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The outcome problem in psychotherapy

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Introduction

As I have pointed out in two autobiographical sketches (Eysenck 1990a; 1990b), my connection with psychotherapy has been along three main lines. The first was an initiation of the debate, still continuing, concerning the efficacy of psychotherapy; the second was playing a part in the establishment of behaviour therapy, and writing its first textbook (Eysenck and Rachman 1965); and the third was the setting up of clinical psychology as a profession in the United Kingdom. These three contributions are not of course independent. The first step in the sequence was my being given the task of setting up clinical psychology as a profession in the United Kingdom; as a result I got interested in the degree to which psychotherapy might be considered to be useful in the treatment of neurosis, and as a consequence of my discovery that there was no evidence to support the view that psychotherapy had any beneficial consequences, as opposed to no treatment, or placebo treatment, I was led to investigate the possibilities of behaviour therapy, originally adumbrated by Watson and Rayner (1920), Jones (1924) and others, as discussed by Kazdin (1978) and Schorr (1984). Thus in my connection with clinical psychology the outcome problem was absolutely crucial, and my feeling was and is that this problem has never been properly addressed by clinical psychologists, and forms the weakest part of our argument to be a socially useful profession.

In the late 1940s and early 1950s, when I became involved in this field, certain things were generally taken for granted. It was widely believed and

taught that psychoanalysis was the only acceptable method of treatment for neurotic patients, being the only method that concerned itself with *causes* rather than merely with *symptoms*; that symptomatic treatment might be superficially successful, but that it would soon be followed by a recurrence of symptoms, or symptom substitution; that 'deep', 'psychodynamic' and long-lasting investigations by trained psychoanalysts were required to produce stable and long-lasting cures; and that such cures could be effected only by analysts who had themselves been psychoanalysed. Diagnoses and cures might be helped by psychodynamically oriented projection tests, like the Rorschach or the Thematic Apperception Test (TAT); the emphasis of both tests and treatment were unconscious causes, transference phenomena, and early childhood experiences reconstructed through analysis of dreams and other types of 'dynamic' evidence.

Few knowledgeable psychologists or psychiatrists would now deny that there was no objective evidence for any of these beliefs. There were no clinical studies comparing the progress of neurotic patients under psychoanalysis with that of similar patients receiving no treatment, or placebo treatment; it was often suggested that it would be unethical to withhold such obviously beneficial treatment as psychoanalysis from patients, disregarding the fact that of all the people suffering from severe neurotic illness in the United States, less than 0.01 per cent would in fact receive psychoanalytic treatment! The superiority of psychoanalysis was simply assumed on the basis of pseudo-scientific arguments, without there being any evidence in its favour. It was often suggested that the cases treated and described by Freud provided such evidence, but apart from the obvious point that there were no controls involved in his work, and no follow-ups, it is now well known that Freud was very economical with the truth, as far as description of his famous cases is concerned, and that the alleged 'cures' in fact were not cures at all (Eysenck 1985b). Thus the famous 'Wolf Man' was not in fact cured as claimed, but continued with the self-same symptoms from which Freud claimed to have relieved him for the next sixty years of his life, being under constant treatment during this time (Obholzer 1982). Similarly, the famous 'cure' of Anna O. by Breuer, which was supposed to constitute the beginnings of psychoanalytic treatment, was shown by historians to have been a misdiagnosis and the 'cure' a fraud (Thornton 1983). Anna O. was not a hysteric, but suffered from tuberculous meningitis: she was not cured, but lived for many years with the self-same symptoms in a hospital (Hirschmuller 1989). The 'Rat Man' was far from a therapeutic success as claimed, and Freud's process notes deviate notably from his final account (Mahony 1986). It would be difficult to adduce these and other cases treated by Freud as evidence of psychoanalytic successes (Eysenck 1985b).

The outcome problem, then, was being completely disregarded, and the only kind of clinical studies to be done were concerned with problems internal to the analytic process, it being assumed that psychoanalysis was not only the best, but also the only method of treatment. It seemed to me from the beginning that this was entirely the wrong way to look at the whole problem of psychotherapy. Both from the theoretical and the practical points of view, the outcome problem was the most important and critical of all; if psychoanalysis, or psychotherapy in general, which was then as it is now, largely based on Freudian assumptions, did not in fact do better than placebo treatments or no treatment at all, then clearly the theory on which it is based was wrong. Similarly, if there were no positive effects of psychoanalysis as a therapy, then it would be completely unethical to apply this method to patients, to charge them money for such treatment, or to train therapists in these unsuccessful methods. The practical importance of the issue will be clear without any great discussion, but the theoretical implications have been debated for a long time; it has been asserted that even if the therapy itself did not work, the theory might nevertheless be correct. I took it for granted that if a treatment method based on theory does not work, then this suggests that the theory itself must be mistaken. Grunbaum (1984) has argued the case in considerable detail, and has come to a very similar conclusion. I shall return to this point presently.

Effectiveness of psychoanalysis

In order to satisfy myself concerning the alleged efficacy of psychoanalysis, I carried out an analysis of all the published material concerning recovery from neurotic illness after psychoanalysis, after psychotherapy, and after no treatment of a psychiatric nature at all; the results were published in an article which has been referred to as 'the most influential critical evaluation of psychotherapy' (Kazdin 1978: 33). In this article (Eysenck 1952) I examined a number of outcome studies that primarily evaluated treatment of neurotic patients. I attempted to assess the effects of psychotherapy by comparing its outcome with an estimate of improvements in patients that occurred in the absence of therapy. I concluded that approximately 67 per cent of seriously ill neurotic patients recover within two years, even in the absence of formal psychotherapy. If we regard this as an approximate baseline against which treatment can be evaluated, then we can compare therapy outcome with it, and I found a cure rate of approximately the same magnitude. Thus remissions with no treatment ('spontaneous remission') appear to be as effective as psychotherapy and psychoanalysis. The dissatisfaction with psychotherapy expressed in this conclusion was shared by many others who published similar articles around the same time (e.g. Denker 1946; Landis 1937; Salter 1952; Wilder 1945; Zubin 1953), all of whom came to very similar conclusions, although they tended to express them less definitively and perhaps less clearly than I had done.

The paper produced a plethora of replies, all of which, interestingly enough, criticized me for saying something I had not said, namely that the evidence proved psychoanalysis and psychotherapy to be ineffective. The quality of the papers surveyed, it was stated, was too poor to allow such a conclusion to be substantiated. This, of course, is true; the evidence is methodologically and statistically inadequate, but my conclusion had been rather different,

namely that the evidence was *not sufficient to prove that psychoanalysis and psychotherapy were instrumental in mediating recovery.* The poorer the evidence, the stronger this conclusion is; if the studies surveyed are so poor that no conclusions can be drawn, then they cannot be used to support the idea that psychoanalysis and psychotherapy have a positive effect!

In case it might be thought that I am making a special case for my own view, or that the issue is not a serious and important one, readers are recommended to read an article by Erwin (1980) who, as a trained philosopher, has exhaustively looked at the argument (also treated in his previous book: Erwin 1978). His conclusion agrees with me that the issue is important, and that the critics of my original thesis were mistaken.

It is interesting, in view of other criticisms that I have to make, that the critics of my paper had to resort to misrepresentation in order to enable them to put up some kind of argument. It is also interesting that neither the referees nor the editors of the journals concerned noted this misrepresentation.

I took up the topic again in 1960 and in 1965, summarizing the large number of articles that had appeared, partly as a consequence of the stress on the importance of the outcome problem which we had insisted on. The results were not very dissimilar, as the following quotation from my 1960 paper will illustrate:

1. When untreated neurotic control groups are compared with experimental groups of neurotic patients treated by means of psychotherapy, both groups recover to approximately the same extent.
2. When soldiers who have suffered a neurotic breakdown and have not received psychotherapy are compared with soldiers who have received psychotherapy, the chances of the two groups returning to duty are approximately equal.
3. When neurotic soldiers are separated from the service, their chances of recovery are not affected by their receiving or not receiving psychotherapy.
4. Civilian neurotics who are treated by psychotherapy recover or improve to approximately the same extent as similar neurotics receiving no psychotherapy.
5. Children suffering from emotional disorders and treated by psychotherapy recover or improve to approximately the same extent as similar children not receiving psychotherapy.
6. Neurotic patients treated by means of psychotherapeutic procedures based on learning theory improve significantly more quickly than do patients treated by means of psychoanalytic or eclectic psychotherapy, or not treated by psychotherapy at all.
7. Neurotic patients treated by psychoanalytic psychotherapy do not improve more quickly than patients treated by means of eclectic psychotherapy and may improve less quickly when account is taken of the large proportion of patients breaking off treatment.
8. With the single exception of the psychotherapeutic methods based

on learning theory, results of published research with military and civilian neurotics, and with both adults and children, suggest that the therapeutic effects of psychotherapy are small or non-existent and do not in any demonstrable way add to the non-specific effects of routine medical treatment, or to such events as occur in the patients' everyday experience.

