

NOTE ON "FACTORS INFLUENCING THE OUTCOME OF PSYCHOTHERAPY"

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It is argued that it is improper to draw conclusions from studies using psychotherapy regarding causal influences unless control groups are included in the analysis (or are available in the literature) which give information about the changes that would have taken place without therapy (spontaneous remission). It is suggested that the conclusions drawn by Luborsky, Chandler, Auerbach, Cohen, and Bachrach from their 1971 survey of empirical study do not follow from the data, but may be simply artifacts due to their failure to include a proper base line in their computations.

In an important recent paper, Luborsky, Chandler, Auerbach, Cohen, and Bachrach (1971) have undertaken to review the evidence regarding factors influencing the outcome of psychotherapy. Their stated aims were threefold: to offer guidelines to clinicians; to compare clinical and quantitative factors, in the hope of indicating what should be investigated by quantitative research; and to provide a methodological evaluation of the research as a guide to future investigation. Their results are interesting and may prove in due course to have highlighted the most relevant and important factors. In this brief note, an attempt is made to take issue with a major methodological point, neglect of which may render their endeavor nugatory. Put briefly, it is suggested that when we are dealing with an attempt to study the influence of certain factors on certain types of activity, then we must include in our evaluation control groups not exposed to any of the factors under investigation, or we must demonstrate unambiguously that in the absence of certain factors studied, there are no significant changes in the activity in question. Such a base line is absent from the review given by Luborsky et al. (1971), and there does not appear any realization in the paper that without it the conclusions drawn do not follow in any rigorous way from the data.

This would not be so serious if control groups not exposed to psychotherapy, as de-

finied by Luborsky et al. (1971), did not in fact show significant change during time intervals comparable to those used in the studies reviewed by them. This is not so. Eysenck (1952, 1960, 1969) has reviewed the evidence on "spontaneous remission" and has shown that while the studies in this field are methodologically no more sophisticated than are those concerned with the effects of psychotherapy, nevertheless, they do show an impressive degree of agreement. Severely ill psychoneurotic patients, not treated by psychotherapy or any other type of psychiatric treatment, improve and get better to a very significant extent; the formula $X = 100 [1 - 10^{-.00435N}]$ was suggested as fitting observed results quite closely (X = proportional improvement; N = number of weeks since beginning of experiment). Eysenck (1960, 1969) suggested that unless treatment can be shown to improve on this no-treatment base line, then it is scientifically impossible to defend the hypothesis that treatment has in fact had any effect; all the effects observed could be due to spontaneous remission. Eysenck (1963) has also elaborated a theory of spontaneous remission that may suggest reasons for the occurrence of these curious and important effects; this theory is derived from Pavlovian and Hullian formulations of conditioning and extinction phenomena.

Kiesler (1966) has thrown doubt on the existence of spontaneous remission, and referred to it as a "myth" which, he believes, "has been perpetuated primarily by Eysenck [p. 114]." He suggested that the evidence

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seems to lead unequivocally to the conclusion that there is no evidence for spontaneous remission of psychoneurosis. Hence the belief seems to be nothing more than a myth propagated by a popularized and naive interpretation of two research studies [the studies of Landis, 1937, and Denker, 1947] [p. 119].

This statement is not correct; many more studies demonstrating spontaneous remission are quoted by Eysenck (1960), and even by Kiesler himself. Luborsky et al. may not agree with Eysenck's estimate of the importance of spontaneous remission, but if so it would have been useful had they indicated in their paper their reasons for such disagreement. The evidence, such as it is, is remarkably unanimous in its results (Rachman, 1971).

Luborsky et al. (1971) do, in fact, mention the possibility of spontaneous remission, but apparently without realizing that this factor, if left uncontrolled, may vitiate all their conclusions. They stated:

Our survey is limited to the factors which influence change as a result of psychotherapy *only for those who have started psychotherapy. For most of the studies surveyed, therefore, it cannot be determined whether the type of individual who profits most would also have profited from another form of treatment, or from change-inducing experiences which are usually not designated as psychotherapy—or indeed from nothing more than the mysterious changes attributed to the passage of time* [p. 161].

Consider one of their main conclusions, and one which has received the most powerful support from a large number of studies.

In 20 out of 22 studies of essentially time-unlimited treatment, *the length of treatment was positively related to outcome; the longer the duration of treatment or the more sessions, the better the outcome!* It is a temptation to conclude—and it may be an accurate conclusion—that if psychotherapy is a good thing, then the more the better [p. 154].

Luborsky et al. (1971) also discussed two alternative possibilities, but they made no mention of the most obvious hypothesis to explain their results: If untreated neurotics improve roughly in accordance with the Eysenck formula, and if treatment has no effect whatever, then we would expect precisely the observed effects—improvement over time! Methodologically it seems impossible to conclude anything from the studies surveyed unless it can be shown that the rate of pro-

gress was significantly faster than would be suggested by the Eysenck formula. The conclusion seems forced upon us that the neglect of spontaneous remission, and the absence of proper control groups not receiving treatment by psychotherapy, make it impossible to regard the conclusions drawn by Luborsky et al. (1971) as following rigorously from their data.

Little can be said by way of conclusion, other than that the findings of the Luborsky et al. (1971) article cannot be accepted as delineating factors influencing the outcome of psychotherapy; it is possible that the conclusions are in fact correct, but the fundamental methodological error underlying all their work makes it impossible to derive any conclusions from the facts presented. Luborsky et al. (1971) did not discuss the writer's attempts to unify the whole field by suggesting similar mechanisms at work in behavior therapy, psychotherapy, and spontaneous remission, nor did they quote papers supporting the conditioning theory of neurosis implicit in this attempt (Eysenck & Rachman, 1965); this makes it difficult to know how they would react to these arguments. It is not essential, however, for these additional hypotheses to be correct in order to see that the paradigm adopted by Luborsky et al. (1971) is in fact erroneous; all attempts to judge the importance of factors that lead to a departure from a base line require the establishment of such a base line. Without base line, no comparisons of a worthwhile, rigorous nature are possible (Eysenck, 1972).

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