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Research in Psychotherapy

SIR: The report by Shapiro & Firth (*Journal*, December 1987, **151**, 790–799) provides an opportunity to ponder on the requirements of psychotherapeutic research. I venture to make my criticisms in the hope that debate will lead to improvement in this field of study. I will consider some principles under the following headings: (a) specifying the therapeutic intervention; (b) defining the subjects treated; (c) consideration of assessment methods; and (d) the design of the study.

Firstly, the therapeutic techniques. Any single technique, such as anxiety management training, contains within its structure a sufficient number of potentially therapeutic interventions to keep a large number of researchers busily occupied for many years in the effort to determine which are the most important interventions. The authors quote the 'dodo-bird verdict', i.e. that all therapeutic methods have some effect; alright then, but what is now required is enquiry into the effective elements. To amalgamate a number of techniques does little to advance knowledge, since one cannot know what, among the pot pourri of strategies, was bringing about the improvement.

Secondly, the characteristics of the sample of subjects must be most carefully described if there is to be any hope of drawing useful conclusions from the study. The authors' sample consisted of patients referred by doctors and people who had referred themselves; there was some negative information – i.e. that they had not suffered from psychiatric disorder for more than two years and that psychotic and obsessional symptoms were absent; all complained that their work was affected by their problems; but there description closed. Judgement of psychotherapeutic procedures has too often led to scepticism because of their practitioners' claims that all people will benefit no matter what their disorder or problem may be. Such claims of universality should be abandoned by serious researchers, who should address the specific issues of just what types of disorder are helped by exactly which therapeutic approach.

Thirdly, assessment instruments must have been shown to be valid and reliable measures in the proposed field of application. The prevalent habit, followed by the authors, of selecting instruments composed of the whole gamut of psychiatric symptomatology (in their case the PSE and the SCL) and reporting change in terms of a fall in scores is not a procedure to be endorsed: it is equivalent to studying a treatment for a cardiac disorder in terms of a measure composed of all symptoms of somatic illness and reporting the result in terms of an improvement in an unspecified number of them. The most meaningful psychotherapy outcome research involves the task of defining the goals of treatment and measuring outcome by some technique such as Goal Attainment Scaling, in terms of the proportion of subjects achieving the aim: Mulhall's Rapid Scaling Technique is another useful device, and the authors incorporated it, although they did not present their results in terms of it.

Finally, the design. Cross-over designs in comparison of treatment methods introduce unfathomable complications in the interpretation of the results. The greatest need in psychotherapy research is not the comparison of one procedure with another, but the ascertainment of the durability of improvement. This requires a long follow-up interval following the intervention. This is a stringent requirement, especially when the duration of research posts is limited, but it is one which must no longer be dodged. Psychotherapy is an expensive intervention and there are those who have argued cogently that present information concerning outcome does not justify its use in a state-funded health service. Psychotherapy researchers must now demonstrate, not that they can produce short-term improvement, but that the improvement is lasting once contact with the therapist has ended.

One more point, regarding cost-effectiveness. Future research reports should clearly state the cost of the treatment in terms of the total time of therapist intervention and the training or skill, and hence 'expense', of the therapist.

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SIR: Dr Snaith's letter provides an opportunity to emphasise the wide-ranging and complementary research strategies that are required in the psychotherapy field. Comparative outcome studies are but one part of the overall effort to develop and identify effective and efficient treatments (Stiles *et al*, 1986). We will reply to each of Dr Snaith's comments in turn.

(a) Therapeutic techniques. Of course any method can and should be analysed into its constituent elements, to identify the helpful components. Such

work is currently underway in the Sheffield programme (e.g. Stiles *et al*, 1988). However, despite the imperfect precision noted by Dr Snaith, comparative evaluation of clinically realistic packages is also required, as a guide for researchers, trainers, and practitioners to the value of investing further effort in the diverse methods currently available.

(b) Patient characteristics. Naturally, it is desirable to define as precisely as we can the nature of the sample in any research. However, the more tight this definition, the narrower the potential range of utility of the findings, and the harder it becomes to recruit patients meeting the criteria and hence to complete the study. Dr Snaith's espousal of what Stiles *et al* (1986) term the 'matrix paradigm' sounds fine in principle, but there is no foreseeable prospect of investigating (say) ten treatments for ten types of patient administered by ten types of therapist in ten different settings, as would be required by sole reliance on this approach to advance the field. In our study PSE-ID-CATEGO diagnoses were obtained, and Firth-Cozens & Brewin (1988) showed that these were unrelated to treatment outcomes. Other analyses are currently considering patient characteristics such as age and initial symptom levels.

(c) Assessment instruments. The assessment of outcome is complex and requires multiple methods, observer perspectives, and degrees of specificity *vs* generality (Lambert *et al*, 1986). Goal Attainment Scaling has not fulfilled its early promise of overcoming the limitations of other methods. We have reported some of our results from Mulhall's PQRST method elsewhere (Barkham *et al*, 1988). Given the patterns of correlations among psychotherapy change measures that are typically found, Dr Snaith's analogy with cardiac disorder is somewhat misleading.

(d) Design. We chose a crossover design, fully aware of the questions that Dr Snaith rightly says it cannot answer, because it enabled us to answer other questions. By holding therapist and patient variations constant, it provides a more precise and sensitive test of the effects of different methods, albeit over a shorter period of time. The Sheffield Project, of which the outcome report is but a part, was designed to enable detailed study of the elements within each treatment associated with immediate session impact and longer-term change over a series of sessions, and this is enhanced by the crossover's control for large and stable individual differences between participants. Although not designed for long-term comparative evaluation of treatments, the Project did include an as yet unpublished 2-year follow-up, which showed that improvement was maintained over that period.

In the psychotherapy field, no single study can meet all methodological desiderata simultaneously, so that research design is necessarily a matter of considered compromise between conflicting requirements. In this letter, we have tried to account for some of the decisions that informed the design of the Sheffield Psychotherapy Project. Dr Snaith may be a little happier with the design of the Second Sheffield Psychotherapy Project, currently underway (Shapiro *et al*, 1988). Here, patients are restricted to those presenting with major depression, as defined by DSM-III, and the 2×2 design evaluates long-term effects and cost-effectiveness by comparing 8 and 16-session versions of prescriptive or exploratory therapies. But this does not make it a 'better' study in any absolute sense; rather, it resolves the conflicting desiderata of the psychotherapy research enterprise somewhat differently, and will have different strengths and limitations.

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Mania in the Early Stages of AIDS

SIR: Fenton (*Journal*, November 1987, **151**, 579–588), in a review of AIDS-related psychiatric disorders, referred to a number of cases of psychosis complicating various stages of human immunodeficiency virus (HIV) infection, and Thomas & Szabadi (*Journal*, November 1987, **151**, 693–695) reported a case of paranoid psychosis as the first presentation of acquired immune deficiency syndrome (AIDS) which