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INVITED ESSAY

THE OUTCOME PROBLEM IN PSYCHOTHERAPY: WHAT HAVE WE LEARNED?

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Summary—The outcome problem in psychotherapy has usually been studied without much regard to the theories underlying the methods used. It is suggested that theories are vital to scientific advancement, and that without them we cannot even specify criteria to judge outcomes. Numerous studies since the 1950s have in essence failed to disconfirm the view that various forms of psychotherapy do not show greater effectiveness than spontaneous remission or placebo treatment. An effort is made to clarify the nature of spontaneous remission and placebo treatment, and to discuss the consequences of the many empirical findings and meta-analyses published over the past 50 years. A theory is suggested linking spontaneous remission, placebo treatment, psychotherapy and behaviour therapy, leading to a discussion of ethical considerations and cost-effectiveness issues.

THE ROLE OF THE PARADIGM

In 1952, I wrote my first paper on "The effects of psychotherapy" (Eysenck, 1952), a paper reprinted in the *Journal of Consulting and Clinical Psychology* 40 years later (Eysenck, 1992a). Events of the intervening 40 years have been discussed elsewhere (Eysenck, 1991a, 1993a); they have not caused me to change my verdict of 'not proven'. I still do not believe that 'dynamic' psychotherapists have provided unambiguous evidence that their methods are significantly superior to no treatment, to placebo treatment or to non-dynamic behavioural treatments. This view is often regarded as old-fashioned. Thus, Garfield (1992), in reply to a chapter of mine (Eysenck, 1992b), stated that in his opinion "Eysenck has adhered too fixedly to his views concerning spontaneous remission, the placebo response and the complete superiority of behaviour therapy over all other forms of therapy" (p. 129). Similarly, Grawe (1992) quotes the meta-analyses of therapy effects of Smith, Glass and Miller (1980) and Lambert, Shapiro and Bergin (1986) to conclude that "with these results we can regard Eysenck's general doubts concerning the efficacy of psychotherapy as done with" (p. 135)—a conclusion the truth of which I took the liberty to doubt (Eysenck, 1993b).

Even the media, with typical arrogant ignorance of the very nature of the debate, have decided that: "Today, researchers have enough data to refute Eysenck's charge with conviction" (Goode, 1993). As the only people consulted were professional psychotherapists, their judgement might have been predicted. After all, the very existence of a large professional group is at stake, and one would not expect them to imitate the lemmings! Just as the early eighteenth century physicists defended the concept of phlogiston to the death, long after Lavoisier had demonstrated that the concept had no scientific value, so the varied theories underlying psychotherapy are defended by practitioners and the media.

I believe that the differences between my own approach and that of critics like Grawe, Garfield and others are much greater and more serious than is often realized, and that it is very doubtful if empirical data and meta-analyses can conceivably settle the issue. I believe that we are dealing with a paradigm conflict here (Eysenck, 1988), as defined by Kuhn (1962, 1974), and that the various terms used in the discussion, such as 'spontaneous remission' 'placebo treatment' and 'psychotherapy' require far more detailed analysis than they have received in the past. Gruenbaum's (1993) detailed discussion of the divergent definitions and uses of the placebo concept may

alert the reader to the dangers of using such concepts without careful definition and specification. I shall try and state my position as clearly as I can, rather than enter into a battle of the meta-analysts. Where one such meta-analysis of short-term dynamic therapy can conclude, on the basis of analysing the results of 19 short-term psychodynamic psychotherapy studies, that effects did not differ significantly 1 year after termination of treatment from those for no-treatment groups (Svardberg & Stiles, 1991), while another could conclude that "brief dynamic therapy demonstrated large effects relative to waiting list conditions" (Crits-Christoph, 1992, p. 151), it seems obvious that meta-analysis is too subjective and flawed as a procedure to give us a definitive answer (Eysenck, 1984, 1992c). I have discussed elsewhere the faults of published meta-analyses, like that of Smith *et al.* (1980), and I find it odd that anyone can still take the results seriously (Eysenck 1992b, 1993a). Authors who claim that they have proved psychotherapy to be extremely successful when their own data show no difference between the major therapies and placebo treatment should not be cited as disproving my doubts about effectiveness of psychotherapy!

What is the essence of Kuhn's notion of the role of paradigms? His views, as well as those of many other philosophers of science, are well discussed and reviewed in Suppe's (1974) Structure of Scientific Theories. Kuhn's view arose as one of many criticisms of the 'Received View' of the Vienna school of logical positivism, so earnestly embraced by many behaviourists. He objected to the central idea of that school, which accounts for the interpretation of a theory's formulation in terms of correspondence rules. According to the Received View, the empirical or observational content of the symbolic generalization in a theory is fully or partially specified by correspondence rules which explicitly state the allowed methods for attaching the generalizations of phenomena and also supply the various theoretical terms in the generalizations with their empirical interpretation or meaning. Kuhn, on the other hand, rejects this account, arguing instead that "a new theory is always announced together with (exemplary) applications to some concrete range of natural phenomena" (Kuhn, 1962, p. 46). He posits instead that scientists work according to paradigms, or readily accepted theories and methods of observation, experimentation and interpretation. A paradigm consists of a disciplinary matrix, acquired implicitly through the educational process, whereby one comes to be a licensed practitioner of the scientific discipline. In the course of this learning process the student encounters numerous *exemplars*; these are instances exemplifying the ways the science's symbolic generalizations (the so-called laws or theories) apply to phenomena. Exemplars are archetypal applications of symbolic generalizations or theories to phenomena. According to Kuhn, the scientist obtains his disciplinary matrix from the study of exemplars, and these in large part determine the nature of that matrix. What I shall argue is that psychotherapists like Grawe and Garfield argue from a different paradigm from the one I have adopted, so that their disciplinary matrix and their exemplars are very different from mine. This sort of difference, Kuhn argues, makes communication difficult if not impossible. As the Victorian wit, Sydney Smith, once said, when two Aberdonian fishwives were shouting insults at each other from the windows of their houses at opposite sides of a narrow street, "These two will never agree-they are arguing from opposite premises!"

SCIENCE AND THEORY

It is relevant to my thesis to consider how knowledge is acquired. To begin with we have experience and common-sense; we acquire an enormous amount of knowledge of physics, chemistry, psychology and even medicine through change, experience and the application of common-sense. The most primitive tribes know enough about gravitation, wind force and air resistance to throw stones and spears accurately, jump with precision, light fires and do a thousand-and-one things needing some understanding of the forces of nature. Similarly, they have enough knowledge of psychology to understand the effects of emotion, motivation and mood; to realize the importance of obvious differences in ability, leadership and personality; to use this knowledge to domineer, subject, frighten or please others. Indeed, without a large body of such knowledge, man would have found it impossible to survive, and even animals obviously have important rudiments of such knowledge in their repertoires.

Such knowledge in time led to the beginnings of technology. Man learned to mine for coal, iron and gold, to produce metal weapons and containers, make alloys like bronze to improve the quality

of the product. Whole eras (Iron Age, Bronze Age) testify to the importance of these technological achievements. Man also learned to work hides to make garments, to weave, to cook, to produce colouring materials—the history of technology is the history of human adjustment to life on this planet. Technology reached a high degree of development in ancient Egypt, Greece and Rome, and above all in China (Temple, 1991); Joseph Needham's multi-volume work on *Science and Civilization in China* will be familiar to most readers. Chinese technologists discovered and built suspension bridges, fishing reels, the stirrup, paper money, playing cards, the seismograph, the blast furnace and mustard gas. They also were the first to chart the heavens, discover the circulation of the blood, isolate sex hormones—the list is endless. But as Mendelssohn (1976) has so clearly demonstrated, they never discovered science. Science is an exclusively Western discovery, although of course now it has become a world-wide industry, in terms of making technological advances possible (Wolpert, 1992).

