

The Outcome Problem in Psychotherapy: A Reply

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In his recent article in this journal, Strupp (1963) has this to say in relation to the outcome problem in psychotherapy: “A brief review of Eysenck’s (1952) widely quoted survey, which capitalized upon and added considerably to the existing confusion may be instructive.” In reply I would like to suggest that Strupp’s review is, in the lawyer’s phrase, irrelevant, incompetent and immaterial. Fortunately, the points I wish to make are so simple that they will not take up much space.

1. *Incompetent.* In my review I came to one main conclusion, which was stated in the Summary as follows: “The figures fail to support the hypothesis that psychotherapy facilitates recovery from neurotic disorder. In view of the many difficulties attending such actuarial comparisons, no further conclusion could be derived from the data whose shortcomings highlight the necessity of properly planned and executed experimental studies into this important field.” In other words, it was asserted that no data existed disproving the null hypothesis scientifically. There is only one way to answer such an argument and that is to point to an experimental study or investigation conclusively disproving the null hypothesis. Strupp clouds the issue by lengthy argument but fails to adduce a single study disproving my original conclusion, much strengthened since then by numerous more recent and better executed studies summarised in my 1960 article in the *Handbook of Abnormal Psychology* (Eysenck, 1960b).

When this point was first made in 1952, it was considered controversial and many articles have been written along lines similar to those adopted by Strupp. Since then even leading psychoanalysts and psychotherapists have agreed with my conclusion. Consider just a few examples which could be multiplied at length. The Chairman of the Fact Finding Committee of the American Psychoanalytic Association, Dr. Harry L. Weinstein, had this to say in a lecture given a few years ago at the Maudsley Hospital: “No claims regarding the therapeutic usefulness of analytic treatment are made by the American Psychoanalytic Association. We are not responsible for claims made by individuals in whom enthusiasm may outrun knowledge.” E. Glover, the leading British psychoanalyst, has this to say in his recent book *The Technique of Psychoanalysis*: “We have next to no information about the conduct of private analytic practice . . . such figures as are published regarding clinic practice would in the majority of cases be rejected as valueless by any reputable statistician, uncorrected as they are for methods of diagnosis and selection, for length of treatment, for method of treatment, for after-history and for spontaneous cure.

Indeed apart from an occasional reference to a case that may have remained well for some years, we have no after-histories worth talking about . . . this absence of verifiable information, when added to the loose assumptions I have already set out, fosters the development of a psychoanalytic *mystique* which not only baffles investigation but blankets all healthy discussion . . . we cannot attach *any* scientific significance to general impressions or assumptions regarding *any* form of psychotherapy.” (pp. 376–377).

Even more important from some points of view is a recent book entitled *The Crisis in Psychiatry and Religion*, by O. H. Mowrer, a former President of the American Psychological Association, a brilliant experimentalist, and for over thirty years a leading psychoanalyst and psychotherapist. Here are some of the things he said: “From testimony now available from both the friends and foes of analysis it is clear that, at best, analysis casts a spell but does not cure (p. 121) . . . as a result of a succession of personal and professional experiences, I have become increasingly convinced, during the last ten or fifteen years, of the basic unsoundness of Freud’s major premises (p. 123) . . . psychiatrists and psychologists are rather generally abandoning psychoanalytic theory and practice (p. 134) . . . there is not a shred of evidence that psychoanalysed individuals permanently benefit from the experience, but there are equally clear indications that psychoanalysis, as a common philosophy of life, is not only nontherapeutic but actively pernicious (p. 161).”

Last we may quote Dr. D. H. Malan, Senior Hospital Medical Officer at the Tavistock Clinic of London, which is well known as the most influential psychoanalytic institution in England. He writes in his book *A Study of Brief Psychotherapy*: “There is not the slightest indication from the published figures that psychotherapy has any value at all.” If any such evidence existed we may assume that Strupp, Malan, Glover, Mowrer, Weinstein, or the many critics of my original paper would have unearthed it. Seeing that they did not, I think the conclusion must stand.

