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Comments

What Price Psychotherapy? A Rejoinder

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In 1952 Eysenck threw down the gauntlet, when he claimed that psychoanalytical forms of psychotherapy were no more effective than spontaneous remission. As we trudge through the fourth decade of this debate, its quality remains as divisive and acrimonious as ever. Professor Michael Shepherd (1979, 1980) has latterly taken on Eysenck's mantle, averring that psychotherapy is not only ineffective but may actually harm patients. In an editorial in the *British Medical Journal* (1984), he launched yet a further attack, arguing on this occasion that the psychotherapist is little more than a 'placebologist' exerting his effects through non-specific means. Why bother with highly trained therapists when an inert pill will produce the same result?

In criticising psychotherapy as he does, Professor Shepherd's scholarly vision is, in our opinion, distorted by a lack of objectivity. This is particularly evident in the editorial, where he latches on to a review of outcome studies of psychotherapy by Prioleau *et al* (1984), which purports to show that 'for real patients there is no evidence that the benefits of psychotherapy are greater than those of placebo treatment'. In supporting this conclusion, Shepherd reveals a disregard for the several weaknesses of the Prioleau work, as well as a neglect of a number of other recent reviews of the literature on the outcome of psychotherapy, which are substantially more sound and relevant (Smith *et al,* 1980; Andrews & Harvey, 1981; Landman & Dawes, 1982; Shapiro & Shapiro, 1982; Dush *et al,* 1983; Miller & Berman, 1983; Nicholson & Berman, 1983; Quality Assurance Project, 1983).

Prioleau *et al* (1984) adopted what has become a popular approach in recent years—meta-analysis. This radically innovative procedure enables the magnitude of the effects of treatment to be calculated, thus avoiding the limitations inherent in traditional reviewing methods, in which there is an obligation to arrive at a qualitative judgement about the effectiveness of a particular mode of therapy. A statistic—the 'effect size'—is applied to the results of outcome studies published in the literature, and is calculated as the mean difference between treated and control (no-treatment or placebo) subjects, divided by the standard deviation of the control group.

We ourselves would have assigned a low priority to the Prioleau review, compared to other meta-analytical studies, but since Shepherd relied very heavily on it, we are obliged to consider their work carefully. In fact, using a previous meta-analysis by Smith *et al* (1980), the Prioleau team extracted studies (but deleting those that were exceedingly flawed) comparing psychotherapy—loosely defined as the exploration and clarification of a patient's emotional experiences—with a placebo control, operationally defined as any procedure described by the researcher as a placebo in which the potential of benefit is conveyed to the patient. The effect sizes in the 32 studies were then calculated, yielding a mean of .42 which indicates that the average recipient of treatment would be better off than 70% of the placebo control group. But, because the distribution included one extreme value, the authors also derived a median effect size—of .15—and focussed on this latter figure to support their conclusion of the equal efficacy of therapy and placebo. That such a conclusion is misleading, inaccurate, and constitutes a massive generalisation is shown by the following observations:

1. The psychotherapy treatment was quite unrepresentative of conventional clinical practice; only eight studies, for instance, involved 'real' patients. Subjects in the other 24 studies included college students with speech anxiety, disruptive schoolchildren, institutionalised adolescent orphans, and underachieving high school pupils (in fact 13 studies were of children and ten of college students). What a hodge podge! The same could be said of the therapists, who ranged from elementary school teachers, an education graduate student, graduate students in counselling, and junior high school counsellors to psychologists, psychiatric social workers, and psychiatrists. It is obvious that many and probably most of these therapists had had limited training and even less clinical experience. Furthermore, the average duration of treatment was quite unlike conventional dynamic psychotherapy: the modal amount of treat-

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ment was between five and ten hours, and in only three studies did the number of sessions exceed 20. In one series, the subjects got a princely total of 45 minutes of treatment! As for the forms of therapy administered, these included: social learning, rational-emotive, Rogerian, supportive, reinforcement, humanistic counselling, problem-solving, and play. Only a minority of the studies tested approaches which are conventionally classed as examples of the psychodynamic school. We would therefore strongly support Greenberg (1983), one of the discussants of the Prioleau review, when he observes that: 'Overall, the characteristics and heterogeneity of the few studies selected for review suggests that they are unsuitable as a basis for drawing sweeping inferences about outpatient psychotherapy'.