What separates science from technology? Poincare put his finger on the difference when he said: "Science is built up with facts, as a house is built with stones. But a collection of facts is no more a science than a heap of stones is a home". Technology consists of isolated advances, but science is *organized knowledge*. Technology works; science tells us why it works and predicts new advances. Science is essentially abstract, where technology for applications. Each can aid the other, but there is an essential difference between them. This difference is related to the importance of large-scale, fact-based *theories*; as Darwin once said, he could not understand why any scientist would do anything if not to support or disprove a theory.

It is here, I think, that the major difference arises between my own point of view, and that of my critics. My view is that psychotherapy is essentially a technology without any scientific basis, and that our major concern should be with the creation and working out of a *scientifically valid* theory underlying our efforts. Grawe (1993) puts this very clearly and explicitly in a reply to my 1993b article: "In contrast to Eysenck, I am not concerned with the truth or falseness of any particular theory. My concern is primarily with as complete and unbiased a determination of the facts as possible" (p. 181). But as Poincare said: "Les faits ne parlent pas"—the facts don't speak. As most scientists now realize, facts are theory-laden; a failure to understand this elemental truth lies at the basis of all our misunderstandings. This fundamental error is also at the basis of *implicit* disregard of the value of theory, as in the advocacy by Lazarus (1971) of *eclectic* psychotherapy, i.e. the use of all type of methods of treatment *regardless* of theory. I have criticized this view elsewhere (Eysenck, 1970), advocating instead "behaviour therapy as a scientific discipline" (Eysenck, 1971); I will not repeat these arguments.

Kuhn's argument that no real debate is possible between adherents of two different paradigms also points up the importance of theory. Without theory, the alleged 'facts' are impossible to interpret. Such questions as "Does psychopathology work?" cannot be answered in any form whatsoever without theory. How are we to test such an assertion about the success of psychotherapy? The Freudian theory rejects concern with symptom removal or alleviation of symptoms; symptoms, in that theory, are like fever, indicative of an underlying illness, and it is this illness that has to be cured in order to remove symptoms on a permanent basis. Symptom-oriented therapies will lead to symptom-restitution or symptom-substitution, not a permanent recovery. Behaviour therapy, on the other hand, is based on the theory that symptoms are directly produced by conditioning processes, and that a therapeutic process aimed at *extinction* of the CR will permanently remove the symptom. Which is to be the criterion of success, symptom removal, or general personality change as demanded by Freud (Erwin, 1978)? We can only answer such a question by attacking the theoretical problem directly, by demonstrating that symptom-substitution does not normally follow successful behaviour therapy, but that symptom-restitution often follows apparently successful 'dynamic therapy' (Eysenck & Martin, 1987). [A detailed discussion of this problem is given by Schulte (1993).]

Again, in comparing 'dynamic' psychotherapy and behaviour therapy, we must specify the population to which the therapy applies. I suggested in my original article on behaviour therapy that the theory specified *neurotic* disorders (Eysenck, 1959, 1960). It is not so clear for what type of disorder 'dynamic' psychotherapy would apply, but there seems to be good agreement that neurotic disorders are its prime concern. So proper comparison would require groups of neurotic

patients, yet such a meta-analysis as that by Crits-Christoph (1992) includes a group called 'mixed', opiate addicts, cocaine abusers, pathological grief and personality disorder, i.e. groups which are hardly typical of neurotic disorders, and for which it is doubtful if either theory would make any confident prediction. However that may be, *the make-up of the population at risk can only specified in terms of a proper theory*.

If there is to be a comparison with a placebo group, or a no-treatment group, theory is doubly necessary to decide on the nature of the placebo, and the conditions under which the no-treatment group is acquired. Both points will be discussed later on, but consider the theory that spontaneous remission occurs because the neurotic, in the absence of explicit therapy, will seek solace, compassion, advice and support from parents, friends, relatives, teachers, priests etc., who will dispense a kind of amateur therapy which may or may not be equally effective as 'dynamic' therapy (Eysenck, 1980). If, as in many studies, we use a waiting-list group for our control, the likelihood is that members, knowing that proper treatment will soon be available, will not appeal to lay persons for help, so that they will be sharply differentiated from true no-treatment groups. This is an hypothesis, derived from theory, but it is clearly testable and, if correct, would require explicit recognition of differences in method of recruiting no-treatment groups. It is characteristic of the lack of interest in theoretically important problems that no empirical studies are available to support or reject this notion; we pretend that no differences exist, and confound effects in our meta-analyses.

Another important variable is duration of treatment. Freud predicted that the acquisition and fixed resolution of transference effects is essential to cure, and takes a long time. The very term 'transference' is of course theory-laden, and should never be used until the theory on which it is based has been empirically confirmed. (Typically, Freudians use the *occurrence* of the effect as *proof* of the theory brought forward to explain it.) Behaviour theorists would not deny the (occasional?) occurrence of observable dependence relations between therapist and patient, but would ascribe them to positive reinforcement produced by relieving anxiety. Conditioning theory predicts short duration of successful treatment, so that here too predictions from the two theories differ. Again, the differences predicted by the two theories should be used to carry out specific experimental tests of the correctness of the prediction, rather than carry out comparisons of treatment effects which confounded them. The widespread use of short-term psychotherapy suggests that Freudians have implicitly acknowledged that Freudian theory was wrong on this point.

A final point concerns the measure to be used in assessing the outcome of the therapy. A bewildering number of quite different measures has been used for that purpose. We have so-called projective measures like the Rorschach or the TAT, introspective ratings by the patient, ratings by dependent judges or by family members; we have general questionnaires like the MMPI, or special questionnaires like the fear thermometer; we have objective scoring of neurosis-related behaviour, like counts of the number of hours spent by the obsessive-compulsive patient on cleaning rituals or the number of bed-wettings per week by the enuretic patient. Theory governs the choice of instrument (although one often suspects that convenience is more important in making the choice!), and instruments acceptable to one side may not be so acceptable to the other. Freudians may accept data obtained through projective devices; behaviour therapists would prefer evidence from behaviour and observation. Meta-analyses adding together results obtained through the use of all of these methods simply confound incommensurate data.

Differences in theory clearly result in differences in therapy, as Jones and Pulos (1993) have demonstrated. Studying details of actual treatment programmes, they found that very marked differences existed between typical procedure and aims of these two contrasting therapies. The whole field of what are the aims of therapeutic intervention, together with a discussion of how we can approach the problems associated with it, is treated in great detail by Grawe (1993b), in an article that spells out details I have no space to discuss.

It would be possible to continue almost indefinitely identifying differences between the two theories in question, differences which make direct comparisons in terms of percentage cures meaningless. It is the *theory*, which is ultimately important, and which should be tested directly; on this point perhaps Freud and I would agree. To take pride in *not* being interested in theory, as Grawe clearly does, seems to me a denial of everything science stands for, and an attitude that will make it impossible to answer the question of therapeutic success. (I have chosen Grawe as the exponent of a very widespread view, precisely because he is one of the most level-headed psychotherapists, aware of criticisms of the 'dynamic' schools, and very favourable to empirical testing; indeed, his own work is exemplary in this respect. I have not singled him out because I consider him a weak opponent, quite the contrary. I consider him, and Garfield as well, as good scientists, good empiricists and men devoted to the ideals of science.)