(Eysenck 1960: 719–20)

In these writings I made a number of other points, which have not always been considered by critics. Thus I emphasized the need for carefully controlled therapy research that not only took into account spontaneous remission, but also controlled for non-specific treatment effects, that is the inclusion of placebo treatment in assessment. I also pointed out that showing that therapy is superior to no treatment is not sufficient to demonstrate that any particular technique or ingredient of therapy is effective; non-specific treatment effects such as attending treatment and meeting with the therapist would still have to be ruled out to argue for specific benefits of treatment (Eysenck 1966). Arguments for and against were taken up by many contributors whose points of view are summarized by Kazdin (1978) and Schorr (1984). It would not be useful to take up these arguments here again, but it may be worthwhile to look at more recent surveys of the burgeoning literature, and try and see to what extent more recent studies have validated or invalidated my original conclusions.

Before turning to this task, however, let us consider the degree to which psychoanalysts have responded to the widespread criticism voiced concerning the efficacy of their treatment. Following the principle that criticism should be directed at what is regarded the best, rather than the worst project in the area, we may look at the Menninger Clinic project (Kernberg 1972; 1973), a lavishly financed study published after eighteen years of work. The aim of the project was to 'explore changes brought about in patients by psychoanalytically oriented psychotherapies and psychoanalysis' (Kernberg 1972: 3). Forty-two adult neurotic patients were studied, those in psychoanalytic therapy receiving an average of 835 hours of treatment, and those in psychoanalytically oriented psychotherapy receiving an average of 289 hours. A detailed criticism of the project has been made by Rachman and Wilson (1980). Listing such obvious faults as contamination, non-random allocation, absence of any control, and so on, we find that the authors of the Menninger report themselves admit that the most severe limitation of their study was its 'lack of formal experimental design' (Kernberg 1972: 76). They point out that it was not possible:

(i) To list the variables needed to test the theories;
(ii) To have methods of quantification of the variables, preferably existing scales which would have adequate reliability and validity;
(iii) To be able to choose and provide controlled conditions which would rule out alternative explanations for the results.
(iv) To state the hypothesis to be tested; or finally,
(v) To conduct the research according to the design.

(Kernberg 1972: 75)

As Rachman and Wilson correctly point out,

This astonishing conclusion can have few equals . . . one is left with a study that is so flawed as to preclude any conclusions whatsoever. Whilst the honesty of this self-appraisal is highly commendable, one cannot help wondering how the authors succeeded in persuading large and reputable foundations to provide them with financial support extending over many years. How does one persuade a foundation to uphold research which, in the words of the authors themselves, lacks a formal experimental design, or methods of quantification, or hypotheses? And which cannot be conducted 'according to the design'?

(Rachman and Wilson 1980: 73)

(Over $1 million was spent on the project, at a time when this was a considerable amount of money.)

Did this waste of time and money induce in the authors of the report a suitable feeling of humility? Malan (1976: 21) states: 'When I met Dr. Kernberg at the meeting of the Society for Psychotherapy Research in Philadelphia, in 1973, he said that the problem of measuring outcome on psychodynamic criteria was essentially solved, and I could only agree with him.' It is difficult not to resort to a quotation: 'Quem Jupiter vult perdere dementat prius' (Whom Jupiter wants to destroy, he first renders mad). Clearly psychoanalysts have learned nothing and forgotten nothing! They have no intention of submitting their beliefs to any kind of empirical proof: they prefer assertion to demonstration. Anyone seriously interested in this topic ought to read the detailed critique of Rachman and Wilson (1980) to discover how far intellectual vacuity can go.

It is sometimes said that no proper clinical trial of psychoanalytic therapy has been carried out because of the expense involved. This is not true. The Menninger study failed because of incompetence, complacency and a daunting lack of elementary methodological sophistication, not because of lack of funding. Or consider another example. Many years ago a large grant-giving body offered Sir Aubrey Lewis $1 million to organize a clinical trial to compare psychoanalytic treatment and behaviour therapy. He asked the Tavistock Clinic, the leading psychoanalytic institution in Britain, to take part, offering them a leading role in the design and organization of the clinical trials, and the evaluation process. (Sir Aubrey was quite neutral between the rival claims made at the time for these methods of treatment.) The Tavistock Clinic turned down the offer, presumably because they feared the outcome would be unfavourable to their claims. My colleagues and I welcomed it, but of course the offer was contingent on both sides agreeing, and was withdrawn when no psychoanalysts could be found to take up the challenge.

Am I being too hard in my interpretation? As Anthony Storr, one of the best known psychoanalysts in Britain, wrote in 1966:

> The American Psychoanalytic Association, who might be supposed to be prejudiced in favour of their own speciality, undertook a survey to test the efficacy of psychoanalysis. The results obtained were so disappointing that they were withheld from publication. . . . The evidence that psychoanalysis cures anyone of anything is so shaky as to be practically non-existent.
>
> (quoted by Wood 1990)

If a soi-disant 'scientific' organization can behave in this fashion to prevent the public from knowing that their 'science' was worthless, we should not be surprised at anything done by psychoanalysts to protect their religion.

Rachman and Wilson (1980) provide us with so far the best and most honest survey of the literature, and it is reassuring that their verdict is not very different from that I arrived at thirty years before. A quotation makes clear their overall evaluation:

> The need for strict evaluations of the effects of various forms of therapy arises from several observations. In the first place, there is clear evidence of a substantial remission; as a result any therapeutic procedure must be shown to be superior to 'non-professional' processes of change. Closely allied to this point is the wide range of therapeutic procedures currently on offer and the competing and often exclusive claims for effectiveness. The lengthy business of separating the wheat from the chaff can only be accomplished by the introduction of strict and rational forms of evaluation. One important function of strict evaluation would be to root out those ineffective or even harmful methods that are being recommended. The availability of incisive methods of evaluation might have averted the sorry episode during which coma treatment was given to a large number of hopeful but undiscriminating patients.
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> The occurrence of spontaneous remissions of neurotic disorders provided a foundation stone for Eysenck's (1952) sceptical evaluation of the case for psychotherapy. His analysis of the admittedly insufficient data at the time led Eysenck to accept as the best available estimate the figure that roughly two-thirds of all neurotic disorders will remit spontaneously within 2 years of onset. Our review of the evidence that has accumulated during the past 25 years does not put us in a position to revise Eysenck's original estimate, but there is a strong case for refining his estimate for each of a group of different neurotic disorders; the early assumption of uniformity of spontaneous remission rates among different disorders is increasingly difficult to defend.
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> Given the widespread occurrence of spontaneous remissions, and it is difficult to see how they can any longer be denied, the claims made for the specific value of particular forms of psychotherapy begin to look exaggerated. It comes as a surprise to find how meagre is the evidence to support the wide-ranging claims made or implied by psychoanalytic therapists. The lengthy descriptions of spectacular improvements achieved in particular cases are outnumbered by the descriptions of patients whose analyses appear to be interminable. More important, however, is the rarity of any form of controlled evaluation of the effects of psychoanalysis. We are unaware of any methodical study of this kind which has taken adequate account of spontaneous changes or, more importantly, of the contribution of non-specific therapeutic influences such as placebo effects, expectancy, and so on. In view of the ambitiousness, scope, and influence of psychoanalysis, one might be inclined to recommend to one's scientific colleagues an attitude of continuing patience, but for the fact that insufficient progress has been made in either acknowledging the need for stringent scientific evaluations or in establishing criteria of outcome that are even half-way satisfactory. One suspects, however, that consumer groups will prove to be far less patient when they finally undertake an examination of the evidence on which the claims of psychoanalytic effectiveness now rest.
>
> (Rachman and Wilson 1980: 259)

The rather negative evaluation of psychoanalysis, and indeed other forms of psychotherapy, to which Rachman and Wilson are finally forced contrasts spectacularly with conclusions arrived at by authors like Bergin (1971), Bergin and Lambert (1978) and Luborsky *et al.* (1975). The latter, in a memorable phrase, summarized their comparative studies of psychotherapies in the quotation that 'Everyone has won, and all must have prizes'. This will illustrate the *Alice's Adventures in Wonderland* atmosphere of this whole field, as do the equally optimistic conclusions of Bergin and his colleagues.

