

except at the chance level. Friends of the Ss likewise failed to pick the test-based interpretations better than by chance. Ranking of the interpretations by another group according to adequacy of self-description showed a general tendency toward acceptance or rejection which agreed with the original group. Analysis of the data and the personality descriptions suggested that acceptable college personality interpretations are short and include vague, double-headed, modal, and favorable statements.

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## THE EFFECTS OF PSYCHOTHERAPY: A REPLY

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THE TITLE and tone of Dr. Rosenzweig's recent paper in this journal (3) suggest that he does not agree with the main conclusions of my paper entitled "The Effects of Psychotherapy: An Evaluation" (1). It is difficult to argue with him as on all crucial points we seem to be in complete agreement. His criticisms are not directed against what I wrote but against a quite erroneous impression of what my main points were; consequently, a brief restatement of these points should suffice to settle the dispute.

In the first place, Rosenzweig apparently believes that I maintain that psychotherapy does more harm than good. He quotes the actual figures for recovery which I give, showing greatest improvement to follow the least amount of therapy, and least improvement to follow the psychoanalytic type of therapy, and adds "We are therefore to infer that psychotherapy is less effective than no psychotherapy". He does not, unfortunately, quote the sentence which appears at the end of this paragraph dealing with the inverse correlation between recovery and psychotherapy, in which I say: "This conclusion requires certain qualifications." He gives the impression that I am putting forward a dogmatic statement regarding the ineffectiveness of therapy. Nothing could be further from the truth, and in evidence I may perhaps be allowed to quote the qualifications I myself was careful to point out:

The figures quoted do not necessarily disprove the possibility of therapeutic effectiveness. 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. But even the much

more modest conclusion that the figures fail to show any favourable effects of psychotherapy should give pause to those who would wish to give an important part in the training of clinical psychologists to a skill the existence and effectiveness of which is still unsupported by any scientifically acceptable evidence.

Rosenzweig takes me to task for not enumerating the shortcomings mentioned, and contrasts this failure with the procedure of Landis (2), who has discussed them in detail. This does not seem to be a reasonable criticism. Landis was writing a chapter in a book and had ample space for discussion; I was writing a short paper from which the editor would almost certainly have excised any unduly lengthy repetitions of what had already appeared in the literature. After all, it is customary in scientific journals to present new evidence and arguments, not to recapitulate in tiresome detail what had already appeared before and should be presumed to be known to one's colleagues.

I am fully in agreement, therefore, with most of what Rosenzweig says about the difficulties of defining neurosis, of defining psychotherapy, and of defining recovery; these difficulties, as I pointed out in my article, arise from the fact that "there is so little agreement amongst psychiatrists relating even to the most fundamental concepts and definitions." Rosenzweig dots the *i*'s and crosses the *t*'s at some length, but does not, in effect, contradict what I have to say.

All through his article, Rosenzweig seems to criticize me for having attempted to prove that psychotherapy is ineffective. This, however, I never attempted to do. I was not trying to *prove* the null hypothesis, which would be foolish as well as inadmissible; I was simply examining available data to see whether these data *succeeded in disproving* the null hypothesis. For the various reasons which Rosenzweig details, and which I agree with, the

available data are not of a kind to inspire much confidence. Nevertheless, *they are the only ones available for the purpose* and, so far as they go, they must be said to fail to disprove the null hypothesis. Hence, I cannot see how it is possible to take issue with my conclusion: "The figures fail to support the hypothesis that psychotherapy facilitates recovery from neurotic disorder." This statement I take to be, in essence, identical with Rosenzweig's statement that "broad generalizations as to the effectiveness of treatment are to be avoided."

What I cannot understand, however, is Rosenzweig's insistence that I "generalize freely," "lightly dismiss" sources of difficulty, and indulge in "hasty generalization." Perhaps he has failed to read the last sentence of my summary in which, having pointed out that available figures do not disprove the null hypothesis, I go on to say: "In view of the many difficulties attending such actuarial comparisons, no further conclusions could be derived from the data whose shortcomings highlight the necessity of properly planned and executed experimental studies into this important field." I thus only make two claims: (a) that available data have serious shortcomings, and (b) that as far as they go, these data do not support the theory of psychotherapeutic effectiveness. As Rosenzweig himself explicitly agrees with both these statements, it is difficult to see precisely what it is that he is criticizing.

The only possibility left is that he disagrees with some of the subjective judgments which, as I explicitly pointed out in my paper, I had to make in order to summarize the data. It would be fruitless

to discuss points of disagreement in detail; I can only suggest to anyone interested in the issue that he should go through all the papers quoted, making his own subjective judgments as to what is or is not to be included in the terms "neurosis," "psychotherapy," and "improvement." I think it will be found that no possible combination of criteria will yield results showing psychotherapy to be therapeutically effective. When it is realized that these data, poor as they are, are all the evidence available regarding a method of therapy which has been practiced for more than 50 years on hundreds of thousands of patients, then it will, I think, be agreed that the failure of the data to show any degree of therapeutic effectiveness should act as a spur to ensure the initiation of large-scale, properly planned, rigidly controlled, and thoroughly analyzed experimental studies in this important field. I feel that on this point, just as much as on the others, Rosenzweig and I are in complete agreement.

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#### CALUMET

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IF I HAD "a quite erroneous impression" of Eysenck's main conclusions, it is my further impression that I was not alone in this reaction to his paper. The implicit tone as well as the explicit statement work together in the process of communication. But it is good to see that Eysenck has

troubled to correct the false impression his paper created. It is particularly gratifying that he reiterates the subjective basis of the judgments he found it necessary to make in summarizing the literature. Since he insists that we are in agreement—calumet!

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