SPONTANEOUS REMISSION AND THE GROWTH OF SCIENCE

The importance of spontaneous remission is widely recognized (Rachman & Wilson, 1980). Obviously, it is not 'spontaneous', in the sense of not being caused; it must be an effect of events occurring during the time elapsing between breakdown and recovery. Returning to our discussion in the previous section of pre-scientific common-sense discoveries, it seems clear that since prehistoric times men and women have used many strategies to reduce anxiety, depression and other psychological dysfunctions. Many of these are obvious precursors of psychotherapeutic and behavioural methods. Sufferers have used relaxation, in the sense of taking time off, going on holidays etc.; they have tried to seek social support, often in get-togethers with other sufferers; they have discussed their problems with friends, family or special officials like priests, rabbis etc.; they have sought absolution in the confessional, they have used pet-therapy and 'given their hearts to a dog to care'. All these methods, especially that involving a sympathetic listener, have been shown to relieve anxiety and depression, and reduce the severity of neurotic disorders; indeed, if they did not do so, even if in a rather haphazard manner, they would hardly have survived as long as they have-people continue with the use of effective reducers of anxiety and discard non-effective methods. Let us call the sum of these treatment methods To, for original, primitive, fundamental, early, elementary, primordial types of treatment, based on no hypotheses at all, i.e. Ho!

 T_o also includes methods we would now consider examples of behaviour therapy. I have elsewhere (Eysenck, 1990) quoted from the German poet Goethe's semi-autobiography, *Dichtung und Wahrheit*; Goethe, like Freud, was beset with neurotic symptoms, but unlike Freud was successful in getting rid of them. This is what he has to say describing his situation as a 20-year-old law student of the University of Strassburg in 1770–1771:

"I found myself in a state of health which furthered me sufficiently in all that I would and should undertake; only there was a certain irritability left behind, which did not always let me be in equilibrium. A loud sound was disagreeable to me, diseased objects awakened in me loathing and horror. But I was especially troubled by giddyness which came over me every time that I looked down from a height. All these infirmities I tried to remedy, and, indeed, as I wished to lose no time, in a somewhat violent way. In the evening when they beat the tattoo, I went near the multitude of drums, the powerful rolling and beating of which might have made one's heart burst in one's bosom. All alone I ascended to the highest pinnacle of the minster spire, and sat in what is called the neck, under the nob or crown, for a quarter of an hour, before I would venture to step out again in the open air, where, standing upon a platform scarce an ell square, without any particular holding, one sees a boundless prospect before, while the nearest objects and ornaments conceal the church, and everything upon and above which one stands. It is exactly as if one saw oneself carried up into the air in a balloon. Such troublesome and painful sensations I repeated until the impressions became quite indifferent to me, and I have since then derived great advantage from this training, in mountain travels and geological studies, and on great buildings, where I have vied with the carpenters in running over the bare beams and the cornices of the edifice, and even in Rome, where one must run similar risks to obtain a nearer view of important works of art. Anatomy, also, was of double value to me, as it taught me to tolerate the most repulsive sights, while I satisfied my thirst for knowledge. And thus I attended, also, the clinical course of the elderly doctor Ehrmann, as well as the lectures of his son on obstetrics, with the double view of becoming acquainted with all conditions and of freeing myself from all apprehensions as to repulsive things. And I have actually succeeded so far, that nothing of this kind could ever put me out of my self-possession. But I sought to steel myself not only against these impressions on the senses, but also against the infections of the imagination. The awful and shuddering impressions of the darkness in churchyards, solitary places, churches and chapels by night, and whatever may be connected with them, I contrived to render likewise indifferent; and in this, also, I went so far that day and night, and every locality were quite the same to me; so that even when, in later times, a desire came over me once more to feel in such scenes the pleasing shudder of youth, I could scarcely force this, in any degree, by the strangest and most fearful images which I called up."

Goethe not only anticipated methods such as flooding with response prevention, he even laid down the theoretical essence of behaviour therapy when he said, in his novel, *Whilhelm Meister*: "To heal psychic ailments, that we have contracted through misfortunes or faults of our own, the understanding avails nothing, reasoning little, time much, but resolute action everything". Could anyone, even nowadays, express the theory of behaviour therapy more succinctly?

Even earlier, Aristotle was asked how the timid might acquire courage. "It is through acting bravely that we become brave", he said. There are many other early examples of behavioural precepts.

Aversive conditioning was illustrated by Demosthenes, the famous Athenic orator, when he tried to cure a tic-like lifting of his left shoulder by suspending a sharp sword just above it. Whenever the tic occurred, the skin of his shoulder was punctured, and the pain caused the tic to disappear. Even such apparently modern designs as the token economy (Ayllon & Azrin, 1968) were clearly anticipated in exact detail by Alexander Maconochie of Norfolk Island (Barry, 1958), who introduced just such an economy in the prison population of Norfolk Island in 1840, with considerable success (Eysenck, 1972).

Many other examples could be given; these will suffice to show that T_0 is very extensive, although it preceeds scientific psychology by hundreds or even thousands of years. T_0 became formalized in the wake of two movements—Mesmer's animal magnetism and the German stream (Pichot, 1989), with the members of the latter coining the term 'Psychic Therapy' in 1803. The Franklin Commission, set up to investigate Mesmer's claims, discovered the phenomena of 'transference', commenting on the amorous nature of the relationship between magnetizer and magnetized. Pichot makes it clear how absurd are the claims that psychotherapy started with Freud; it was actually being practised and systematized into a technology long before he made his own contribution, the major points of which had been adumbrated by Janet (1893, 1904) and others.

We now come to a consideration of the scientific nature of Freud's contribution. Here it is necessary to recognize the important role which *pseudosciences* have played in the transition from common-sense observation and technology to true science. Chemistry arose from the ashes of alchemy; astronomy from the sad remains of astrology. Great scientists were often embroiled in the workings of these pseudosciences; Kelper was court-astrologer, Newton worked in the field of alchemy. These pseudosciences had a beneficent influence by stressing the importance of general theories in order to promote scientific growth, even though their theories were vague and (essentially) wrong. Every science has to pass through this ordeal by quackery, and at the present time psychology, on its way toward becoming a proper science, has to slough off the quackery of Freudianism. Psychoanalysis is a pseudoscience just as alchemy and astrology were pseudosciences (Popper, 1959), and while its influence prevails, it will prevent psychology from emerging from its chrysalis.

This is not to say that alchemy and astrology did not play an important part in the emergence of chemistry and astronomy. Paracelsus proclaimed sulphur, mercury and salt as the *tria prima* of his 'spagyric art of chemistry', anticipating modern theories of metals, where there is an excess of electrons, non-metals, where there is a lack of electrons, and salts, where exchanges have taken place between the metal and the non-metal ions. Thus, even in the dross of psychoanalysis there may be found some traces of gold—perhaps! But they will not be found by the *methods* of psychoanalysis.