Let us first consider the argument advanced by Luborsky and his colleagues, which is also put forward by Smith *et al.* (1980) in a meta-analysis of all published data, to the effect that all types of therapy are equally effective, and that this proves the correctness of the views of those who support the effectiveness of psychotherapeutic research, and the theories on which this is based. Let us assume that it is true that different methods of psychotherapy (let us call them T_1, T_2, $T_3 \ldots T_n$) have indeed been shown to be equally effective in reducing or abolishing the neurotic illnesses for which they have been recommended. It should be obvious that such an outcome would not support the hypotheses or theories on which the treatments were based (let us call them H_1, H_2, $H_3 \ldots H_n$), but would completely disprove them. Let us consider psychoanalysis as T_1. This is based on H_1, which asserts, as we have seen, that only psychoanalytic methods can produce a proper cure, and that all other methods must inevitably fail to do so. But according to Luborsky, T_2, $T_3 \ldots T_n$ are equally successful as T_1; this clearly demonstrates that H_1 is incorrect, because it predicted the opposite, namely that T_2, $T_3 \ldots T_n$ would have no effect, or at most, a markedly weaker effect, than T_1.

Much the same can be said for all the other types of treatment – client-centred, Gestalt, 'primal therapy', etc. They are all based on specific hypotheses which would assert that the respective methods of treatment should be superior to all others; if they are not, then surely the theories themselves cannot be correct. If it can also be shown, as we shall see, that placebo treatments are as effective as genuine treatments, then it should become plain that the outcome of all these studies must be that it is non-specific factors, such as discussing one's troubles with a friendly person, receiving advice, relieving one's tensions through receiving positive reactions, etc. which are effective in mediating therapeutic success, rather than the specific methods derived from the various theories in question. If indeed all have won, and all must have prizes, then that surely spells the definite rebuttal to all the theories psychotherapists have fought so earnestly to elaborate and establish.

As far as Bergin and Lambert (1978) are concerned, their main argument rests on the assertion that Eysenck's original suggestion of a spontaneous remission rate of about two-thirds is incorrect, and that this figure should be very much lower. As they say, '*It can be noted that the two-thirds estimate is not only unrepresentative but is actually the most unrealistic figure for describing the spontaneous remission rate or even rates for minimal treatment outcomes*' (Bergin and Lambert 1978: 147, original emphasis).

Eysenck's estimate had been based on data published by Landis (1937) and Denker (1946), which gave an estimate of spontaneous remission effects of something like two-thirds; Bergin (1971) compiled a table containing fourteen studies, and provided percentage improvement rates for each. The rates vary from 0 per cent to 56 per cent and 'the median rate appears to be in the vicinity of 30 per cent!' Although his figures 'have their weaknesses', Bergin nevertheless felt that 'they are the best available to date' and rest 'upon a much more solid base' than the Landis–Denker data. In the face of such a large discrepancy, which is obviously vital in coming to any conclusions, it is essential to study the figures and arguments in detail. This has been done very carefully by Rachman and Wilson (1980), and the reader is referred to their discussion. We shall quote only brief excerpts to illustrate the essential dishonesty of the Bergin and Lambert argument:

> Before commencing the close examination of what Bergin presents as the best available data, two points should be borne in mind. In the first place it seems to be a curious procedure in which one rediscovers data and then calculates a median rate of improvement, while ignoring the data on which the original argument was based. The new data (actually some of them are chronologically older than those of Landis–Denker) should have been considered in conjunction with, or at least in the light of, the existing information. The second point is that although Bergin considered some new evidence, he missed a number of more satisfactory, and indeed more recent, studies which are more pertinent to the question of spontaneous recovery rates. His estimate of a 30% spontaneous recovery rate is based on the fourteen studies which are incorporated in Table 8 of his work. It will be noticed that the list omits some of the studies discussed earlier in this chapter, which antedate Bergin's review.
>
> (Rachman and Wilson 1980: 41)

Rachman and Wilson now give a list of the fourteen studies cited in Bergin's review, and then go on to a detailed discussion of each. Let us consider as an example the spontaneous remission rate of 0 per cent given by Bergin for a study by Cappon (1964). Here is what Rachman and Wilson have to say about this study:

> The first surprise is its title – 'Results of psychotherapy'. Cappon reports on a population consisting of 201 consecutive private patients 'who underwent therapy between 1955 and 1960'. Their diagnoses were: psychoneurosis 56%, psychopathic personality 25%, psychosomatic reactions 8%, and others 3%. As 163 had ended their therapy in 1960, 'this was the operative sample'. Cappon describes his treatment as being 'applied Jungian'. The results of the treatment were 'admittedly modest', and the follow-up was conducted by mail. Unfortunately, only 53% of the patients returned their forms, and the follow-up period varied from 4 to 68 months. In addition, the follow-up sample 'was biased in that these patients did twice as well at the end of therapy, as rated by the therapist, as those who did not return the forms'. It was also noted that 'the operative patient sample (n = 158) was still different (sicker) from a controlled normal sample, at the time of the follow-up. Patients showed more than 4 times the symptoms of normals. This ensured the fact that the sample was indeed composed of patients'.
>
> Cappon states that 'the intention of this work was not so much to prove that results were actually due to psychotherapy as to show some of the relationships results. Consequently, there was no obsessive preoccupation with "controls" as the sine qua non dictate of science.' We seem in the midst of all this to have strayed from the subject of spontaneous remissions. In fact, Cappon did make some brief comments on the subject. He argued that 'if worsening rather than improvement were rated, 4 to 15 times as many patients changed (got worse) in the follow-up (control) period combined with the therapeutic (experimental) period, depending on the index used'. As the follow-up period averaged some 20 months and the therapeutic period some 6½ months, 'this fact alone casts great doubt on Eysenck's data on spontaneous remission which led him to the false conclusion that patients did better without treatment than with treatment'. Leaving aside the fact that Cappon unfortunately lost approximately half of his sample between termination of treatment and follow-up, we can perhaps leave uncontested his conclusion that many of the patients got worse after treatment. Cappon's report adds slender support to the belief that some patients get worse after psychotherapy. It tells us nothing at all about spontaneous remission rates, and far from giving a spontaneous remission rate of 0%, Cappon does not provide *any figures* on which to calculate a rate of spontaneous remission.
>
> Bergin's figure of a 0% spontaneous remission rate appears to be

drawn from Cappon's introductory description of his patients, in which he says that they 'had their presenting or main problem or dysfunction for an *average of 15 years* before the treatment' (original italics). Clearly, one cannot use this single-sentence description in attempting to trace the course of neurotic disorders or to determine their spontaneous remission rate. Nearly half of Cappon's patients apparently had disorders other than neurotic; we are not aware that they had been untreated prior to attending Cappon; we cannot assume that their diagnosis at the beginning of treatment would correspond with their condition in the years prior to treatment; we do not know whether the 201 patients constitute 90% of the relevant population or even 0.00001% of that population. Without labouring the point, this incidental sentence cannot be taken as evidence for or against the occurrence of spontaneous remissions. Bergin's use of the information is unjustified. His introduction of Cappon's report, coming from someone who complains of the 'irrelevance' and 'inadequacy' of the studies by Landis, Shepherd, and others, is baffling. In any event, the occurrence of therapeutic failures, and of a large minority (33%?) of unremitting neuroses, are consistent with the Eysenckian argument. A special collection of therapeutic failures no more demonstrates a spontaneous remission of 0% than a similar collection of patients who have recovered without treatment (easy to compile) would demonstrate a spontaneous remission rate of 100%. The matter rests on the proportion of neurotic patients who show marked improvements within 2 years of the onset of their disorder – or if one prefers a longer or shorter period of study, then a modified hypothesis can be put forward.

(Rachman and Wilson 1980: 41)

Rachman and Wilson continue:

> Bergin also gives a 0% spontaneous remission rate for the paper by O'Connor *et al.* (1964). Once again, the title – 'The effects of psychotherapy on the course of ulcerative colitis' – is surprising as the subject under discussion is the spontaneous remission rate in neurotic disorders. Ulcerative colitis is defined by O'Connor and his co-authors as 'a chronic non-specific disease characterized by inflammation and ulceration of the colon and accompanied by systemic manifestations'.
>
> (Rachman and Wilson 1980: 42)

According to O'Connor *et al.* (1964)

> 'its course is marked by remissions and exacerbations, its aetiology is considered multifactorial, and it has been variously attributed to infections, genetic, vascular, allergic and psychological phenomena'.
>
> (quoted in Rachman and Wilson 1980: 42)

Rachman and Wilson further note the following:

> It will not pass unnoticed that 'psychological phenomena' are only one in a list of five types of attribution, nor indeed, that the course of the

