

A REPLY TO LUBORSKY'S NOTE

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Luborsky appears to have two objections to my paper on 'The effects of psychotherapy'. The first one is that the conclusions do not follow from the data; the second one is that 'the main implicit argument', based on these conclusions, is a *non sequitur*. He seems to feel strongly on these points as he appears to accuse me of 'clouding the facts and coming to wrong conclusions'.

I find it difficult to answer his paper as I agree with most of what he says throughout. He is concerned with pointing out the shortcomings of the researches I have summarized, both from the point of view of selection of control and experimental groups, and from the point of view of the clinical judgements made of the improvement or otherwise of the patients. With these criticisms I agree; I have myself commented, in the article criticized, on the poor quality of the data 'whose shortcomings highlight the necessity of properly planned and executed experimental studies into this important field'. I am grateful to Luborsky for dotting the i's and crossing the t's, but I fail to see how this amounts in any sense to a criticism since it supports what I myself have written.

Basically, I think, Luborsky has misunderstood the logic of my approach. He seems to imagine that I tried to prove from the existing data that psychotherapy has no effect; this would be foolish (because you cannot prove a negative) as well as inadmissible (because of the poor quality of the evidence). What I tried to do, however, was something quite different: I tried to show that the available evidence, in spite of, or because of, its poor quality, fails to support the hypothesis that psychotherapy has any beneficial effects. I did not try to prove the null hypothesis, but merely examined the efforts of other investigators to disprove it. My conclusion was, and Luborsky does not attempt to deny its correctness, that these efforts ended in failure. As I point out in my conclusion: '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 conclusions could be derived.'

I do not see that Luborsky in any way disagrees with this statement; if he has found data to support the hypothesis of psychotherapeutic usefulness he certainly fails to mention them. He does say explicitly 'we do not have any studies which validly compare psychotherapeutic treatment results with the absence of such treatment'. I take this to be in complete agreement with my own conclusion that there is no evidence to show that psychotherapeutic treatment is successful. (Luborsky says in a footnote to the above sentence, 'Eysenck neglects the *one* outstanding exception'. This exception turns out to be a set of mimeographed interim reports of an experiment which, when it is completed and published, will undoubtedly be relevant to the problem under consideration.)

With regard to Luborsky's first criticism, then, we must conclude that it rests on a misunderstanding. Whether we are willing to accept the researches reviewed by me, bad as they are, as being of interest, or whether we reject them all, as Luborsky would seem to wish us do, we come to the same conclusion, namely, that *there is no scientific evidence*

regarding the beneficial effects of psychotherapy. This is the point I wished to make and this is the point which Luborsky apparently agrees with.

What consequences follow from this demonstration? The views expressed in my paper were these: 'Until such facts as may be discovered in a process of rigorous analysis support the prevalent belief in the therapeutic effectiveness of psychological treatment, it seems premature to insist on the inclusion of training in such treatment in the curriculum of the clinical psychologist.'

Luborsky regards this as a *non sequitur*, i.e. he does not feel that this conclusion follows logically from the premises. I may have been at fault in not stating my major premise and not putting down in detail the intermediate steps of the argument. The main reason for this neglect was that they seemed to me self-evident; apparently I was wrong in this assumption. My main premise, then, is that psychology is a science; in other words, that it deals with observable facts and verifiable theories and that that which is not observed or verified does not form part of psychology. A clinical psychologist on this view would be a psychologist who applies approved scientific methods in the abnormal field and uses his scientific knowledge in dealing with the problems that arise. To apply methods having no proper rationale and to use treatments, the hypothesis of whose effectiveness is not supported by any empirical studies, does not appear to be the proper exercise of his scientific training, and it would seem to follow that such training should not include therapeutic procedures until their effectiveness had been proved beyond reasonable doubt. Lehner's comment seems apposite here, referring to 'therapy as an undefined technique which is applied to unspecified problems with non-predictable outcome. For this technique we recommend rigorous training.'

Luborsky attempts to gain sympathy for his plea by saying that 'if medical doctors had followed such advice their entire science would not have developed'. This statement ill agrees with my reading of medical history: I cannot recall any reputable method of treatment in modern times, used as widely as psychotherapy, and advocated as enthusiastically, which has been in use for over fifty years without any serious attempt to prove its effectiveness. This fact in itself is an interesting social phenomenon on which much could be said.

In summary, it appears that Luborsky has misunderstood the logic of my analysis, and that he does not, in fact, disagree with my conclusions. His disagreement with deductions made from these conclusions may be based on genuine differences of opinion, or on the fact that the major premise of my argument was not explicitly stated. If the latter, then it may be assumed that we are now in agreement; if the former, then we disagree in the sense that I regard psychology as a scientific discipline, while Luborsky does not. On this issue the reader no doubt will form his own opinion.

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