PLACEBO TREATMENT

The notion of placebo treatment has a firm basis of fact in the medical treatment of physical conditions. Here we can separate out quite clearly the psychological (placebo) factors from the physical (medical) effects of the pills administered, or the operations performed. Even there, as Gruenbaum (1993) has cogently pointed out, there are problems. Does the therapist believe in the power of the treatment, or not? Can he be blind to side effects? There are many complications, but essentially the power of the placebo is undoubted (e.g. Beecher, 1955; Benson & Epstein, 1975; Benson & McCallie, 1979; Shapiro & Morris, 1978); over 1000 articles and books attest to its influence (Turner, Gallimore & Fox-Henning, 1980; White, Tursky & Schwartz, 1985). The concept

has given rise to many controversies in the biological (Fisher & Greenberg, 1989; Margraf, Ehlers, Roth, Clark, Sheikh, Agras & Taylor, 1991), and psychosocial fields (Bentler, 1991; Luborsky, Singer & Luborsky, 1975).

How great is the placebo effect? While the importance of non-specific treatment effects is widely recognized, the effect size has been rather neglected in research. Beecher (1955) reported 26 studies with placebo response rates ranging from 15 to 58%. Moertel (1978) reviewed 31 controlled trials of the treatment of acute ulcers with the drug cimetidine; for 18 of the 31 trials, there was no specific difference between drug and placebo outcomes. The mean placebo effect was 48%, trials ranging between 10 and 90%. These and other studies reviewed by Roberts, Kewman, Mercier and Howell (1993) may be judged to give *minimum* estimates of placebo effectiveness, because by definition the people administering them *did not believe in their effectiveness*; such belief may constitute a crucial factor in placebo effectiveness.

An important study of placebo effectiveness when such belief was present has been reported by Roberts *et al.* (1993). They investigated the hypothesis that the power of non-specific effects may account for as much as two-thirds of successful treatment outcomes when both the healer and the patient believe in the efficacy of a treatment. Five medical and surgical treatments, once considered to be efficacious by their proponents, but no longer considered effective based upon later controlled trials, were selected according to strict inclusive criteria. A search of the English literature was conducted for all studies published for each treatment area. The results of these studies were categorized, where possible, into excellent, good and poor outcomes. For these five treatments combined, 40% excellent, 30% good and 30% poor results were reported by proponents. It was concluded that, under conditions of heightened expectations, the power of non-specific effects far exceeds that commonly reported in the literature.

These results are in good agreement with Roberts' (1983) earlier hypothesis of one-third excellent results, one-third good results and one-third poor results under conditions of heightened expectations; they contradict the common belief that non-specific effects will generate an average of only one-third positive outcomes. Roberts *et al.* (1993) quote Rousseau (1992), who described three characteristics of what he called 'pathological science': (a) The effect being studied is often at the limits of detectability or has a very low level of significance so that it is difficult to design experiments that test the effect reliably; (b) a readiness to discard prevailing ideas and theories, and (c) the near impossibility of devising and conducting experiments which can provide definitive answers to the questions being asked. These three characteristics are uncomfortably close to what has been occurring in the study of the relative efficacy of psychological treatments. The scientific study of whether successful psychotherapeutic outcome is a matter of technical skills, as specific factors, or interpersonal skills, as non-specific factors, may be an example of pathological science. The results of the studies to be discussed point in this direction.

Roberts et al. (1993) conclude:

"In summary, under the treatment conditions described in this report, the average power of non-specific effects appears to be considerably greater than the one-third routinely reported in both the psychological and the medical literature. The treatment conditions leading to such high levels of non-specific effects in some medical and surgical treatments are routinely the case for most psychosocial treatments. Such reasoning leads to the conclusion that there is little, if any, room for demonstrating differential effects among psychosocial treatments when positive expectations are shared by practitioners and patients. This observation may extend also to treatments provided by many non-conventional practitioners. A major block to conducting research to resolve some of the issues raised by this report is the absence of a theory or model encompassing the phenomena of non-specific effects. Such a theory or model is sorely needed." (p. 388) (My italics).

It seems likely that there are factors common to all therapies, and in particular psychotherapies, which are non-specific, unrelated to the *theory* on which any particular treatment is based and which may account for a considerable portion of any change observed. Jerome Frank (1961) has argued that four factors common to all psychotherapies and other methods of healing account for therapeutic effects:

1. The existence of a specific relationship between the patient and the help-giver. The relationship arouses the patient's faith in the confidence of the therapist and his desire to help.

- 2. Treatment is conducted in a locale designated by society as a place of healing. Hence, the patient's expectancies for help are aroused.
- 3. A rationale is provided that presents a patient's problem in an optimistic light. The rationale explains the cause of the problem as well as specific goals and processes of the therapy.
- 4. A task or procedure is presented that requires the therapist's and patient's participation. These tasks serve as a medium through which a therapist can exert his/her influence.

We may call these factors, and the hypotheses theory underlying their effectiveness, H_P , and thus create a class of influences $T_O + T_P$ which summates the non-specific elements of psychological treatment. Placebo treatment in psychotherapeutic experiments is of course very variable; it may consist of little more than registering a prospective patient for future therapy (waiting-list control), which is often regarded as no-treatment, but may lead to a neglect of T_O effects, and a possibly compensatory T_P effect. Usually placebo treatments in such experiments are not very credible to the patient, and of course are not believed to be effective by the therapist; hence they are likely to be much less effective than they would be if the therapist actually believed in them (Roberts *et al.*, 1993). Do recognized treatments actually outperform $T_O + T_P$? I have always been doubtful about the evidence brought forward to support such an allegation, and we must now turn to a consideration of such evidence as is often produced. After all, if the effects of diazepam are all due to placebo effects (Shapiro, Struening, Shapiro & Milcarek 1983), why not those of psychotherapy also?

THE EFFECTS OF PSYCHOTHERAPY

I shall contrast psychotherapy and behaviour therapy along the lines of my original definition of behaviour therapy (Eysenck, 1959). The essential features differentiating the two types of theories are listed in Table 1. Nowadays the contrasting groups have a wider inclusion. Grawe (1992) has labelled these 'dynamic-heuristic' and 'behavioural-cognitive', and that may be acceptable to characterize the psychotherapeutic and the behavioural approaches, respectively. I am of course aware that there have been many attempts to slur over the important differences between these theories (e.g Arkowitz & Mercer, 1984; Marmor & Woods, 1980; Wachtel, 1977), but the differences are too important and indeed fundamental to be thus judged. Efforts to do so are reminiscent of Tycho Brahe's attempt to reconcile Ptolemy's geocentric and Copernicus' heliocentric theories by a compromise that made the sun go round the earth, but the planets round the sun. Like all such compromises between irreconcilable theories, it ended in failure and ridicule.

Do such therapies work? Smith *et al.* (1980) have given an answer that has been widely accepted. "Psychotherapy is beneficial, consistently so and in many different ways. Its benefits are on a par with other expensive and ambitious interactions, such as schooling and medicine" (p. 183), they gush.