2. *Irrelevant.* Strupp spends much of his time criticising the data on which my conclusion was based. He adds that “Several writers have taken Eysenck to task for his conclusion, pointing out numerous failures in his design.” He forgets to add that I drew attention to these shortcomings myself and criticised the absence of any proper design in the published studies. If I may quote from my original article: “The figures quoted do not necessarily disprove the possibility of therapeutic usefulness. There are obvious shortcomings in any actuarial comparison and these shortcomings are particularly serious when there is so little agreement among psychiatrists relating even to the most fundamental concepts and definitions. Definite proof would require a special investigation, carefully planned and methodologically more adequate than these *ad hoc* comparisons.”

Clearly we may take different attitudes to the published data which are summarized. We may say that reports of the therapeutic

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outcome are, for all practical purposes, worthless; this presumably is the position taken by Glover, and I have much sympathy with it. If we accept this argument, then there is nothing more to be said; my conclusion that there is no evidence establishing the value of psychotherapy stands *a fortiori*, as all the existing evidence is declared inadmissible. If Strupp is inclined to take this line, he obviously has no leg to stand on.

My own attitude was rather to argue that we must acknowledge the great weaknesses in all existing studies, but we might still be justified in looking at the figures as they stand in order to see whether they provide any suggestive evidence for the usefulness of psychotherapy. This they do not do, and each individual will have to decide to what extent he would be willing to accept these data as justifying more far-reaching conclusions than the ones I drew myself. However much we may look at the data, we cannot escape from the conclusion that Strupp's arguments are irrelevant to the main conclusion.

I would particularly disagree with Strupp's argument that "improvement" in the non-treatment group was not comparable with "improvement" in the treatment group. The possibility cannot, of course, be ruled out that such differences did, in fact, exist, but they might have cut either way. It is true that criteria for the improvement of the non-treatment groups are not as well defined as we might have wished, but it is even more obviously true that the criteria for improvement in the treatment groups are even less satisfactory. Denker, on whose work I have relied most for my assessment of improvement under non-treatment conditions, does at least list a set of reasonable criteria which have a certain degree of reliability and objectivity; the psychoanalytic writers whose outcome results I have quoted do not vouchsafe anything like as much information about their criteria as does Denker. That this is true will be obvious to anyone who reads the relevant documents and the point is conceded by Glover, for instance, in the passage from his book *The Technique of Psychoanalysis* which I have already quoted. It is difficult, therefore, to accept Strupp's statement that "the statistics reported by the four psychoanalytic treatment centers may be accepted as reasonable assessments." I see no rhyme or reason in accepting data so strongly criticised by fellow psychoanalysts whilst rejecting data which in many ways are clearly superior in terms of standards stated and follow-ups carried out. In addition one may ask why Strupp has neglected to mention the large body of data now available to which his criticisms do not apply because similar criteria were used for treatment and non-treatment groups; some of these were reported in my 1960b paper, and one of the most striking more recent ones has been published in J. T. Barendregt's book *Research in Psychodiagnostics*. All these studies reinforce my conclusion that there is no difference in outcome between treated and non-treated groups.

Indeed, there is one point which suggests that these more recent studies may even lead to the conclusion that therapy actually has a deleterious effect on mental patients. Strupp argues "that therapists have fairly specific—and presumably valid—notions about the kinds of attributes which a 'good' patient should possess"—meaning by this, patients considered good prognostic risks. Now in the Barendregt study patients applying to the Amsterdam Institute of Psychoanalysis for treatment were divided into three groups and followed up; the three groups consisted of those who were given psychoanalytic treatment, those who were not accepted and received other treatment, and those who were not accepted and

received no treatment. Presumably only "good" patients were accepted for psychoanalysis and the out-look for them, irrespective of any other factor, should have been better than for those rejected. Nevertheless, they did no better than those not receiving treatment, and if the selection actually did succeed in picking the "best" patients then one might argue that the treatment actually had deleterious effects. While this is certainly possible, my own feeling would be that Strupp is rather optimistic in his belief that the "fairly specific notions" held by therapists about the nature of good patients are indeed "presumably valid"; the evidence does not seem to me as convincing as it does to Strupp.

