SIR: Although I am an analytical psychologist (Jungian analyst), I have gradually become aware of the need to learn from critics of psychological and psychodynamic psychotherapy. Indeed, I have gone so far as to term many such critics 'therapists of therapy' (Samuels, 1992). Reading Andrews' piece with this thought in mind (*Journal*, April 1993, **162**, 447–451), I could see that analysts might well consider explaining more to some patients/clients what it is that seems (to the analyst) to be the matter (as Andrews proposes, p. 450).

Thus far I can make use of Andrews as a therapist of the profession to which I belong. But when he begins to define what he calls 'good clinical care', I have to diverge from his line. The problem is not in the word 'good'; I myself have used the word, conscious of all that it stirs up – and irony is a most valuable spice in a stewing debate (Samuels, 1993). However, and I note it with some amusement, what Andrews defines as good clinical care by psychiatrists does not differ from what already exists and what is well theorised in fields, such as pastoral and other counselling.

In fact, in parts of this country where qualified psychodynamic psychotherapists (never mind consultant psychotherapists) are not present in numbers, it is precisely this kind of therapy that patients/clients are getting. But they are *not*, on the whole, getting it from doctors. They get it from social workers, art and music therapists, occupational therapists, clergy, some psychologists, and so on. The basic texts of these disciplines are replete with the kinds of thinking Andrews supports. His views could have been uttered by my social-work tutors 20 years ago.

So my question is: why doctors? Indeed, given the often noted but insufficiently researched contra-indications for the choice of psychotherapy as a profession, arising from the scientific backgrounds of the majority of medical students, and given that Andrews wants large numbers of his good clinical carers in place, one might seriously question the capacity of the medical profession to deliver the goods.

There is the usual justification for medical training: only doctors can identify non-apparent physical illness or latent but serious psychiatric illness. To some extent, there may be truth in that, though as far as I am aware, the proposition has not been tested.

It seems to me that what we need to do is to seek resources for the establishment of a corps of 'barefoot therapists'. I have no hesitation in claiming for qualified analysts and psychotherapists a role in the training of such people (though not in material matters such as their deployment, renumeration, etc.). After all, when Andrews refers to undercurrents in the relationship using terms such as 'over-determined', 'defences' and 'transference', he is tacitly conceding this point.

The role of the doctor would perhaps remain as gatekeeper and certainly as provider of medical services. The role of the psychotherapist or analyst, in addition to that of training, would be to provide assistance and perhaps treatment for cases that, for some reason, proved too difficult for the bare-foot therapist.

SAMUELS, A. (1992) Foreword to *Psychotherapy and Its Discontents*, (eds W. Dryden & C. Feltham), pp. xi–xvi. Buckingham & Bristol, PA. Open University Press.

—— (1993) What is a good training? *British Journal of Psychotherapy*, **9**, 317–323.

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AUTHOR'S REPLY: The issues of fact raised by the correspondents are addressed in the paper and its references (*Journal*, April 1993, **162**, 447–451); the matter of my provenance and attitude was not.

For ten years I have been involved in projects evaluating the efficacy of psychiatric treatments. I was Director of the Australian Quality Assurance Project (see review; Andrews, 1993) and rapporteur to the World Health Organization Scientific Committee on Psychiatric Treatment (WHO Scientific Group, 1991), and an editor of the consequent book (Sartorius *et al*, 1993). These projects required that I be familiar with the scientific basis for the efficacy of all psychiatric treatments, and necessitated my working closely with psychiatrists who were expert in either dynamic psychotherapy, cognitive-behavioural therapy, drug therapy, or physical treatments, as they prepared their accounts of the art and science of treatment. Since the predicament of someone with schizophrenia, depression, or agoraphobia is likely to remain the same from one decade to another, I expect these accounts of the art of therapy will be of value for some time but, as science only issues interim reports, I am prepared for the scientific conclusions of these projects to date more quickly.

