom outcome measures. Perhaps the most surprising result was the extent of the improvement in the placebo-plus-clinical management group, which substantially outperformed control groups in most other studies, although a *post hoc* analysis showed that it was less effective for patients with more severe depressive disorders. Imber *et al.* (1990) have further shown that there were no specific effects of treatments on measures such as the Dysfunctional Attitude Scale on which, for example, the cognitive therapy condition would have been expected to make more impact than the other treatments. In summary, the NIMH trial illustrates that although it has been important to test treatment effectiveness, the simple comparison of outcome of treatment is the most expensive and least informative way in which to approach the issue.

To move now to an example of meta-analysis, we will start with the Robinson *et al.* (1990) study, which has been widely cited and is the most influential meta-analysis in the area of depression. Robinson *et al.* identified 58 trials of a comparison of psychological therapies and a further 15 trials of psychological therapy versus pharmacotherapy that were published between 1976 and 1986. The statistical combination of these studies gave an effect size of 0.73 for psychotherapy versus control (as a reminder, an effect size significantly greater than zero shows a positive benefit, with an effect size of >0.7 being considered to be a large effect). Other comparisons revealed a benefit of cognitive therapy over non-CBT therapies of 0.47, and of other cognitive-behavioural therapies over non-CBT therapies of 0.27. This meta-analysis has often been quoted as showing a distinct small-to-moderate benefit of cognitive and cognitive behavioural approaches over psychodynamic approaches in the treatment of depression. However, there are a considerable number of qualifications to this apparently straightforward conclusion. First, only nine of the 58 trials were based on standard clinical recruitment, with 50 % of trials recruiting participants through the media, 25 % of trials being student based, and with only 35 % having inclusion criteria for clinical depression. In addition, most of the studies reported only post-treatment data without follow-up data being included. Although more recent studies and more recent meta-analyses (such as Gloaguen *et al.*, 1998) generally have stricter inclusion criteria and include follow-up data, it is important to note the limitations of many of the earlier studies and the earlier meta-analyses.

A second type of approach for combining quantitative data has begun to appear – this is the so-called *mega-analysis* in which case-level data from several studies are combined in order to provide statistical power for more sophisticated analyses. For example, Thase *et al.* (1997) combined data from six different studies to give a total of 795 participants who had received cognitive behaviour therapy or interpersonal psychotherapy alone or combined with an anti-depressant. Their analyses showed that there is a benefit for combined drug-psychotherapy treatment for more severe levels of depression, but for mild to moderate levels of depression there was no advantage for combined treatment.

SOME BENEFITS OF THE EVIDENCE-BASED THERAPY (EBT) APPROACH

The examples presented so far illustrate that the evidence-based approach is fraught with more pitfalls than might at first be apparent. Qualitative and quantitative data analyses

to date have provided few categorical assertions that do not require caution and careful interpretation. In psychotherapy, the evidence-based approach should therefore be seen as more of a method to aid the asking of questions than as a source of answers. It is a method fraught with its own problems. We can, however, begin to draw together and examine criteria for the evidence-based approach.

The American Psychological Association Task Force (American Psychological Association, 1995; see also Crits-Cristoph, 1996) proposed a set of criteria for evidence-based therapies (or 'empirically validated treatments' in their parlance), which are a useful starting point. The Task Force proposed that for a treatment to be well-established it should:

(1) Have at least two good between-group design experiments demonstrating efficacy in one or more of the following ways:
   A. Superior to pill or psychological placebo or to another treatment
   B. Equivalent to an already established treatment in experiments with adequate statistical power (about 30 per group). OR
(2) Have a large series of single-case design experiments (N >= 9) demonstrating efficacy. These experiments must have:
   A. Used good experimental designs and
   B. Compared the intervention to another treatment as in 1)A.

These overall criteria were additionally qualified by a requirement for the use of treatment manuals, that the client groups should be very clearly described and that the benefits should be found by at least two different teams.

The Task Force also spelled out criteria for 'probably efficacious treatments' as follows:

(1) There should be two experiments showing the treatment is more effective than a waiting-list control group. OR
(2) One or more experiments meeting the Well-Established Treatment criteria above but only from one team rather than two or more. OR
(3) A small series of single-case design experiments (N >= 3) otherwise meeting Well-Established Treatment Criteria but again only from one team rather than two or more.

