

will often take years to complete, that dependent measures must be chosen because they are meaningful, and that the purpose of research is to obtain knowledge, the case for psychotherapy will remain unproven.

REFERENCE

Smith, M. L., & Glass, G. V. Meta-analysis of psychotherapy outcome studies. *American Psychologist*, 1977, 32, 752-760.

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An Exercise in Mega-Silliness

The article by Smith and Glass (September 1977) begins promisingly by referring to my "tendentious diatribes" (p. 752) on the outcome problem in psychotherapy, inviting the reader to study two papers of mine, one of which I have no recollection of writing, and which the reader will look in vain for in the journal appearing in the list of references—indeed, the year and volume number given in the reference do not agree! The authors go on to the "astute dismantling of the Eysenck myth" (p. 752) by Bergin, not mentioning that the Bergin myth has in turn been astutely dismantled by Rachman (1971). No discussion of the issue can be regarded as meaningful which accepts the quite erroneous and—indeed, in places—absurd arguments of Bergin and pays no attention to the serious criticisms brought forward by Rachman. Indeed, the latter is not even mentioned in the bibliography, although his book *The Effects of Psychotherapy* is a classic in the literature. Smith and Glass have a somewhat arbitrary method of reference selection that does not augur well for their major opus.

This major opus disregards completely some of the major findings of Rachman's book, for example, that there are large differences in recovery (spontaneous remission) between different types of patients; the analysis takes no account of this effect. The analysis accepts without a word of

warning subjective reports of therapists as a major source of information on outcome, although it is well known that such assessments are extremely unreliable even when made by well-qualified psychiatrists who have no personal involvement in the patient's recovery (Block, Bond, Qualls, Yalom, & Zimmerman, 1977); the distortion which is likely to arise when a therapist assesses his own patients' progress can well be imagined. It is noted that "subjectivity of the outcome measure" has much the highest correlation with effect size; this alone would invalidate all the complex statistics offered. Smith and Glass also do not mention the problem of selection, so well discussed by Rachman; patients for psychoanalysis are much more highly selected (for high intelligence, emotional resources, ego strength, etc.) than are patients for behavior therapy, and hence much more likely to improve spontaneously.

The most surprising feature of Smith and Glass's (1977) exercise in mega-silliness is their advocacy of low standards of judgment. More, they advocate and practice the abandonment of critical judgments of any kind. A mass of reports—good, bad, and indifferent—are fed into the computer in the hope that people will cease caring about the quality of the material on which the conclusions are based. If their abandonment of scholarship were to be taken seriously, a daunting but improbable likelihood, it would mark the beginning of a passage into the dark age of scientific psychology.

The notion that one can distill scientific knowledge from a compilation of studies mostly of poor design, relying on subjective, unvalidated, and certainly unreliable clinical judgments, and dissimilar with respect to nearly all the vital parameters, dies hard. This article, it is to be hoped, is the final death rattle of such hopes. "Garbage in—garbage out" is a well-known axiom of computer specialists; it applies here with equal force. There is only one sentence in the article with which one can wholeheart-

edly agree: "Extracting knowledge from accumulated studies is a complex and important methodological problem which deserves further attention" (p. 760). Until it has received such further attention, it would be highly dangerous to take seriously the "results" reported by Smith and Glass. Only better-designed experiments than those in the literature can bring us a better understanding on the points raised; in particular, placebo groups must be included in all designs which aim to study therapy-specific effects, and several therapists must be included for each method in order to obtain evidence on the therapist variance. I would suggest that there is no single study in existence which does not show serious weaknesses, and until these are overcome I must regretfully restate my conclusion of 1952, namely that there still is no acceptable evidence for the efficacy of psychotherapy.

REFERENCES

- Block, S., Bond, G., Qualls, B., Yalom, I., & Zimmerman, E. Outcome in psychotherapy evaluated by independent judges. *British Journal of Psychiatry*, 1977, 131, 410-414.
- Rachman, S. *The effects of psychotherapy*. London: Pergamon Press, 1971.
- Smith, M. L., & Glass, G. V. Meta-analysis of psychotherapy outcome studies. *American Psychologist*, 1977, 32, 752-760.

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Reply to Eysenck

We have numbered our responses to correspond to the successive paragraphs in Eysenck's (this issue) rejoinder:

1. Of our two citations of Eysenck's work, the 1952 reference is correct. The 1965 reference should have been to the *International Journal of Psychiatry*, not the *Journal of Psychology*. We read Rachman (1971) when it appeared and again when we began the project on which we recently reported. On the sub-