Table 1. Differentiation between theories underlying psychotherapy and behaviour therapy

Freudian psychotherapy		Behaviour therapy		
1.	Based on inconsistent theory never properly formulated in postulate form.	 Based on consistent, properly formulated theory leading testable deductions. 	g to	
2.	Derived from clinical observations made without necessary control observation or experiments.	 Derived from experimental studies specifically designed to basic theory and deductions made therefrom. 	test	
3.	Considers symptoms the visible upshot of unconscious causes ('complexes').	3. Considers symptoms as unadaptive conditioned responses.		
4.	Regards symptoms as evidence of repression.	4. Regards symptoms as evidence of faulty learning.		
5.	Believes that symptomatology is determined by defence mechan- ism.	 Believes that symptomatology is determined by individual di ences in conditionability and autonomic lability, as wel accidental environmental circumstances. 	iffer- ll as	
6.	All treatment of neurotic disorders must be historically based.	 All treatment of neurotic disorders is concerned with ha existing at <i>present</i>; their historical development is largely ir vant. 	abits rele-	
7.	Cures are achieved by handling the underlying (unconscious) dynamics, not by treating the symptom itself.	 Cures are achieved by treating the symptom itself, i.e. extinguishing unadaptive CRs and establishing desirable C 	. by CRs.	
8.	Interpretation of symptoms, dreams, acts etc. is an important element of treatment.	 Interpretation, even if not completly subjective and erroneou irrelevant. 	us, is	
9.	Symptomatic treatment leads to the elaboration of new symptoms.	 Symptomatic treatment leads to permanent recovery prov autonomic as well as skeletal surplus CRs are extinguished 	ided d.	
10.	Transference relations are essential for cures of neurotic dis- orders.	 Personal relations are not essential for cures of neurotic disor although they may be useful in certain circumstances. 	rder,	

The outcome problem in psychotherapy

Type of therapy:		Effect size: psychotherapies	SD	SEM
T ₁	Psychodynamic	0.69	0.50	0.05
Т,	Adlerian	0.62	0.68	0.18
Τ,	Client-centred	0.62	0.87	0.07
T₄	Gestalt	0.64	0.91	0.11
Τ,	Rational-emotive	0.68	0.54	0.08
T ₆	Transaction	0.67	0.91	0.17
Average		0.65		
T ₇	Placebo	0.56	0.77	0.05
Type of therapy		Effect size: behaviour therapies	SD	SEM
T _s	Systematic desensitization	1.05	1.58	0.08
T,	Implosion	0.68	0.70	0.09
T ₁₀	Behaviour modification	0.73	0.67	0.05
T ₁	Cognitive-behavioural	1.13	0.83	0.07
T_{12}	Other cognitive therapies	2.38	2.05	0.27
T ₁₃	Hypnotherapy	1.82	1.15	0.26
Average		1.30		

 Table 2. Effect sizes of psychotherapies, behaviour therapies and placebo treatment, according to Smith et al. (1980)

"Psychotherapy benefits people of all ages reliably as schooling educates them, medicine cures them, or business turns a profit" (*ibid*.). "Psychotherapists have a legitimate, though not exclusive, claim, substantiated by controlled research, on those roles in society ... whose responsibility it is to restore to health the sick, the suffering, the alienated, and the disaffected" (p. 184). Are these effects differential? No. "Different types of psychotherapy (verbal or behavioral, psychodynamic, client-centred, or systematic desensitization) do not produce different types or degrees of benefit." (p. 184). (All italics are the authors'.) In other words, as Luborsky et al. (1975) claimed, "everyone has won and all must have prizes"! Are we really in Alice-in-Wonderland country?

Most psychotherapists seem to have agreed with these summaries of the work surveyed, yet the very evidence provided by Smith *et al.* (1980) seems to prove their claims ill-founded. Looking at the average effect sizes of the therapies studied by them, one notices a minimum of 0.14 and a maximum of 2.38, i.e. well over an order of magnitude! That hardly suggests equality of effectiveness. It might be argued that neither the high nor the low values belong to typical psychotherapies, and that is true; cognitive therapies (2.38), systematic desensitization (1.05) and cognitive-behavioural therapy (1.13) are well above the average, but these are all behaviour therapies. So let us look at traditional types of psychotherapy.

Here we have psychodynamic therapy (0.69), Adlerian therapy (0.62), client-centred therapy (0.62), Gestalt therapy (0.64), rational-emotive therapy (0.68), transactional analysis (0.67) and reality therapy (0.14), with an average effect size of 0.65, (if we omit 'reality therapy') with very little evidence of any differences between effect sizes (except for 'reality therapy', for which there are very few studies). So for these *therapies* (which probably make up the great majority of therapies used) the Luborsky summary seems to apply. An effect size of 0.65 overall, seems quite acceptable, but there is an ominous final 'treatment' listed in Table 2, namely 'placebo treatment', with an effect of 0.56! It is a curious aberration to list 'placebo' as a treatment; it should have been regarded as a control condition, and subtracted from the various treatments considered! When this is done, we have a mean effect size of 0.06 (or 0.13 with 'reality therapy' omitted), i.e. hardly noticeable or worthwhile. Prioleau, Murdoch and Brody (1983) agree, in their analysis of placebo effects, that there is no evidence of therapy effects when placebo effects are controlled for.

This is a curious and probably unexpected result, considering the very poor quality of the placebo treatments used, and the fact that the persons administering the placebo treatments would not have *believed* in their effectiveness, while strongly believing in the effectiveness of the therapy proper being administered. Thus the therapy proper would have an *adventitious* placebo effect over and above that of the placebo itself, yet the expected superiority does not seem to have become apparent.

THE EMERGENCE OF BEHAVIOUR THERAPY

The major feature that sets apart behaviour therapy and the various psychotherapies is the dependence of behaviour therapy on a properly developed theoretical basis (Eysenck & Rachman, 1965; Eysenck & Martin, 1987). This point has been stressed recently in a symposium entitled: "From Behaviour Theory to Behaviour Therapy" (Symposium, 1993). It is not suggested that there are not different conceptions of the nature and relevance of the theories in question, it is suggested that such differences should be taken seriously, and their consequences tested. In contrast to the theories underlying the various psychotherapies, such behaviouristic theories are (1) developed clearly enough to lead to testable consequences and (2) are derived from the large corpus of facts and theories which constitute modern learning theory, which provides an academically acceptable basis. We would expect that superior effectiveness of treatments so based would characterize behaviour therapy.

It will already have been apparent in the discussion of the Smith *et al.* (1980) book that behaviour therapy appears significantly higher in effect size than psychotherapy or placebo treatment. The average (1.14) of cognitive-behavioural therapy (1.13), other cognitive therapies (2.38), systematic desensitization (1.05), implosion (0.68), behaviour modification (0.73) and eclectic behavioural therapy (0.89), is almost twice as high as the psychotherapies, in spite of the inclusion here of some doubtful candidates (such as behaviour modification, which is based on a Skinnerian, not a Pavlovian theory). Smith *et al.* (1980) themselves agree that "behaviour therapies were more effective than others for neurosis, true (complex) phobias, and simple (monosymptomatic) phobias. Verbal therapies were not reliably more effective for any diagnostic type" (p. 98). In other words, for the groups for whom the theory of behaviour therapy is intended, the therapy works better than psychotherapy; it is with groups for whom it was not intended that it is not superior.

How then do Smith *et al.* (1980) come to the conclusion that psychotherapy 'works'? They adopt a very simple and ingenious method; they label *all* methods as 'psychotherapy', and subdivide this into 'verbal' and 'behavioural'; thus behaviour therapy is made part of psychotherapy, and its success effectively is counted as successful psychotherapy!