2. The placebo treatment condition does not fulfil the customary criteria of placebo in many of the studies reviewed by Prioleau *et al.* Can relaxation training be regarded as placebo, especially when applied to patients with phobias or to students with speech anxiety? What about a problem-solving group for disruptive high school pupils? Or college students with speech anxiety reading a text on a rational-emotive therapy? These approaches cannot reasonably be conceptualised as placebo in type.

We alluded earlier to a number of other meta-analytical studies. In some of these (Smith *et al,* 1980; Andrews & Harvey, 1981; Miller & Berman, 1983; Quality Assurance Project, 1983), the effects of psychodynamic therapy have been compared with a placebo-controlled condition, along similar lines to the Prioleau work. But, and this is a crucial difference, these other reviewers have been far more circumspect and cautious in interpreting their data. They are also more aware of the limitations inherent in meta-analysis, especially when data from diverse studies are grouped together; they have not jumped to sweeping conclusions about the effectiveness of psychotherapy. Whereas Prioleau *et al* can cite as a 'concluding speculation' that '. . . we see no reason to believe that subsequent research using better research procedures and investigating other types of therapy administered to other types of patients will yield clear-cut indications that psychotherapy is more beneficial than placebo treatment', Andrews & Harvey (1981), referring to their own finding of the superiority of psychotherapy over control/placebo conditions, still raise the question of how *clinically* relevant the finding actually is. Elsewhere, Andrews (1983) comments on the urgent need for more research into the effects of psychotherapy, while Smith & Glass (1977) seem to recognise that integrating outcome data is a complicated matter and one that 'deserves further attention'.

We would strongly concur. But it would be unfortunate if the response to Professor Shepherd was in the form of new research focussing excessively on comparisons between placebo and psychotherapy conditions. This would keep us bogged down in the rather pointless question: 'Is psychotherapy effective?' The meta-analytical revolution has demonstrated consistently that psychotherapy does exert some positive effect (Smith *et al,* 1980; Andrews & Harvey, 1981; Landman & Dawes, 1982; Shapiro & Shapiro, 1982; Dush *et al,* 1983; Miller & Berman, 1983; Nicholson & Berman, 1983; Quality Assurance Project, 1983). What is now required of the investigator in psychotherapy is the execution of studies which attempt: (a) to test whether a specific form of treatment has any particular advantage over an established treatment for a target clinical sample; (b) to tease out certain characteristics of the treatment under study, including certain qualities in the therapist, and to note their effect on outcome; and (c) to examine the influence on outcome of other factors, such as the patient's ability to form relationships, and his motivation for change. The investigator should feel free to apply all manner of experimental design in pursuing these questions, e.g. intensive N = 1, patients as their own controls, and cross-over controls. The carefully and rigorously observed case study should also retain its place: Strupp (1980) illustrates superbly the value of the last in his comparison of successful and unsuccessful cases treated by the same therapist.

Happily, we have witnessed in the last decade distinct progress in the conduct of psychotherapy research; for instance, the Temple (Sloane *et al,* 1975), Penn (Luborsky *et al,* 1980), and Vanderbilt (Strupp & Hadley, 1979) studies. They have demonstrated that with intelligent conceptualisation and careful execution, much can be learned, in terms of the therapeutic process and its effects. The "ill fated" Medical Research Council trial (Candy *et al,* 1972), cited by Shepherd (1984) and by other critics of psychotherapy as demonstrating the complexity and, by implication, the impossibility of carrying out research on the outcome of psychotherapy should be quietly put to rest. Complex the task indeed remains, but its productive performance has been now more than proven to be feasible.

We regard the type of criticism of psychotherapy levelled by Professor Shepherd as tantamount to a regression to the Eysenckian era of the 1950s. In the event, that attack had a profoundly advantageous effect, by stimulating highly productive research. Three decades later, little purpose can be served by a similar unduly pessimistic and rather destructive critical approach. It can neither contribute to the achievement of new knowledge in the field nor can it be of any use to the clinical practitioner. Instead,

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psychotherapy requires an unswerving commitment to its intelligent and rigorous study, as well as the exploration of new paradigms for research. It is particularly in this latter regard that psychiatrists, including Professor Shepherd, can play a crucial, constructive role.

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