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I preface my remarks by emphasizing that though I am a psychotherapist and although I feel—as do my wife and friends—that my own experience of psychoanalysis was very beneficial to me as a person, I agree with Dr. Eysenck that the present state of scientific evidence fails to show that the broadly-defined "uncovering, insight-facilitating, interpretative, analytically-oriented" kind of psychotherapy is a highly efficacious procedure. I further believe that, studies aside, our present power to help is in reality quite limited, so that my critical remarks on research studies must not be taken as meaning that, if they were "done right," they would prove psychotherapy to be a powerful form of behavioral engineering. I think it is not, and to this extent I share what I take to be Dr. Eysenck's substantive opinions. Nevertheless, I shall offer two suggestions which have not, to my knowledge, been made previously with anything like sufficient force.

I define patients as "appropriate" for conventional psychotherapy if they are the sort of patients to whom it will make a pragmatically appreciable difference. Two classes are then inappropriate, to wit, (a) those who are going to get better whether they receive psychotherapy or not; and (b) those who are not going to get better whether they receive it or not. (I neglect those who will get worse as a result of receiving it, although I am convinced such a group exists). I next suggest that the great majority of patients who seek professional help belong to one of these two classes; and, further, that the present decision-making policies and procedures of therapists and agencies are so inefficient that selection results in at least half of patients currently *in* conventional psychotherapy being inappropriate. The literature suggests that at least 50–60% of persons seeking help will, if untreated, come to feel within the course of a year or so that they do not need it. These are the people who are fated to "get better anyway," and they will not show up as an experimental difference. On the other side, I believe that a large number—in my experience at least 40%—of patients seeking help are schizotypes in varying degrees of decompensation, and that while they may become better compensated from the relationship, their basic defect is a constitutional, neurological aberration on which is superimposed a largely irreversible set of aversive social learnings, so that when they terminate the therapeutic relationship they tend to exacerbate. Except for research and teaching purposes, I doubt that with the present shortage of personnel the intensive treatment of these patients can be socially justified. The schizotypes plus a smaller number of cyclothymes, essential psychopaths, minimal brain-damaged (undiagnosed), and persons in unmodifiably bad reality-situations constitute the "bad-outlook" group. I predict that when adequate evidence is in, we will realize that *at most* 1/4 of all patients currently in psychotherapy are appropriate cases in the above-defined sense. The director of our psychiatric outpatient clinic, a psychoanalyst, considers that the appropriate group make up about 10% of its clientele. Personally, I reject the great majority of cases who seek me out as a psychotherapist for the above reasons.

My more radical hypothesis concerns the population of professional helpers. One notes a striking difference between the discrimination shown by professionals when they refer a stranger versus someone they love (a close friend or family member). An informal poll shows that my colleagues have a little list of therapists they consider well-qualified, as judged from training, experience, feedback from previous referrals, and—quite importantly—personal contact, both social and professional. These "lists" usually contain around a half-dozen names. Such a figure

represents less than half (actually closer to 1/3 or 1/4) of the supply of local practitioners one knows moderately well in the above respects. The selection of names you hear from colleagues is far from random, i.e., there is a striking consensus among these informal "lists." The majority of local practitioners fail to appear on *anybody's* list.

Now let us suppose that 1/4 represents an upper bound on the proportion of patients currently receiving conventional psychotherapy who are appropriate; and let 1/4 also represent an upper bound on the proportion of therapists who are much good at their job. Assuming an essentially random model of patient-therapist pairing, the joint probability of the suitable patient getting to a suitable therapist is around .06, a very small tail to wag the statistical dog in an outcome study.

I believe that doing more outcome studies is a waste of time unless these factors are attended to. Patient selection should be made by actuarial methods, combing psychometrics, objectified life-history, and semi-objectified Mental Status (e.g., Q-sort) information for prognostic purposes. It is especially critical—and I know of no published study which has done this—to pre-select effective psychotherapists. Since we do not know how to do this psychometrically, we have to treat our psychotherapist pool as we do potential test items, i.e., we select empirically and cross-validate. We begin with a larger pool of available practitioners (perhaps coarsely screened sociometrically via peer-group) and assess their outcomes over a period of time. We identify the small minority—say, 15–20%—who appear to be reasonably good at their work in this initial screening phase; these are the guinea-pigs in the major outcome study proper. Part of their apparent competence would, of course, lie in their superior case-selection. Hence the control group must consist of patients initially judged appropriate by these skilled therapists and *then* assigned to the untreated group by a randomizing procedure. I presuppose here the medical model, in which decision as to treatability and treatment-necessity are part of good clinical practice.

I am myself not much interested in any further outcome studies which fail to meet these conditions. While I concocted the above numbers without scientific data, I am deadly serious in calling them upper bounds. I am prepared to record the prophecy that future investigators will look back upon our present day outcome studies as extremely crude, and as showing that we did not take the time and trouble to do the kind of study which our clinical insight and scientific knowledge show to be needful if the findings are to be really illuminating.

On the other hand, it is a consequence of the above analysis that Dr. Eysenck would be correct in saying, "Current psychotherapeutic practice, given the actual skill level of the modal practitioner in case-selection and treatment, is largely ineffective," since 6% is a pretty small figure.