> disease is 'marked by remissions'. The observation that patients who have ulcerative colitis can show remissions is of interest to gastroenterologists. The study compares the progress made by fifty-seven patients with colitis who received psychotherapy and fifty-seven patients who received no such treatment. The patients in both groups continued to receive medical and even surgical treatment, and those who had psychotherapy are said to have progressed better. In the treated group, '19 patients were diagnosed as schizophrenic, 3 were psychoneurotic, 34 were diagnosed as having personality disorders, and 1 received no diagnosis'. In the control group, however, '3 of the patients were diagnosed as schizophrenic, 3 as psychoneurotic, and 14 as having personality disorders. The remaining 37 control patients were not diagnosed because of the lack of overt psychiatric symptoms'. As only three of the control group were diagnosed as psychoneurotic, the spontaneous remission rate over the fifteen-year period would have to be expressed as the number of spontaneous remissions for a group with an N of 3. Bergin's use of the data in this report also raises a methodological point. He quotes the spontaneous remission rate for colitis patients as 0 per cent over fifteen years. In fact no percentage rate can be obtained from the report as all the results are given as group means – it is possible, and indeed likely, that numbers of patients experienced remission even though the *group* mean showed little change. The study leaves us in no position to determine the spontaneous remission rate in three neurotic patients with ulcerative colitis.
>
> (Rachman and Wilson 1980: 42–3)

Another report quoted by Bergin is equally irrelevant as Rachman and Wilson report thus:

> Orgel's (1958) report on fifteen *treated* cases of peptic ulcer is quoted as showing a 0% remission rate. Bergin appears to argue that because the patients had suffered from stomach ulcers for 4 to 15 years prior to entering treatment, this indicates a remission rate of 0. Factually, Bergin is incorrect in stating that the peptic ulcers 'had persisted from 4 to 15 years without change'. Several of the patients had experienced remissions prior to entering psychoanalytic treatment. Furthermore, some of them experienced remissions and recurrences *during* the treatment. Far more serious, however, is Bergin's assumption that these fifteen ulcer cases are representative of the relevant population. Moreover, the introduction of material on the 'natural history' of patients with peptic ulcer into a discussion on spontaneous remissions in neurotic disorders is not justified.
>
> (Rachman and Wilson 1980: 43)

The remaining studies examined by Rachman and Wilson (1980) are equally irrelevant to the issue in question:

> For reasons that are not explicit, neither Bergin nor Lambert (separately or jointly) appear to be willing to confine their analyses to the

> question in hand, i.e. the rate of spontaneous remission in neuroses. They repeatedly introduce irrelevant information – on the effects of treatment, on recovery rates in surgical patients, on remissions in schizophrenia, on the fate of delinquents, and so on. Lambert (1976: 116) took this inexplicable process one step further and objected to analyses that are confined to untreated neurotic disorders. Contrary to the drift of his argument, the inclusion of studies should not be dictated by caprice, but rather should be an exercise in applying firm standards of selection. It is, after all, simple – if you wish to determine the rate of remission in neurotic disorders, then study data on neurotic disorders.
>
> (Rachman and Wilson 1980: 48)

Above all else, however, the evidence gathered since the original estimate was attempted, emphasizes the need for more refined studies and more accurate statistics. In particular, one can now postulate that the gross spontaneous remission rate is not constant across different types of neurotic disorders. For example, obsessional disorders probably have a lower rate of spontaneous remission than anxiety conditions. Future investigators would be well advised to analyse the spontaneous remission rates of the various neuroses within, rather than across, diagnostic groupings. If we proceed in this manner it will be possible to make more accurate estimates of the likelihood of spontaneous remission occurring in a particular type of disorder and, indeed, for a particular group of patients.

Although the gross spontaneous remission rate has thus far been based on a two-year period of observation (and this serves well for many purposes), attempts to understand the nature of the process will be facilitated by an extension of the periods of observation. The collection of reliable observations on the *course* of spontaneous remissions will, among other things, greatly assist in making prognoses.

Readers will be able to form their own opinion on whether these excursions by Bergin and Lambert into the higher realms of imagination constitute an honest appraisal of the evidence; Rachman and Wilson (1980) leave little doubt on the point. Unfortunately, it has to be said that most writers on the topic prefer the conclusions provided by Luborsky, by Bergin and by Lambert to the much more realistic appraisal offered by Rachman and Wilson; the reasons for this preference are not far to seek in people whose professional advancement and livelihood depend on the popular acceptance of the kinds of psychotherapy they provide. Whether such a procedure is ethically defensible, and scientifically meaningful is of course another question.

The relevance of meta-analysis

Where Luborsky, Bergin and Lambert at least pretend to some kind of scientific objectivity, a much praised book by Smith *et al.* (1980) provides

the seeds of destruction within itself, without requiring any aid from outside critics. The contents of this book, which essentially aims at a meta-analysis of all published studies to date, amount to an essential contradiction of the conclusions I drew in 1952, and which have been virtually unchanged in the review by Rachman and Wilson (1980). To illustrate this conclusion, let me quote first of all the general conclusions drawn by the authors from their data. They assert that

> *Psychotherapy is beneficial, consistently so and in many different ways. Its benefits are on a par with other expensive ambitious interventions, such as schooling and medicine. The benefits of psychotherapy are not permanent, but then little is.*
>
> (Smith *et al.* 1980: 183, original emphasis)

They go on to say that

> The evidence overwhelmingly supports the efficacy of psychotherapy . . . psychotherapy benefits people of all ages as reliably as schooling educated them, medicine cures them, or business turns a profit.
>
> (Smith *et al.* 1980: 183)

Apparently psychotherapy sometimes seeks the same goals as education and medicine, and when it does, psychotherapy performs commendably well:

> We are suggesting no less than that psychotherapists have a legitimate, though not exclusive, claim, substantiated by controlled research, of those roles in society, whether privately or publicly endowed, whose responsibility is to restore to health the sick, the suffering, the alienated, and the disaffected.
>
> (Smith *et al.* 1980: 183)

Smith *et al.* then go on to repeat the Luborsky view that

> Different types of psychotherapy (verbal or behavioural; psychodynamic, client-centred, or systematic desensitization) do not produce different types or degrees of benefit.
>
> (Smith *et al.* 1980: 184)

Allied to this odd conclusion is another one, to wit that

> *differences in how psychotherapy is conducted (whether in groups or individually, by experienced or novice therapists, for long or short periods of time, and the like) make very little difference in how beneficial it is.*
>
> (Smith *et al.* 1980: 188, original emphasis)

As we have already noted, if indeed all different methods of psychotherapy give pretty much the same results, then this disproves conclusively all the theories on which the different methods of therapy are based. Actually of course the data presented in their Table 5.1 completely contradict their own conclusions; they found average effect sizes of 0.28 for undifferentiated counselling, for instance, and of 0.14 for reality therapy, with figures like 1.82 for hypnotherapy and 2.38 for cognitive therapies. This does not suggest equality of outcome! They also fail to note a very important conclusion from the same table that placebo treatment (effect size 0.56) is as effective as Gestalt therapy (0.64), client-centred therapy (0.62) or psychodynamic therapy (0.69). Clearly, their own conclusions force us to argue that all the vaunted effects of psychotherapy are simply placebo effects, a conclusion also arrived at by the much more meaningful analysis carried out by Prioleau *et al.* (1983). It is curious that almost none of the reviewers of the book saw this obvious contradiction between data and conclusions, or commented on the devastating effects this must have on the claims for efficacy of psychotherapy.

Much the same must be said about the final conclusion of the book quoted above, to the effect that for the effectiveness of therapy it makes very little difference if it is done by an experienced or an inexperienced therapist, or for long or short periods of time. If that is true, then clearly claims by psychoanalysts that their discipline requires a lengthy training, and a lengthy time to establish and then resolve transference relations, are completely unjustified. Obviously what we should do is to train therapists for just one hour, and restrict treatment to one hour's duration; clearly, if we can rely on Smith, Glass and Miller, this should make no difference to the outcome! To keep up with our *Alice's Adventures in Wonderland* story, we should apparently have followed the practice of the Red Queen to believe as many as six impossible things before breakfast. To believe the conclusions of Smith *et al.* (1980) would certainly constitute good practice for that.

Smith *et al.* curiously enough do discover that behaviour therapy is more effective than psychotherapy, but they try to argue this conclusion out of existence by a rather specious argument which this is not the place to discuss. Eysenck and Martin (1987) have discussed this question in some detail, and have come to the conclusion that not only is a learning theory of neurosis the only scientific theory available at present, but also the methods of treatment to which it gives rise are the only ones which show a significant improvement over no treatment or placebo treatment. Thus the 'spontaneous remission' objection does not apply to behaviour therapy; Rachman and Hodgson's (1980) book presents an excellent example. The failure of psychotherapy to achieve a similar status, alas, does not seem to have reduced the ardour with which many therapists still proclaim its virtues, and foist it on innocent victims.

Have more recent studies suggested that my estimate was wrong? Garfield and Bergin (1986) have reviewed the field, but I still do not see a single study which would meet what I consider minimum requirements of a meaningful comparison between no treatment, placebo treatment of an acceptable kind, psychoanalysis or closely specified psychotherapy, and behaviour therapy carried out by a properly qualified behaviour therapist, using appropriate methods. If we are dealing with obsessive handwashing, for instance, flooding with response prevention works very well, while desensitization, in our

experience, does not (Rachman and Hodgson 1980); it would be easy to make behaviour therapy do no better than psychotherapy by choosing the wrong method. Also, many people call themselves behaviour therapists without any proper training; one would need to be assured on this point. Ideally studies should be set up by a supervisory group comprised of leading exponents of the methods under comparison, free to select the therapists using their type of treatment. Alas, no such study is familiar to me.