These APA guidelines provide a useful starting point for categorising evidence into different types and grades. However, the use of the term 'experiments' throughout may seem either odd or even aversive to many psychotherapists, as if their patients were being treated as rats running through mazes (although therapy, in imitation of life, may seem like that sometimes!). The criteria reveal the influence of the American behaviour therapy tradition with its focus on quasi-experimental single case designs (for example, so-called ABA, ABBA, ABCA designs) but, apart from the language, the criteria overlap with the Cochrane review criteria that were summarised earlier. That is, the best quality evidence from a 'good between-group design' is considered to come from a randomised controlled trial, although the APA guidelines give more weight to single-case quasi-experimental designs than do the Cochrane criteria.

A second clear benefit of the evidence-based approach is the continued examination of the therapeutic relationship, client variables, therapist variables, and other common factors in relation to therapy process and outcome. The traditional approach to such common factors is best summarised in the series of handbooks that have been edited over the years by Garfield and Bergin (for example, Garfield & Bergin, 1978, 1986; Lambert, 2004) and which have exhaustively detailed research into therapist factors, client factors, and therapy factors. Work

on therapist factors was best exemplified by research into client-centred therapy (Rogers, 1957) and the proposed trinity of warmth, empathy, and genuineness (Truax & Carkhuff, 1967), which every therapist was supposed to possess. However, the early optimism that characterised this work eventually led to the realisation that even 'ideal' therapists had patients with whom they did not get on well and that the presence of these therapist factors in themselves was not sufficient for therapeutic change. As Stiles, Shapiro & Elliott (1986, p. 175) concluded: 'The earlier hope of finding a common core in the therapist's personal qualities or behaviour appears to have faded.'

Work on client variables has in the past been characterised by the examination of atheoretical lists of sociodemographic and personality variables (see, for example, Garfield, 1978), from which it has been possible to conclude very little. In a re-examination of this issue, Beutler (1991) concluded that there still has been no development in our understanding of client variables. Following a summary of some of the major variables that might be examined, Beutler (1991, p. 229) also pointed out that: 'There are nearly one and one-half million potential combinations of therapy, therapist, phase, and patient types that must be studied in order to rule out relevant differences among treatment types.'

Fewer than 100 methodologically sound studies have been carried out to test these possible interactions! There are, however, some promising leads from investigations of client attitudes and expectations that provide a more sophisticated view of such variables. For example, Caine and his colleagues (Caine, Wijesinghe & Winter, 1981) found that the type of model that clients had of their problems (for example, medical versus psychological) and the direction of their main interests ('inner-directed' versus 'outer-directed') predicted drop-out rates and outcome in therapy.

Work on specific therapy factors has also run aground on the problems of finding any differential effects (for example, Stiles, Shapiro & Elliott, 1986). Some of these problems were outlined earlier, when the pattern of outcome equivalence of psychotherapies for a range of disorders was outlined. As we hope this book will demonstrate, there are beginning to be advances in this area, which should continue in the future, for example with the use of so-called 'dismantling', in which one or more of the putative 'active' ingredients of a therapy are dropped in some of the conditions, and the manualisation of therapies combined with measures of treatment adherence, which ensure that something like the therapy in question is actually taking place. However, as the NIMH Collaborative Depression study demonstrated (see above), the fact that some therapists did extremely well and some not so well irrespective of the type of therapy demonstrates that therapy factors will only emerge in interaction with other therapist and client variables rather than as main effects. A specific example of this point comes from the Sheffield Psychotherapy Project carried out by David Shapiro and his colleagues. The analyses of this project published initially showed an advantage for prescriptive (cognitive-behavioural) therapy over exploratory (psychodynamic) therapy in the treatment of stressed managers. However, a later reanalysis (Shapiro, Firth-Cozens & Stiles, 1989) found that this advantage was true for one of the principal therapists involved in the study, but the second therapist was equally effective with both types of therapy. In an interesting conclusion, Shapiro, Firth-Cozens & Stiles (1989, p. 385) turned the initial question of which brand of therapy is better than which other brand on its head, as follows: 'The present findings are broadly consistent with the clinical lore that each new therapist should try different approaches to find the one in which he or she is most effective.'

The notion of the importance of the alliance between therapist and patient arose early in the psychoanalytic literature. Freud (1912) viewed it as the healthy part of the transference,