3. *Immaterial*. It is well known that theories and methods of therapy are not usually overcome by criticisms, however bad the theories, however useless the treatments, and however reasonable the criticisms. Theories and treatments only yield to better theories and better treatments, and I realised fully that my 1952 review would not by itself have much effect on the theory or practice of psychotherapy without the provision of something else to take its place. Fortunately, we now have an alternative method of treatment rationally based on scientific concepts developed in psychological laboratories, and deriving its methods from modern learning theory. Behaviour therapy (Eysenck 1960a, 1964; Wolpe, 1958) has already been shown to be a much shorter, and for many neurotic disorders a much more effective, method of treatment than psychotherapy, whether eclectic or psychoanalytic. We are not, therefore, faced with the alternative, implicit and occasionally explicit, in Strupp's article, "psychotherapy or nothing"; we are in the position of having two contenders in the ring between whom a rational choice should not be impossible. It is to be hoped that in the near future American psychologists and psychiatrists will follow the example of their British and Commonwealth brethren and set up clinical trials to evaluate the adequacy of these two methods of therapy against each other. The recently founded journal *Behaviour Research and Therapy* (Pergamon Press) was brought to life particularly in order to publish such comparative clinical studies and to make available details of treatment of specific disorders along the lines of behaviour therapy; it is to be hoped that one effect of the criticisms made by Strupp and others, and the arguments regarding the outcome of different methods of treatment, will be a more sophisticated approach to, and a better design of, experiments in this field.

In conclusion, I would like to draw attention to one curious feature of Strupp's method of argumentation. Talking about my view that neurosis "seems to run an almost self-limiting course . . . from which the patient somehow recovers through therapy or spontaneously," Strupp comments that "anyone having the slightest familiarity with psychopathology and psychodynamics knows how erroneous and misleading such a conception is." The conception may indeed be erroneous and misleading, but it can hardly be dismissed in this oddly cavalier fashion. Strupp nowhere argues the case properly or deals with the evidence I have carefully listed in my 1960b article. What "anyone . . . knows" is surely irrelevant in the consideration of factual evidence; at one time everyone knew that the earth was flat, or that the sun rotated around it. Such emotionally evaluative statements have no place in scientific discourse, and while I have only drawn attention to this particular one, the careful reader will discover again and again that instead of answering the factual points made in my papers on an equally

factual basis, Strupp simply appeals to the emotional bias of his readers. Little would be gained in following him into this field.

A last point. Strupp throughout appears to argue from the implied premise that the burden of proof lies on those who criticise psychoanalysis and question the efficacy of psychotherapy. This view is clearly mistaken. Psychoanalysts and psychotherapists generally assert that their methods cure psychoneurotic disorders, and are in fact the only methods which can achieve this end. Clearly, therefore, it is on them that the onus of proof must rest. They must define clearly and unambiguously what is meant by neurotic disorder and what is meant by cure; they must put forward methods of testing the effects of the treatment which are not dependent on the subjective evaluation of the therapist, and they must demonstrate that their methods give results which are clearly superior to any alternative methods, such as those of behaviour therapy, or of spontaneous remission. It is indisputable, I suggest, that psychotherapists and psychoanalysts have failed to do any of these things, and until they have all been done I find it very difficult to see how any doubt can be thrown on my conclusion that

published research has failed to support the claims made. Strupp comments, as I too have done, on the poor quality of the research that has been published; he fails to see, however, that the criticisms of these researches only make my conclusion more inevitable. If we were to say that all the research done is completely worthless, then my conclusion would be most triumphantly vindicated!

References

- Eysenck, H. J. (1952). The effects of psychotherapy: An evaluation. *Journal of Consulting Psychology, 16*, 319–327.
- Eysenck, H. J. (Ed.) (1960a). *Behavior therapy and the neuroses*. Oxford: Pergamon.
- Eysenck, H. J. (Ed.) (1960b). *Handbook of abnormal psychology*. London: Pitman.
- Eysenck, H. J. (1964). *Experiments in behavior therapy*. Oxford: Pergamon.
- Strupp, H. H. (1963). The outcome problem in psychotherapy revisited. *Psychotherapy, 1*, 1–13.
- Wolpe, J. (1958). *Psychotherapy by reciprocal inhibition*. Stanford: University Press.

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