Two objections are often made to the claims of psychotherapeutic lack of effectiveness. The first is that clients often report satisfaction with the outcome of the treatment, even if symptom-removal failed to occur. Cognitive dissonance theory would lead us to expect precisely this; patients who have spent four years or more in treatment, and spent upwards of $100,000 on fees, would not be human if they willingly acknowledged that it was all for nothing. There is also the suggestion constantly reiterated by analysts that if the patients are no better it is all their own fault –'resistance' and all that! Finally, there is the hello-and-goodbye phenomenon; all mental disorders have their ups and downs, and therapy is usually entered in a down phase, and left in an up phase. Anyone making claims of this kind would have to make a proper study of the proportions of satisfied customers, then eliminate possible causes like those mentioned, and finally set the results against the number of dissatisfied customers. Nothing of the kind has yet been done.

The other argument concerns 'market forces' – why, if behaviour therapy is so much more successful, do people not choose it in preference to the discredited psychotherapy? Such an argument is clearly disingenuous. There are still very few properly qualified behaviour therapists, so there can be no proper choice. Few people have heard of the alternatives, so can hardly make a choice. They are not told of the evidence favouring behaviour therapy, so cannot make a *meaningful* choice. 'Market forces' require an open market where buyers and sellers know what the conditions governing the sale are; this is demonstrably not so in the psychiatric field. Patients are sent to the hospital by a general practitioner who has probably never beard of behaviour therapy, and are seen by a psychiatrist who has been brought up in the psychotherapeutic tradition, and regards the psychologists who use behaviour therapy as rivals whose lack of a medical background disqualifies them from treating patients altogether.

But above all there is the Semmelweiss effect. Semmelweiss was a Hungarian physician in Vienna who reduced the mortality of women giving birth in hospital from something like 30 per cent to something like 2 per cent by asking his colleagues to wash their hands when going from one woman to another, thus avoiding the infections which were the killers. The effect was obvious, and so huge, that no argument would seem necessary. However, his colleagues laughed him out of court, refused to follow his advice, and finally forced him to go back to Budapest in disgrace. I recall giving a lecture on behaviour therapy in the university there which bears his name – honoured centuries after the event. New methods make their way slowly in medicine; there is an immense resistance to change, and arguments concerning facts, experiments and clinical trials tend to fall on deaf ears. Psychologists should know better than anyone that human beings are seldom motivated by rational considerations; in the long run psychoanalytic notions will be recognized as the oddities they are, but this time is not yet (Eysenck 1985b). When that happens, behaviour therapy will be the universal method of choice, and historians will wonder about our medieval superstitions.

Negative effects of psychoanalysis

It should not be assumed that the term 'victims' has been chosen inadvisedly in any of the preceding paragraphs. There is ample evidence that psychoanalysis is not an innocent, if ineffective, method of talking to people. As Mays and Franks (1985) have shown, there is frequently a negative outcome in psychotherapy, so that instead of improving the neurotic disorders from which patients suffer, it actually makes them worse. Not only is there good evidence that this is true, but also I have suggested a mechanism, derived from the general theory of neurotic illness in terms of learning theory, which explains why this is so, and why it would have been expected on a theoretical basis (Eysenck 1976a; 1976b; 1977; 1982; 1985a). Those who praise the wonderful effects of psychotherapy customarily disregard or dismiss the negative effects, the evidence for which is much more impressive. This is completely irresponsible of course; the essence of the Hippocratic Oath enjoins us not to harm our patients.

The suggestion that some of the psychological effects of psychoanalysis may be negative, and harm the patients rather than cure them is often rejected as being relatively unimportant, the assumption being that negative consequences cannot be very serious. However, recent work has suggested that not only are they serious psychologically, but also they may involve psychoanalysis as a risk factor in cancer and coronary heart disease (Grossarth-Maticek and Eysenck 1990). These data, as well as those suggesting that behaviour therapy is a very powerful prophylactic aid in avoiding cancer and coronary heart disease, may not be known to all readers, and hence may deserve a special mention, illustrating the wide generality of the positive effects of behaviour therapy, and the negative effects of psychoanalysis.

The work of Eysenck (1987a; 1987b; 1988b; 1989) and of Grossarth-Maticek and Eysenck (1989) and Grossarth-Maticek *et al.* (1988) has demonstrated very clearly two things. The first is that in large-scale prospective studies, in which healthy probands were tested for personality, smoking, drinking, cholesterol levels, blood sugar and blood pressure at the beginning of the study, and were then followed up for ten years or more, specific personality reactions to stress were found to be highly predictive of cancer, while other types of reactions were found to be highly predictive of coronary heart disease. Personality/stress reactions were six times as predictive of these

diseases as were smoking, cholesterol level and the other medical predictors, and deaths from cancer and coronary heart disease were very significantly more frequent in people who were stressed than in people who were not stressed.

It was also demonstrated that a special type of behaviour therapy used in changing the behavioural pattern of cancer-prone and coronary heart disease-prone probands was highly effective in preventing death from cancer or coronary heart disease thirteen years later. When therapy groups were compared with carefully matched control groups, it was found that out of fifty controls, sixteen died of cancer, while in the therapy group none died of cancer. Similarly, out of forty-six controls, sixteen died of coronary heart disease, while in the therapy group only three died of coronary heart disease. These results were obtained with thirty hours of individual therapy; similar results were obtained with group therapy, and with bibliotherapy accompanied by short-term individual therapy (Grossarth-Maticek and Eysenck 1991; Eysenck and Grossarth-Maticek 1991).

In another study an attempt was made to see whether psychoanalysis, which is generally regarded as a very stressful procedure, would add to the stress suffered by cancer-prone and coronary heart disease-prone healthy probands, and would be associated with an increase in mortality from these causes. Studies were made of some 7,000 inhabitants of Heidelberg who were first interviewed in 1973. In 1977 probands were asked whether they had been under any form of psychotherapy, and notes were made at the time concerning duration and type of treatment. In 1986 the participants were followed up, and death and cause of death established by reference to the death certificates of those who had died.

Two groups of physically healthy probands who were under psychoanalytic treatment of an orthodox kind, for mild psychiatric disorders in the main, constituted our therapy groups. One group had been treated for between one and two years, and had then discontinued treatment. Group 2 had been in treatment for two years or more, and had not broken off treatment. Two control groups were created from a large pool of probands so that they could be matched closely with the two treatment groups on age, sex, personality type and cigarette consumption. Matching was person-to-person, thus guaranteeing equality of means and SDs (a measure of variability). A final control group was created to match the two groups together overall.

Table 5.1 shows the final results of our study. The results make certain conclusions very clear (at a high level of statistical significance). Cancer, as expected, is the most frequent cause of death in Type 1 (cancer-prone) persons, coronary heart disease in Type 2 (CHD-prone) persons. Cancer and coronary heart disease are most frequent in the group that had psychoanalysis for over two years, less frequent in those who had psychoanalysis for less than two years, and least in the control groups whose members were not treated by psychoanalysis. There is thus in this table clear evidence that psychoanalysis acts as a stressor, and is a strong risk factor for cancer and coronary heart disease.

Table 5.1 Mortality from cancer, coronary heart disease and other causes for controls and probands treated by psychoanalysis for psychiatric complaints

Therapy	*Status*	*Type 1*	%	*Type 2*	%	*Type 3*	%	*Type 4*	%
(1) Up to two years of psychoanalysis, then terminated	Cancer	11	7.1	4	4.6	5	4.8	1	100.0
	CHD	7	4.5	5	5.8	6	5.7	0	0
	Other	7	4.5	5	5.8	6	5.7	0	0
	Living	129	83.7	72	83.7	87	83.6	0	0
	Omitted	8	4.9	4	4.4	5	4.5	0	0
	Total	162		90		109		1	
			%		%		%		%
(2) Psychoanalysis for longer than two years, not terminated	Cancer	9	9.3	3	6.5	8	7.7	1	33.3
	CHD	8	8.2	6	13.0	8	7.7	1	33.3
	Other	8	8.2	5	10.8	7	6.7	1	33.3
	Living	72	74.2	32	69.5	81	77.8	0	0
	Omitted	5	14.9	0	0	4	3.7	0	0
	Total	102		46		108		3	
			%		%		%		%
(3) Control group for Group 1, matched on age, sex, type and amount of smoking	Cancer	2	1.3	1	1.2	0	0	0	0
	CHD	1	0.6	2	2.4	0	0	0	0
	Other	3	1.9	2	2.4	3	2.7	0	0
	Living	149	96.1	80	94.1	100	95.2	1	100.0
	Omitted	7	4.3	5	5.5	5	4.6	0	0
	Total	162		90		108		1	
			%		%		%		%
(4) Control group for Group 2, matched on age, sex, type and amount of smoking	Cancer	1	1	1	2.2	0	0	0	0
	CHD	1	1	1	2.2	1	0.9	0	0
	Other	1	1	3	6.6	5	4.6	0	0
	Living	94	96.9	40	88.8	98	95.1	3	100.0
	Omitted	5	4.9	1	2.1	5	4.6	0	0
	Total	102		46		109		3	
			%		%		%		%
(5) Control group for Groups 1 and 2 combined, matched on age, sex, and cigarette consumption	Cancer	1	0.6	1	0.5	0	0	1	0.9
	CHD	2	1.2	2	1.0	1	0.9	0	0
	Other	5	2.9	5	2.7	2	1.8	2	1.8
	Living	166	95.4	180	95.7	107	96.4	107	97.3
	Omitted	13	6.9	9	46.6	10	8.3	6	5.2
	Total	187		197		120		116	