As a result of my involvement with these projects I have come to the conclusions that, *circa* 1993, the cognitive-behavioural therapies are better, and sometimes surprisingly so, than good clinical care, and that the improvement is long-lasting and can result in personality maturation and cure. Conversely, while the dynamic psychotherapies are better than no treatment, they are not better than good clinical care, and are more harmful and more expensive than either good clinical care, the cognitive-behavioural therapies or, for that matter, the drug therapies. In

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these straitened financial times I am of the opinion that neither private health-insurance funds nor public health budgets should be used for dynamic psychotherapy. They should be used for the other, more effective treatments.

I consider that all psychological treatments should be evaluated as to their efficacy, safety, and cost, then registered, exactly as drug treatments are at present, before being provided by a health service. My position is, therefore, very similar to that proposed by Cochrane (1971), that no new treatments should be introduced into medicine unless they have been shown to be more effective, or as effective but safer and cheaper, than existing treatments. It is, therefore, important to use shrinking health-budgets to support the proven remedies to comfort, relieve, and cure the maximum number of patients, and not to use them to support a sectarian battle between the psychotherapies.

ANDREWS, G. (1993) Treatment outlines for Australian psychiatry: an overview of the project. *Epidemiologia e Psichiatria Sociale (EPS)*, (in press).

COCHRANE, A. L. (1971) *Effectiveness and Efficiency: Random Reflections on the Health Service*. Oxford: Nuffield Provincial Hospitals Trust.

SARTORIUS, N., DE GIROLAMO, G., ANDREWS, G., *et al* (1993) (eds) *Treatment of Mental Disorders: A Review of Effectiveness (World Health Organization)*. Washington: American Psychiatric Press.

WHO SCIENTIFIC GROUP (1991) Evaluation of methods for the treatment of mental disorders, (rapporteur G. Andrews). *WHO Technical Report Series*, **#812**.

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Treatment resistance in schizophrenia

SIR: Despite major advances in our understanding of the neurobiology of schizophrenia, treatment resistance remains a challenge to the clinicians. Treatments using psychological principles have rarely been subjected to the rigours of controlled trials. Hence we read with interest Tarrier *et al*'s study (*Journal*, April 1993, **162,** 524–532). They reported a significant reduction in psychotic symptoms in drug-resistant schizophrenics treated with cognitive–behavioural methods. They also reported that, at six-month follow-up, five patients (22%) were in complete remission. The method is sound and the findings are highly impressive. However, a few issues are worth considering.

(a) The patient sample was drawn from community settings; it could be argued that they were well enough to be in the community and hence not representative of the truly resistant sample who usually remain in the hospital.

(b) There is a high drop-out and refusal rate. Out of the 48 suitable patients, only 56% continued to post-treatment and 48% to six-month follow-up. Of those who dropped out, two were incoherent and six were readmitted to hospital on account of violent behaviour or affective symptoms. Again, this would suggest that those who remained in the study were less intractable.

(c) Perhaps it would have been interesting to obtain some information about those who dropped out of the study, although we do appreciate the Herculean task involved in such an endeavour.

(d) It has been reported that positive symptoms showed substantial fluctuations despite the patients being on stable psycho-pharmacotherapeutic regimes (Johnston *et al*, *Journal*, January 1987, **150,** 60–64). Hence remission of psychotic symptoms in 22% of patients in this study should be viewed with caution.

(e) The authors mention that the patients were resistant to regular and stable neuroleptic medication, but unfortunately there is no mention of the groups of drugs tried, the dosage, duration, compliance, blood levels, and so on. The definition of drug resistance itself would otherwise be open to criticism.

(f) It would have been interesting to know whether the patients were on medication during the study period.

However, despite these finer points, this study offers hope from therapeutic nihilism and emphasises the need for conducting controlled trials of psychological treatments in schizophrenia.

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AUTHORS' REPLY: We were pleased to note Mirza *et al*'s positive comments about our recent report and we welcome this opportunity to respond to the points that they make, we will respond to these in the order that they are presented.

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