A whole book has recently been addressed to the comparative effectiveness of psychotherapy and behaviour therapy (Giles, 1993), and it would be idle to try and do more than summarize its conclusions, which bear out my general belief that behaviour therapy is clearly and decisively more effective in the cure of neurotic patients than psychotherapy. This summary bears discussion. The first point is that overly general comparisons underestimate the superiority of behaviour therapy. Consider obsessive-compulsive disorder. In our book on behaviour therapy, Rachman and I suggested a learning theory model of such behaviour and its treatment, based on animal studies (Eysenck & Rachman, 1965). This treatment (flooding with response prevention) has been extremely effective in cases of handwashing, cleansing rituals and similar obsessive-compulsive disorders (Rachman & Hodgson, 1980); several replications demonstrated the robustness of the findings, which changed a success rate hovering around the zero mark to something in excess of 90% (Foa & Goldstein, 1978; Meyer, Levy & Schnurer, 1974). Desensitization, in our experience, does not work anything like as effectively. Now consider a study comparing psychotherapy and behaviour therapy. If we treat obsessive-compulsive patients of this kind, the choice of behaviour therapy method becomes decisive; if flooding with response prevention is used, a very massive difference will appear, while if we use desensitization, the difference will be less extreme. Thus comparisons, strictly speaking, are only appropriate if the patient population is specified in detail, and the appropriate method of behaviour therapy used. The observed superiority would be much greater if such points were always kept in mind.

Another problem that has been raised relates to the multitude of factors that may determine the relative outcome success of a given treatment (Beutler, 1991). Beutler looks at potentially important variables, i.e. therapy variables and therapist variables, and concludes that there are nearly, 1,500,000 potential combinations "that must be studied in order to rule out relevant differences among treatment types" (p. 227). He is concerned to indicate the absurdity of the Luborsky "all have won" dictum, but his discussion clearly indicates the insolubility of therapeutic problems in

the absence of *theories*. In the case of the Rachman and Hodgson (1980) study, (1) the patient population is clearly specified by the theory, (2) the method of treatment is clearly specified by the theory and (3) the principle of evaluation is clearly specified by the theory in quantitative form. Therapist differences are clearly immaterial; the method works across therapists (Foa & Goldstein, 1278; Meyer *et al.*, 1974).

A third problem is the adoption by psychotherapists of behavioural methods which may carry the main therapeutic burden while psychotherapy is given the credit. Often this addition is not even mentioned in the description of the method used, and thus quite a false picture is given of the *causes* of any therapeutic success that may be obtained.

One final point. Many authors add the adjective 'cognitive' to 'behaviour therapy', as if that distinguished it from plain behaviour therapy. This represents a misunderstanding of the theory underlying behaviour therapy. As we have pointed out (Eysenck & Martin, 1987), the conditioning theories underlying behaviour therapy *implicity* and *explicitly* contain cognitive elements (as indeed did Pavlov's original work). The old-fashioned and largely irrelevant theories of Watson and Skinner tried to exclude cognition, but modern learning theory has refused to adopt such non-sensical restrictions, and cognitive factors play a very prominent part in recent (and not so recent) theories in the field of conditioning and learning (e.g. Mackintosh, 1984). Critics of behaviour therapy often seem unaware that profound changes have taken place in the past 50 years, changing S–R theories to S–S theories, and emphasizing cognitive factors even in animal learning. To add 'cognitive' to behaviour therapy is either an oxymoron or a redundancy; I would like to suggest it is the latter. *All behaviour therapy is cognitive*; it attempts to alter cognition and behaviour through behavioural determinants. Behaviour therapy is based on the tenets of *modern* learning theory; it is not shackled to ancient shibboleths.

Cognitions, fears and behaviour are clearly highly correlated. If I interpret a bump on the skin as a likely precursor of cancer, this cognition will produce fear and lead me to visit the doctor. We can attack such neurotic (unrealistic) fears and cognitions by either modifying the cognitions directly (cognitive therapy) or indirectly by exposure (behaviour therapy). The main areas in which these two types of treatment have been tried and compared are panic disorders (Barlow, 1988; Beck, 1988; Clark, 1986, 1988, 1993) and phobias (Booth & Rachman, 1992; Rachman & Whittal, 1989; Shafran, Booth & Rachman, 1992), with some works done on obsessional disorders (Arntz, 1992; Salkovskis, 1985) and hypochondriasis (Warwick & Salkovskis, 1990; Viner & Bouman, 1992). Last (1987) and Marks (1987) did not find much evidence to favour cognitive methods of treatment, but more recent studies, like those cited above, have been much more positive.

This work throws much light on the point just made. Cognitions are behaviours of course, as indicated by the inevitable physiological and hormonal accompaniments of thought and emotions, but they may be judged different from physical behaviour as normally understood, and it is possible experimentally to test cognitive theories of neurotic behaviours. Do *purely* cognitive procedures reduce fear? More importantly, does a simple behavioural treatment, like exposure, reduce neurotic cognitions? Cognitive theories would predict the former, but not the latter, behaviour theories would predict both. Rachman (1993) has promising results of comparing cognitive and exposure-based therapy on fear and number of negative cognitions, comparing pre-treatment with post-treatment conditions. Fears and cognitions declined in both conditions, but more so in exposure based treatment. The decline in negative cognitions was equal under conditions of exposure and conditions of cognitive theory. Lee (1992) gives a detailed discussion of cognitive theories and the causation of human behaviour, and Sweet and Loizeaux (1991) provide a critical comparative review of behavioural and cognitive treatment methods.

Given that there is a profound theoretical difference between behaviour therapy and psychotherapy, which should not be glossed over by using the terms psychotherapy, both as a super-ordinate and a subordinate term (there are two kinds of psychotherapy—behaviour therapy and psychotherapy!), we can demonstrate clearly that all claims for psychotherapeutic techniques are based on false premises. The argument goes as follows. A given method of treatment, T_1 , derives from a certain hypothesis, H_2 ; the hypothesis predicts that T_1 gives better results (R_1) than T_2 , T_3 , T_4 ... T_n , based on hypothesis H_2 , H_3 , H_4 ... H_n . If H_1 does not make such a prediction, then H_1 is redundant; other theories and methods of treatment will do just as well, or better. Psychoanalysis is a good

example of H₁; Freud predicted that *no* other method of treatment could equal T₁, which was deduced directly from his general dynamic theory. Similarly, the authors of Gestalt therapy (T₂), Adlerian therapy (T₃), client-centred therapy (T₄), rational-emotive therapy (T₅), and transactional analyses (T₆), claim superiority for their therapies derived from H₂-H₆. Now according to Smith *et al.* (1980), the results of these therapies (R₁-R₆) are as shown in Table 2, i.e. all treatments giving very similar results, and none is significantly better than T₇ (placebo treatment).

Also given in Table 2 are six varieties of behaviour therapy. Actually these do not embody separate theories; they are different methods within one single theory. We might use desensitization for a phobic illness, implosion (flooding with response prevention) for an obsessive-compulsive disorder, cognitive-behavioural for a depression etc., hypnosis might be used in conjunction with other forms of behaviour therapy. Hence the comparison is not strictly speaking a fair one; these are not opposed theories, as are the six psychotherapies. But the comparison will serve to highlight the difference in the *means*, 1.30 vs 0.65. This does *not* mean the behaviour therapy is twice as good as psychotherapy. It means that psychotherapy is no better than placebo treatment, while behaviour therapy is significantly better than either.

Given these figures for the psychotherapies, we may agree with the authors, and with Luborsky *et al.* (1975), that "all have won and all must have prizes". That certainly has been the predominant response to the publication of these data, but it may be entirely erroneous. The data show quite clearly that H_1-H_6 are all mistaken; they predict a superiority for each theory in turn which clearly does not exist; T_1-T_6 fail to show the predicted superiority. But if data fail to agree with prediction, then the theory giving rise to that prediction fails—particularly if the prediction goes to the heart of the theory! So what we should conclude is that none have won, and none may have prizes!