These results ought to give pause to those who still advocate the use of psychoanalysis as a treatment for neurotic disorders; not only does the literature suggest that negative consequences of a psychological kind may often follow, but also as we have seen there is now evidence that quite serious health consequences may also follow. Conversely not only is behaviour therapy superior in curing neurotic patients, but also it can be used as a prophylactic aid in preventing cancer and coronary heart disease. These facts ought to, but probably will not, be instrumental in making therapists think twice about using the discredited methods introduced by Freud and his colleagues so many years ago.

Psychoanalysts reply

What is the reaction of advocates of psychoanalytic treatment to these rather grave charges? Their major reaction has been one of disregarding all criticisms, and discrediting the critics by appealing to the concept of 'resistance'; critics cannot and should not be taken seriously because their criticisms are based on neurotic motivations of a very subtle kind which only a psychoanalyst is capable of understanding. It hardly needs a philosopher of science to see through this attempt at side-tracking criticism; it is not the motivation of the critics that is at issue, but the truth or otherwise of the criticisms made. These require to be answered, regardless of the motives of the critics. Furthermore of course the psychoanalyst's answer to the critic presupposes what has to be proved, namely the correctness of psychoanalytic theories, including that of 'resistance'. As we know, there is no such evidence (Grunbaum 1984; Eysenck and Wilson 1973).

Another argument frequently adduced, following Freud, is that neurotic states are so complicated that it is impossible for any matching to be made between therapy and control groups, so that statistical comparisons and clinical trials become meaningless and impossible. This argument leaves out the essential feature of all clinical trials, namely the power of random assignment to eliminate differences between groups. Psychoanalysts have made no attempt to answer this counter-argument and thus their research has resulted in such absurd and meaningless studies as the Menninger one cited previously.

A third argument often propounded is that critics assess the success or failure of treatment merely by looking at the elimination or persistence of symptoms, while psychoanalysis aims at a complete restructuring of personality. As Erwin (1978) and Grunbaum (1984) have pointed out, there are two counter-arguments. In the first place, the elimination of symptoms is *necessary*, although it may not be a *sufficient* criterion for success of treatments. Second, there is no evidence at all for the success of psychoanalysts in 'restructuring' personalities, and no proper criteria are offered for testing this alleged change. Until and unless this is provided, there clearly is no answer to the criticism that psychoanalysts have no evidence to offer supporting their claims.

It is also frequently adduced that these matters are too complex and difficult to allow any natural science approach or answer; this argument is often produced by those who would turn psychoanalysis into a hermeneutic discipline. However difficult it may be to prove the efficacy of psychoanalysis, claims have been made in that direction, and require proof. If this proof is too difficult to obtain, then the claim should be withdrawn until methods have been elaborated to substantiate it. All scientific advances are difficult and require complex reasoning and experimentation; claims for success are not made until such success can be substantiated and replicated.

The final argument originally proposed by Freud, and enthusiastically adopted by his followers, is what Grunbaum (1979; 1980) has called the 'Tally Argument'. This argument is based on the premise that 'clinical data', that is findings coming from *within* the psychoanalytic treatment sessions, substantiate all the claims of psychoanalysis. Grunbaum (1984) suggests that this argument is the basis for five claims made by psychoanalysts, each of which is of the first importance for the legitimation of the central parts of Freud's theory. These five claims are the following:

1 Denial of an irremediable epistemic contamination of clinical data by suggestion.
2 Affirmation of a crucial difference, in regard to the *dynamics* of therapy, between psychoanalytic treatment and all rival therapies that actually operate entirely by suggestion.
3 Assertion that the psychoanalytic method is able to validate its major causal claims – such as its specific sexual aetiologies of the various psychoneuroses – by essentially *retrospective methods* without vitiation by *post hoc ergo propter hoc*, and without the burdens of prospective studies employing the controls of experimental inquiries.
4 Contention that favourable therapeutic outcome can be warrantedly attributed to psychoanalytic intervention *without* statistical comparisons pertaining to the results from untreated control groups.
5 Avowal that, once the patient's motivations are no longer distorted or hidden by repressed conflicts, credence can rightly be given to his or her introspective self-observations, because these data then do supply probatively significant information.

It is Grunbaum's (1984) very detailed and competent destruction of this argument which forms the basis of his book, and his argument has not been refuted by any philosophers or psychoanalysts, as far as I know. I will not try to undertake a detailed discussion here as this would obviously be out of place.

Summary and conclusions

We may now try to see what conclusions can be drawn from this lengthy discussion. It is sometimes suggested by historians of behaviour therapy like Kazdin (1978) and Schorr (1984) that the major contribution of behaviour therapy has been an insistence on the importance of the outcome problem, and an attempt to investigate it in relation to the different methods of behaviour therapy. This is no doubt true in part, but it would seem that many, too many, behaviour therapists have followed the temptations of Lazarus (1967; 1971) to go along the primrose path of 'eclectic' therapy – in other words, to indulge in an arbitrary and subjective mish-mash of theories, practices and therapies without regard to strict outcome measures, empirical guidance or experimental control. This, taken together with the unscientific and arbitrary handling of data by writers such as Luborsky, Bergin, Lambert, Smith, Glass and Miller, has tended to hide the truth behind the veil of inaccurate and tendentious assertions and claims.

Rachman and Wilson have made it only too clear that there is still no good evidence that psychotherapy is any more effective than any reasonable placebo treatment. As the latest publication by the National Institute of Mental Health Treatment of Depression Collaborative Research Program (Elkin *et al.* 1989) makes clear, even after all this time, we are still faced with a virtual equivalence of lauded programmes of psychotherapy and placebo treatments. No doubt this study, like all the others showing negative effects, will be silently consigned to the oubliette where so many other negative reports are secreted, and there will be no slowing down of claims for the wonders that psychotherapy can perform! The people suffering from neurotic symptoms whom we are supposed to help will not thank us for disregarding all the evidence, in continuing to claim successes where no successes exist. Only behaviour therapy can be exempted from this accusation; as even Smith *et al.* (1980) are forced to admit, as a result of their meta-analysis:

> In those studies in which behavioral therapies were compared directly with developmental therapies, the former were vastly superior. In the direct comparison of verbal and behavioral therapies, behavioral therapy has produced reliably larger effects.
>
> (Smith *et al.* 1980: 107)

Perhaps future generations will pay more attention to empirical facts than has been customary over the past half-century; this certainly is a consummation devoutly to be wished.

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RESPONSE – Sol Garfield

There is little question that Hans Eysenck was, and remains, the staunchest and foremost critic of psychotherapy, and of psychoanalysis in particular. Despite the rather significant increase in research on outcome in psychotherapy in the past thirty years, the results secured haven't caused Eysenck to change the view he first espoused in 1952, namely that the effectiveness of psychotherapy has not yet been demonstrated. Thus he exhibits an amazing consistency in his point of view. In what follows, I shall offer my appraisal of Eysenck's consistently critical view of the outcome problem in psychotherapy.

Earlier responses to Eysenck

There is no question that Eysenck's 1952 article in the *Journal of Consulting Psychology*, 'The effects of psychotherapy: an evaluation', created quite a stir among clinical psychologists, a number of whom hastened to publish critiques of the article (DeCharms *et al.* 1954; Luborsky 1954; Rosenzweig 1954). Not only has this article become the most frequently cited article on outcome in psychotherapy, but also Eysenck kept the controversy alive by additional publications in which he continued to maintain, and even to increase, his critical evaluation of psychotherapy. (Eysenck 1961; 1966).

My own response to Eysenck's 1952 article was much more positive than was true of the responses of most of the clinical psychologists I knew. Even though I could see the important limitations in the 'control groups' used by Eysenck in his appraisal and in his subsequent estimate of the spontaneous remission rate of neurotic individuals, I responded positively to his emphasis on the need to evaluate the effects of psychotherapy. To me, this was an important and needed emphasis. However, the data presented by Eysenck did not provide a truly adequate basis upon which to rest his case. Conceivably, he might have pointed to the need for research in a way which did not alienate clinicians and that would not be viewed as 'overkill'.

In my 1957 text on clinical psychology, for example, I had a section entitled 'Evaluation of psychotherapy'. In this section I mentioned the need for research on outcome and the limitations in the existing research at that time. I also referred to Eysenck's paper, gave a brief summary of it, and made the following comment:

> At first glance the material presented by Eysenck appears quite damaging to the entire field of psychotherapy, and, indeed, it is so presented. However, before accepting such a drastic conclusion, it is worth discussing at greater length the problems encountered in the evaluation of therapy.
>
> (Garfield 1957: 334)

In my view, and in the view of many others, Eysenck did not fully evaluate the limitations in the research reviewed and in the assumptions made for the two so-called control groups. Once he reached his conclusion about the effects of psychotherapy, it seemed as if he were reluctant to consider other alternatives. Certainly, serious questions could be raised about the comparability of the patients treated in the various studies, the suitability of the control groups, and the criteria of improvement used. The classification of patients into the broad classification of 'Neurosis' is an overly broad and unreliable one on which to base serious conclusions.

Despite the various critiques offered of Eysenck's 1952 article, he continued to hold to his original conclusions in his later presentations (Eysenck 1961; 1966) and in the present chapter. As I have already indicated, I believe that Eysenck was fully justified in asserting in his earlier work that the efficacy of psychotherapy had not been demonstrated. Clearly, the existing research was of poor quality and the rates of improvement reported ranged rather widely, from 39 per cent to 67 per cent for psychoanalysis and from 41 per cent to 77 per cent for so-called eclectic therapy. However, both the quantity and quality of research on psychotherapy has increased since the mid-1950s, and there is a much larger body of data available at present to be evaluated and from which at least tentative conclusions can be drawn that do not necessarily agree with those of Eysenck. However, before discussing this material a few other comments can be offered.

Eysenck's critical pronouncements on psychotherapy did appear to stimulate a greater awareness on the part of others to conduct research and to present evidence in response to his critiques. Meltzoff and Kornreich (1970) and Bergin (1971) published reviews that included studies not mentioned by Eysenck and that presented a more favourable view of outcome in psychotherapy. Bergin, in particular, responded to Eysenck's interpretations and included a re-analysis of some of the data on psychoanalysis that had been modified and reinterpreted by Eysenck. This led him to offer very different interpretations. In turn, Bergin was rather strongly criticized by Eysenck's colleague, Rachman (1973), and by Eysenck in the present chapter. There is little question that such heated controversies tend to create polarization and at times to diminish objectivity. As I have pointed out elsewhere, 'Whereas Eysenck (1952) came up with a 39 per cent improvement rate for the Berlin Institute, Bergin (1971) came up with a 91 per cent rate of improvement! Clearly, no scientific conclusions are possible from such data' (Garfield 1974: 388).

Since the early 1970s, additional comprehensive and critical reviews of research on outcome have appeared which are based on a larger number of studies than those reviewed earlier by Eysenck and that are not alluded to in his current chapter. Rather than list a series of such references, I shall simply refer to the review by Lambert *et al.* (1986) which not only evaluates the recent literature but also includes a table that summarizes all or practically all of the meta-analytic reviews that have dealt with outcome in psychotherapy. Although one can criticize some of the studies included in the individual meta-analyses, the overall pattern is relatively clear, and clearly positive. There is a median effect size of 0.82 which does indicate a positive effect for the psychotherapies evaluated. Interpretations of this effect size may vary (Rosenthal 1983; Smith *et al.* 1980), but the direction is clear.

Since Eysenck has questioned this type of finding and its interpretation, some elaboration is required. According to Smith, Glass and Miller an effect size of 0.85 signifies that the average treated patient at the end of treatment is better off than 80 per cent of untreated controls. This is considered to be a large effect according to Cohen (1977) and such results 'suggest that the assignment to treatment versus control conditions accounts for some 10 per cent of the variation in outcome assessed in a typical study' (Lambert *et al.* 1986: 159). My own interpretation based on a variety of clinical reports and studies is that about 65 per cent of patients who receive psychotherapy show some improvement and that perhaps 10–20 per cent of this group show marked improvement. Since Eysenck himself has reported an improvement rate of about 65 per cent for psychotherapy, we agree on this estimate. However, the interpretation of this estimate is another matter, and, therefore, I want to discuss several areas where we definitely disagree.

The placebo response

One basic disagreement concerns the interpretation of the placebo response and the role of the placebo as a control in psychotherapy research. Eysenck believes it is a desirable control whereas I and some others see placebos as by no means 'inert' or comparable to a no-treatment group. Although a placebo may be appropriate for studies of pharmacological agents, it is not a meaningful control for psychotherapy. People do respond to a placebo for a variety of reasons and thus it would appear to have 'psychological' properties. It mirrors certain general features that probably constitute some of the components of the psychotherapeutic process such as the generation of hope, the support received from the placebo therapist, the feeling that one is doing something about one's problem, and the like. Thus, in my view it is not an appropriate control for evaluating psychotherapy outcome *per se* and some individuals have referred to it as the 'powerful placebo'. When some forms of psychotherapy perform little better than some placebos, it may be that the former rely on the same general factors as the placebo without offering much in addition. It is well to remember that the placebos used in the different studies have varied widely (Garfield 1983a). In any event, although some studies may not show statistically significant differences between a form of psychotherapy and a placebo, in most of them psychotherapy secures a visibly larger number who show positive gains.

The recently completed collaborative study of the treatment of depression co-ordinated by our National Institute of Mental Health is also worth referring to here (Elkin *et al.* 1985; 1989). In this study, 239 patients in three medical centres were assigned randomly to one of four treatment groups: Cognitive-Behaviour Therapy (CBT), Interpersonal Psychotherapy (IPT), Imipramine plus Clinical Management (IMI-CM) and a Pill Placebo plus Clinical Management (PLA-CM). Although the placebo group was selected to be a control for the Imipramine group, comparisons were made with all groups. The patients studied all met specific criteria of unipolar depression, therapy manuals were used for training the therapists to ensure the integrity of the therapies, the therapy sessions were monitored and were conducted in centres not associated with the developers of the therapies studied, and a variety of standard measures were used.

The results obtained at the end of treatment are quite similar to those obtained in many studies (Elkin *et al.* 1989) – no significant differences were obtained between the two forms of psychotherapy. Actually, there were few significant differences among the four treatment groups although the patients in all groups showed significant improvement over the course of treatment. In terms of the patients considered 'recovered' at the end of treatment on the basis of securing a score of six or less on the Hamilton Rating Scale of Depression, the percentage reaching this level ranged from 51 per cent to 57 per cent for the three treatment conditions and was 29 per cent for the PLA-CM condition. These findings were for those patients who completed treatment. Although there was a trend toward a statistically significant difference, the group findings were not statistically significant. Nevertheless, the pattern obtained is quite comparable to the pattern secured by Smith *et al.* (1980) in their meta-analysis of 475 studies on outcome and deserves some additional comment.

In the Smith *et al.* review, the overall effect size (ES) for psychotherapy was 0.85 whereas the ES for placebos was 0.56. Thus, the ES for the placebo treatments was somewhat more than half the ES secured for psychotherapy. In the NIMH study, the ratio of the 'recovery' rate for the pill placebo plus clinical management to the rate for the two psychotherapies is approximately the same. In other words, as indicated previously, the so-called placebo used in many studies is not an inert stimulus, but conceivably contains a number of the factors that are common to most of the psychotherapies. In the NIMH study, for example, each of the patients in the pill-placebo plus clinical management condition received an intensive diagnostic appraisal and were seen by a psychiatrist for twenty to thirty minutes throughout the sixteen weeks designated for the study. Without question, this experience was beneficial for a certain percentage of patients, but the number was clearly less than the number helped by the two psychotherapies or Imipramine.

Psychotherapy and behaviour therapy

It is quite evident that Eysenck sharply differentiates behaviour therapy from psychotherapy, or, preferably, from all other forms of psychotherapy. He believes firmly that behaviour therapy is effective, and that psychotherapy is not. I do not know if this is a widely held view in Britain or not, but I believe most of us in the United States tend to view behaviour therapy as one form of psychotherapy, and there is currently an organized movement to attempt some integration of various forms of psychotherapy (Goldfried and Newman 1986). Be that as it may, there is considerable interaction among behaviourally oriented clinical psychologists and clinical psychologists of other theoretical persuasions.