This is true particularly, if we include in our survey T_7 , placebo treatment, whose inclusion is premised on the view that T_1-T_6 are in fact *all* placebo treatments. This theory (T_7) is the only one to be supported by the data so assiduously gathered by Smith *et al.* (1980). If this conclusion be granted, then we may dispense with the use of placebo groups, and all the problems raised ethically and methodologically by their use; we can use any method from T_1-T_6 for our control group, representing *non-specific effects* like any other placebo, but recommended for use because (a) the person giving the treatment *believes* in its effectiveness, (b) the treatment is likely to be believed by the patient and (c) the use of an acknowledged therapy as a placebo does not raise the ethical problems which use of a method defined as non-therapeutic does. Another advantage of using therapeutic techniques as a placebo is that any T_8 suggested to be an effective technique will find it easier to avoid Type 1 errors because now the placebo treatment is allowed maximum effect; most placebo treatments are weak, unbelievable and easily seen through. It speaks volumes that, in spite of all the weaknesses of existing placebo treatments, T_7 is still very close to the mean of T_1-T_6 , 0.56 as opposed to the 0.65, i.e. an effect size of 0.09, less than the average standard error of the mean!

In treating T_1-T_6 seriously, and arguing that they represent genuine scientific theories, I have done them a disservice. As Gossop (1981) has pointed out in his book *Theories of Neurosis*, the theories are often "stated in such imprecise terms that it is difficult to see at which points they are in agreement and at which other points they make different predictions" (p. 137). For the most part, these are not scientific theories in the usual sense, built on a large body of experimental evidence, stated with some degree of precision, and making clear-cut, testable predictions that can be used to assess the value of the theory, by how well these predictions are borne out by the experimental results. Rather, they are vague notions, stated imprecisely, derived from ill-defined, obscure and nebulous observations, difficult or even impossible to test, and resting ultimately on their ability, seldom properly tested, to suggest methods of treatment hopefully giving better results than placebo treatment, or other methods equally vague, deriving from other 'theories' equally ill-defined. Where there is empirical evidence about a theory's prediction, the evidence speaks strongly against the theory, as in the case of psychoanalysis (Kline, 1981; Eysenck & Wilson, 1973).

This universal failure of psychotherapeutic methods to stem from academically reputable theories stands in sharp contrast to the claims of behaviour therapy to derive in essence from modern theories of learning and conditioning, i.e. from a large body of experimental evidence directly relevant to the therapeutic treatment of neurotic patients. It is not suggested that the theory or theories on which behaviour therapy tests are monolithic, unchanging, and laid down on Mount Sinai. Watson and Rayner (1920) and Jones (1924a,b) laid the foundations on which others have built (e.g. Mowrer, 1960a; Wolpe, 1958; Eysenck & Rachman, 1965). I have attempted to remodel Watson's original sketch of a theory, and introduce the many advances which modern learning theory has made into this field (Eysenck, 1967, 1976, 1977, 1978a,b, 1980, 1982a,b; Eysenck & Beech, 1971), and I believe that this new model can account for many or even most of the facts that modern research into behaviour therapy has unearthed. Modern critics of this approach seldom bother to address the recent advances in modern learning theory (Dickinson, 1980; Mackintosh, 1984), or the applications of parts of that theory, to clinical problems (e.g. Davey, 1987, 1989, 1992); most still seem to believe that conditioning theories rely on Pavlovian A type conditioning, rather than B type conditioning (Grant, 1964)!

Therapeutic procedures can be devised along proper lines of logical argument from the relevant aspects of this general theory; it is here that behaviour therapy differs most profoundly from psychotherapy (Eysenck & Martin, 1987). To argue in favour of some form of 'eclecticism' (Lazarus, 1971), or to recommend integration (Wachtel, 1977) is to disregard this vital difference. Chemists do not usually advocate integrating chemistry and alchemy, or basing some 'eclectic' method on elements derived from both. I am not saying that our theories are perfect, our deductions foolproof and our methods of treatment optimal. I am well aware of the many problems, uncertainties, errors and false steps psychological theroy has taken, and I am also well aware of the usefulness of accidental, serendipitous discoveries which have to be integrated into any scientific account. All theories are weak to begin with, encounter anomalies that require explanation, and are finally replaced by better theories. Nevertheless, a beginning has to be made if we are ever to have a proper science, and the denial of the very need for a proper theory that characterizes so much of modern psychotherapy is in essence a rejection of the scientific method.

If H_8 , the conditioning theory underlying T_8 , i.e. behaviour therapy (T_8-T_{13}) , is along the right lines, then it should be more successful than T_1-T_6 , and of course T_7 (placebo treatment). As already mentioned, the Smith *et al.* (1980) meta-analysis shows this quite clearly, as Table 2 has demonstrated. All the methods of behaviour therapy score significantly better than the various psychotherapies.

Similar results were reported for German studies of the effectiveness of psychotherapy. Wittman and Matt (1986) carried out a meta-analysis of studies carried out in the German-speaking countries and found that "not everybody has won, therefore not all must have prizes" (p. 35); in particular, "therapies with behavioural orientation show the highest effects" (p. 35). A meta-analysis of work with children (Petermann & Beckmann, 1993) also give evidence of the effectiveness of behavioural therapy.

Grawe (1992) gives a table summarizing the outcomes of three large meta-analyses comparing the outcome of dynamic-humanistic and behavioural-cognitive methods. All show similar differences favouring the behavioural-cognitive methods; the means are, respectively, 0.44 and 1.00, giving the dynamic-humanistic approach a success rate very much below the placebo value given by Smith *et al.* (1980). Yet Grawe still advocates the employment of these methods!

Looking at treatment for criminal and psychopathic behaviour, Andrews, Zinger, Hoge, Bonta, Gendreau and Cullen (1990) conclude their analysis of published work by saying that "appropriate types of service ... involve the use of behaviour and social learning principles of interpersonal influence, skill enhancement, and cognitive change. Specifically, they include modelling, graduated practice, rehearsal, role-playing, reinforcement, resource provision, and detailed verbal guidance and explanation" (p. 375). They also point out that "traditional psychodynamic and non-directive client-centred therapies are to be avoided" (p. 376). Here too there is clear superiority in effectiveness for behaviour therapy over psychotherapy.

Psychosomatic diseases provided another example. Grawe (1992) provides ample evidence to show that psychotherapy of the traditional variety is practically useless (effect size is 0.16!) in the treatment of psychosomatic patients, as compared with other much more effective methods. Yet 98% of medical therapists and 72% of psychologists used depth-psychological methods! As he says, "methods of treatment which have the best record of efficacy are almost completely displaced in the provision of psychotherapeutic help. It would take considerable argumentative ability to justify these conditions in terms of the interests of the patients (p. 141). And he goes on to state what I believe is true over a much wider area: "The simple truth is that the supply of psychotherapeutic

help nowadays is still determined by the personal preferences and interests of the therapists" (p. 141). There appears to be no way in which the facts unearthed by scientific research can be made to bear on the actual practices of therapists.

In the field of psychosomatic diseases, and really serious physical diseases like cancer and coronary heart disease, there is good evidence of the value of behavioristic psychological treatment methods for prophylaxis, and for prolongation of life in cases of incurable disease (Eysenck, 1991b). Equally, there is evidence that Freudian psychotherapy has a *negative* effect (Grossarth-Maticek & Eysenck, 1990). Even here the difference in effectiveness between psychotherapy and behaviour therapy is apparent.

All these studies should serve once and for all to remove the "myth of psychotherapy" (Szasz, 1979) and the cult of an inherently anti-scientific methodology (Eysenck, 1992b). Much more material supporting the superiority of behaviour therapy over psychotherapy will be found in Giles, Neims and Prial (1993) and Giles' (1993) *Handbook of Effective Psychotherapy*; in general, it would seem unrealistic to doubt this superiority any longer.

What I am suggesting is this. The true, effective and 'specific' parts of therapy are those deduced from and suggested by the theories of conditioning and learning, such as relaxation, desensitization, modelling, suggestion, flooding with response prevention etc. Time honoured common-sense methods used by teachers, relatives, priests and friends to alleviate emotional troubles work in so far as they employ any of these techniques, albeit unthinkingly. Hence the success of 'spontaneous remission' and of placebo treatment. Psychotherapy works for the same reason, being in effect no more and no less than a placebo treatment. It follows that the success rates of spontaneous remission, placebo treatment and psychotherapy will vary according to the extent to which each incorporates the relevant principles of behaviour therapy; there is no scientific meaning attaching to any meta-analytic average. Only behaviour therapy, applying these principles deliberately systematically and in a planned fashion, has a significantly superior success rate, but no doubt that rate will be drastically improved as we succeed in sharpening up our theories and improving our methods (Eysenck, 1980).

There are still some critics who would deny the outstanding success rate of behaviour therapy *when properly applied* (Wolpe, 1986, 1989); as Wilkins (1986) has shown, these arguments are usually based on theoretical misconceptions and misreadings of the literature. Readers of the literature should be careful to assess the truth-value of arguments playing down the efficacy of behavioural methods (Giles, 1984).

SUMMARY AND CONCLUSIONS

The general tenor of the evidence produced in recent years seems to be that the conclusion of my 1952 article is still valid: psychotherapy works, as far as it does, by means of non-specific or placebo effects. It is not derived from clearly formulated theories, nor supported by observed superiority of effectiveness. It is clearly inferior to behaviour therapy in effectiveness, and in the much greater sophistication of the theories on which behaviour therapy is based, and the large empirical support for these theories. This outcome of many hundreds of studies raises important problems and questions, relating, respectively, to: (a) the ethics of teaching, advocating and employing methods of therapy that are clearly no more effective than placebo treatments, or no treatment at all, and that are time-consuming and expensive; and (b) the cost-effectiveness of such methods, as compared with shorter and significantly more effective methods, available at lower cost.

As an example to illustrate these two sets of problems, consider enuresis. As Kaplan and Busner (1993) have shown, "the weight of reviewed evidence clearly favours the bell-and-pad either alone or in combination with other behavioural interventions. It is an ironic commentary on the sociology of knowledge that < 5% of American primary care practitioners prescribe the bell-and-pad for enuretic patients (Rushton, 1989), and, despite the absence of any evidence supporting the efficacy of the approach, 73% of 196 therapist-members of the American Association for Marriage and Family Therapy, when surveyed, chose individual psychotherapy or family therapy over behavioural conditioning as the treatment of choice for bed-wetting (Wagner & Hicks-Jimenez, 1986,

p. 147). How can we justify an educational, medical and social system that leads to a complete disregard of an effective, cheap, quick-acting method in favour of a lengthy, costly and useless method for the treatment of a very common psychological disorder? Is it ethically justifiable to trick parents, against the evidence, to pay for a method of treatment that is demonstrably inferior to a much cheaper and quicker method? How does the state (in England, which has a National Health Service) or the insurance companies (in the U.S.A. or Germany) justify wasting money in this fashion?

Another good example of the irrational nature of the present psychiatric system is the treatment of alcoholism. Brengelmann and Fahrner (1992) have told the history of the founding of the first behavioural hospital in Germany devoted to the treatment of alcoholism and addiction generally. The use of behavioural methods *doubled* the success rate of treatment, as measured by 1 year of abstinence. Many other behavioural hospitals have since followed, but still the major type of treatment is the relatively unsuccessful type of psychotherapy typical of the years before the war. Success and failure of different types of treatment seem to have little influence on practitioners.

These examples can with advantage be extended to more serious neurotic complaints, where the long, expensive and ineffective methods of psychotherapy are likely to cause considerable distress to the patients, and may quite frequently make the patient worse rather than better (Mays & Franks, 1985; Eysenck, 1985). There is no way this can be justified or defended on ethical grounds, and the Hippocratic Oath becomes a hypocritical oath when treatment is administered on a self-serving rather than a patient-oriented basis.

The use of psychotherapy is sometimes defended on the basis that through its use more knowledge will be acquired that will improve effectiveness. This is the moral equivalent to Moertel's (1978) advocacy of conventional chemotherapy, including 5-fluorouracil, in spite of its admitted uselessness, in the treatment of gastrointestinal cancer. This curious advocacy of a painful and dangerous type of treatment admitted to be useless, was justified on the curious grounds that "patients with advanced gastrointestinal cancer and their families have a compelling need for a basis of hope. If such hope is not offered, they will quickly seek it from the hands of quacks and charlatans". May heaven protect us from such as Moertel, who would use debilitating methods of proven uselessness to give 'hope' to his unfortunate patients! [See Richards (1991) for a detailed discussion of the controversy of which this is just one aspect. It will serve as a good introduction to the area she labels "Medicine or Politics?" in the sub-title of her book.]

Moertel uses his argument to discourage not only the use, but even research into Linus Pauling's vitamin C treatment of cancer, which he regards a 'quackery'. I would suggest that the use of 'dynamic' and other psychotherapeutic methods lacking proven usefulness is as equally inadmissible as Moertel's practice. Where superior, cheaper and quicker-acting methods of therapy are available, the continued use of expensive, lengthy and non-effective methods is unethical, and contrary to the best traditions of medicine. There has been a paradigm shift from psychotherapy to behaviour therapy, and it is time this shift was reflected in treatment choice (Fishman, Rotgers & Franks, 1988; Eysenck, 1988).

In recent years the cost-effectiveness question has joined the ethical discussion in considering the respective merits of psychotherapy and behaviour therapy (Andrews, 1993; Pekarik, 1993). Both voice dissatisfaction with the continued funding of treatment using outmoded methods of psychotherapy, and suggest reliance on behavioural methods. Governments about to be submerged by ever escalating costs of medical treatment have been forced to consider carefully the cost-effectiveness of all treatments; this alone should shift the balance decisively from psychotherapy to behaviour therapy. It is time psychoanalysis and psychotherapy joined phlogiston on the list of scientific theories that misled whole generations, only to be thrown overboard when their essential emptiness was discovered, and better theories took their place.

All I have said is of course relevant to the vexed question of science-practice relationship and professional training, which provides the exemplars of Kuhn's disciplinary matrix. The Boulder model (Raimy, 1950) of the scientist-practitioner has received much praise but little implementation (Thelen & Ewing, 1970; Kimble, 1984; Meltzoff, 1984; Franks, 1984, Peterson, 1985; Hoskmand & Polkinghorne, 1992). Practising psychologists tend to know little of the science that legitimises their work; this is another problem in the paradigm shift that is needed to make treatment more scientific and effective. How can one convince a practitioner brought up in the old

psychotherapy tradition of the relevance and importance of modern learning theory, if he or she is in fact ignorant of the literature and unwilling to study it? Kuhn's model is clearly very apposite to the present situation in applied psychology: one might wish that he had indicated how such paradigm shifts can best be accomplished!

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