Of more importance, however, is the fact that there have been studies that compared behaviour therapy with other forms of psychotherapy. Eysenck does not mention the very well-known study of Sloane *et al.* (1975) that compared behaviour therapy and brief psychoanalytically oriented psychotherapy. This study was distinguished by the fact that the therapists used were for the most part very experienced and well-known therapists. Joseph Wolpe and Arnold Lazarus, for example, were two of the three behaviour therapists employed in the study and the analytically oriented therapists were of comparable experience and distinction. Overall, in terms of most of the group comparisons, there were no important differences among the two forms of psychotherapy. In terms of the primary criteria of change, the target symptoms of each patient, both therapies secured significantly better results than a wait-list control group, and were not significantly different from each other. Furthermore, of particular interest was the finding that at the end of therapy, 'The successful patients in both therapies placed primary importance on more or less the same items' (Sloane *et al.* 1975: 206).

As I have noted elsewhere, 'The Society for Psychotherapy Research in 1980 awarded Sloane and his colleagues its first award for an outstanding research study in the area of psychotherapy' (Garfield 1981: 39). Although the study was criticized by several behavioural psychologists (Bandura 1978; Kazdin and Wilson 1978; Rachman and Wilson 1980), similar findings have been reported by others (Berman *et al.* 1985; Thompson *et al.* 1987; Zeiss *et al.* 1979). Space limitations preclude additional elaboration.

Concluding comments

As I indicated previously, despite inadequacies in the evaluation made by Eysenck in his 1952 article, I responded very positively to his emphasis on the need to evaluate the effects of psychotherapy. 'There is little question that this was an important event historically and helped focus attention on the need to evaluate the effectiveness of psychotherapy' (Garfield 1983b: 35). I also felt his criticisms of psychoanalysis had justification and I, too, offered a strong criticism of the eighteen-year Menninger study (Garfield 1981). However, my current view of the research on outcome in psychotherapy diverges from that of Eysenck. His view has remained unchanged since 1952 and apparently the accumulated research over forty years has had little impact on him. In my 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. Because of this, he has not been able to accept the fact that even if neurotic patients recover in two years without treatment, they may be helped more rapidly by means of psychotherapy. In a similar fashion, although behaviour therapy may be the most effective for such disorders as phobias, compulsions and infantile autism, there is considerable comparability in outcomes for behaviour therapy and other forms of therapy for a variety of other problems and this does suggest the possibility of important common factors among the psychotherapies (Garfield 1980; Lambert *et al.* 1986). We shall not settle our differences here and both of us most likely won't be around when these issues may be settled more conclusively on the basis of better research in the future.

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REBUTTAL – Hans Eysenck

I am not at all convinced that Garfield and I differ all that much with respect to *answers*; we may well differ with respect to *questions*.

This may mirror our respective major commitments to therapy and science. Garfield sees large numbers of people suffering from neurotic disorders; his major interest is in what may help them. I see a scientific problem of discovering *why* neurotics fall prey to neurotic disorders, why they seem to recover without treatment, or with placebo treatment, whether any of the treatments based on some form of theory does better than placebo treatment, or any alternative form of treatment, and to what extent results bear out theoretical preconceptions. My emphasis thus has been to build a model which would incorporate the major empirical findings. On these findings Garfield and I seem to agree for the most part, but there is a curious lack of response by Garfield to my interpretation of these findings.

I have tried to show that if 'all have won, and all must have prizes' is really true; that is if different forms of treatment based on different theories have the same effect, then all these theories must be wrong. Each theory predicts that the treatment based on it will be significantly more successful than treatments based on other (false) theories; if that prediction fails, then the theory fails. The result would seem to be that *all* theories concerning psychotherapy are wrong (I am here sharply divorcing psychotherapy from behaviour therapy). Garfield does not tell us whether he agrees with this conclusion, but it seems to me incontrovertible, and highly damaging to the whole enterprise seen as a scientific endeavour to build a proper *model* of therapeutic effectiveness.

The placebo response, I believe, is particularly important in this discussion. I fully agree with Garfield that placebos are by no means inert, but have 'psychological' properties. I would go even further than that and say that what a 'non-treatment control' does to alleviate a person's suffering (consult a priest, discuss his or her problems with a friend, talk over worries and anxieties with a family member) has the same psychological properties, and explains the 'spontaneous remission' which has been found so successful.

If there is a grading from 'spontaneous remission' through placebo treatment to psychotherapy and finally to behaviour therapy, I would explain this in terms of a model which makes desensitization and other methods of behaviour therapy the fundamental ingredients in successful treatment (Eysenck 1980). The differential effectiveness of their methods would be explained in terms of the deliberate use of these methods, least for spontaneous remission, most for behaviour therapy. It should be remembered that nearly all these encounters contain some personal interaction between patient and therapist – friend–priest–relative, in which friendly acceptance facilitates desensitization. The only exception would be psychoanalysis along classical lines in which such sympathetic aid and friendly interaction is neglected in favour of dogmatic neutrality and 'interpretation' (Sutherland 1976). It is this that probably accounts for the 'negative outcome in psychotherapy' (Mays and Franks 1985) that Garfield fails to mention.

Garfield is right in thinking that I make a firm distinction between behaviour therapy and psychotherapy, and he is also right in saying that in the United States there is some attempt to produce some integration between them. Seeing how different the theories are on which behaviour therapy and psychotherapy are based, it will be interesting to see how such reconciliation is produced. My own interpretation would be that what is sought is simply an eclectic mish-mash of theories (Eysenck 1970) signifying nothing, and completely untestable scientifically. I am certainly unaware of any demonstration that a treatment based on such a confabulation has been shown significantly superior to behaviour therapy. To some this may read like dogma; I have elsewhere argued that behaviour therapy is applied science, not dogma, and that science is not well served by eclectic committee decisions, but only by firm theorizing and experimental testing of deductions (Eysenck 1976).

It is sometimes suggested that behaviour therapy, based on deductions from learning theory, has been displaced by cognitive behaviour therapy, which admits cognitive elements. This, it is suggested, brings behaviour therapy and psychotherapy closer together.

This argument is completely fallacious (Eysenck and Martin 1987). Learning theory from Pavlov onwards has always included cognitive elements, and these are absolutely fundamental (Mackintosh 1984). Criticisms based on the aberrations of Watson or Skinner are irrelevant; in my definition of behaviour therapy as being based on modern learning theory, I laid special

emphasis on the relevant theories being *modern*, not anchored in the early 1920s. The term 'cognitive behaviour therapy' is either an oxymoron or a tautology, and it should be eliminated from meaningful scientific discourse.

One final comment. Garfield cites some meta-analyses which at times seem to contradict my conclusions based on other meta-analyses. But disagreements between meta-analysts are no less frequent than those between reviewers prior to the advent of this particular type of analysis. Wittman and Matt (1986), for instance, in a meta-analysis of the large body of German studies, found, contrary to Smith *et al.* (1980) that quality of study did make a large difference, and that the effects of different types of psychotherapy did differ with therapies of behavioural orientation showing the highest effects. (Riedel and Schneider-Duker (1991) criticize and extend this discussion.) What all this suggests to me is that in the presence (still!) of gross disagreement between experts (Rachman vs Bergin; Wittman and Matt vs Smith, Glass and Miller; Wolpe vs Lambert), it would be premature to dismiss my original conclusion that the superiority of psychoanalytic treatment or psychotherapy over (credible) placebo treatment had not been proved beyond reasonable doubt. (The average effect size of placebo treatment is badly affected by placebos which lack credibility.) For behaviour therapy I would claim a more positive conclusion; such advances as those demonstrated by Rachman and Hodgson (1980) over all previous therapeutic interventions in the treatment of obsessive-compulsive disorders show what can be done when we take theories seriously, and base our methods of treatment on modern learning theory. But above all I agree with Garfield on the need for better research in the future; this alone will settle any differences.

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SIX

The myth of therapist expertise

KATHARINE MAIR

Psychotherapists of the 1990s are in many ways in a similar position to the physicians of eighty years ago. Their patients have faith in their expertise and expect them to say what is wrong and how to put it right. Psychotherapists' understanding of patients' problems, and knowledge about how they can be remedied is, however, very much less than they imagine. It is also less than therapists imagine. They have been through a training which claims to give them a model by which to understand their patients, and methods by which to treat them. Physician and psychotherapist alike believe in their models and methods because they see them work. I hope to demonstrate that, although psychotherapy can be a valuable means of helping people, its efficacy is not due primarily to the models and methods that it uses (which may be as irrelevant to the patient's problems as the application of leeches was to the curing of a fever eighty years ago), and that too blind a faith in them may actually interfere with therapists' ability to help their patients. George Bernard Shaw voiced his scepticism of the doctors of his day in his preface to *The Doctor's Dilemma* in 1911. His comments seem appropriate to this argument.

The expert healer

> *I presume nobody will question the existence of a widely spread popular delusion that every doctor is a man of science.*
>
> (Shaw 1911)