and to symbolise her. The child throws a spinning reel away and exclaims "fort!" ("gone"); he pulls it towards him – it is "da" ("here"). Thus language is necessary because no relationship with another can be perfectly satisfying; there must always be some separation between mother and child, child and siblings, subject and other. Language also confers on us a history – another way in which it differentiates us from other primates. In analysis, for Lacan, "the subject assumes his own history" (Lacan, 1953). It is not the gaining of insight which is therapeutic in psychoanalysis (and Freud never said anything about insight), it is the very act of speaking, "the putting into words of the event ... [which] determined the lifting of the symptom" (Lacan, 1953).

It may be that this "talking cure" has implications at a neurochemical or neurostructural level. We know that the process of memory storage involves changes at these levels and it is reasonable to assume that the retrieval of memory and the remembering through speech in analysis results in similar changes. I do not wish to attempt to direct correlation between the praxis that is psychoanalytic discourse, and the science of neurobiology. Nevertheless it is interesting that Thomas & Fraser end their paper with a brief review of language therapy as a way of altering faulty discourse. It would appear that these therapies have some overlap with psychoanalysis.

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A. CAMPBELL

St. Vincent's Hospital
Elm Park, Dublin

Psychiatrists and priests

SIR: Sims (*BJP*, October 1994, **165**, 441–446) asserts that "We need to balance the importance of the spiritual in the life of our patients with denying absolutely any sort of priestly role for ourselves as psychiatrists."

In the course of psychotherapy, we cannot avoid being put into a priestly role by some of our religious patients, any more than we can avoid being put into a parental role by patients who need to work through, or who attempt to act out, something about their relationship with their real or internal parents. In both cases we should interpret the transference processes to enable our patients to achieve more healthy and realistic relationships with these figures.

Most priests believe they have the authority to convey forgiveness to those who feel guilty. No psychiatrist does this in so many words, but in therapy a similar process often takes place. A depressive patient gradually realises that the behaviour for which he blames himself was only partially under his control and that he was driven to act as he did by his past experiences. Thus his guilt diminishes. Another patient may accept more responsibility for his behaviour and feel ashamed. Because the therapist does not condemn him, and may, to his surprise, still hold him in high esteem, he can internalise this experience and forgive himself. So, through his relationship with his therapist, his 'sin' is forgiven.

In the course of the work, the therapist conveys to his patient a sense that he is valuable, lovable in spite of what he is like at the moment, and worthy of all the care he needs, even if it is not practically possible to give so much. The response of some patients to this is like a religious experience. Then the therapist is functioning very much like a priest, even through neither party acknowledges the spiritual aspect of their activity. When such things happen in our work I think we should accept them humbly and with appropriate awe, rather than deny our priesthood as Sims seems to require us to do.

A. STEDEFORD

71 Sandfield Road
Oxford OX3 7RW

Comparing treatments for generalised anxiety disorder

SIR: Durham *et al*'s comparison of cognitive therapy, analytic psychotherapy and anxiety management training as treatments for generalised anxiety disorder (*BJP*, September 1994, **165**, 315–323) made certain assumptions which would seriously question the validity of their results. These assumptions fall under the uniformity myths for psychotherapy research. First, the patient uniformity myth assumes that all patients who suffer from the disorder are expected to respond, irrespective of their underlying psychopathology, to any of the three treatments, in a similar fashion.

This patient sample had a significant bias towards the lower social classes. Forty-six per cent were also diagnosed as having an avoidant or dependent personality disorder, and 66% were taking psychotropic medication. These factors suggest

that they would be poor candidates for AP (Overall & Aronson, 1962), hence biasing the results towards CT.

Second, the treatment uniformity myth posits all three psychological treatments will effect similar changes within the same time framework. The authors acknowledged that not all three therapies aim for similar changes: AP aims at more fundamental personality changes, while CT and AMT focus on symptom reduction. Furthermore, these approaches do not effect changes in the same time frame. The three cannot be meaningfully compared.

There are several potential methodological errors. In AP, patients tend to respond with greater variability and so some may have improved greatly, while others became worse, especially during the initial period, i.e. within the time frame of the present study. By randomly assigning patients and then averaging out ratings, the positive and negative results will cancel each other out, thus the scores clearly do not reflect the true picture. On the other hand, using instruments primarily to assess symptoms, the types of changes aimed for in AP are not elicited.

Third, the therapist–patient dyad uniformity myth assumes that the therapist–patient interaction is similar no matter who the therapist is, nor who the patient is, and whether the two 'fit' with each other. This factor simply cannot be ignored (Hjelle & Clouser, 1970).

We are not questioning the effectiveness of cognitive therapy in anxiety disorders. We believe the conclusion drawn should be rephrased: Cognitive therapy is likely to be more effective in *symptom reduction* than psychodynamic psychotherapy with chronically anxious patients *within 20 sessions*. Unfortunately, some fifty years since earlier, biased views of psychotherapy (Eysenck, 1952), the trend to prove Brand A is better than Brand B has not let up. Time, money, and effort are still being put into trying to find the most cost-effective treatment, without paying attention to the individual needs of the patients. The most sophisticated statistical manoeuvring will not hide the inadequacies of a poor design (Morstyn, 1993).

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L.Y. CHENG
H. BAXTER

The Chinese University of Hong Kong
Prince of Wales Hospital
Shatin, Hong Kong

AUTHORS REPLY: Cheng & Baxter may well be right that a similar comparison to ours but with a more affluent patient sample, a longer treatment time, a closer focus on personality changes and a more careful matching of patient and therapist, would have produced a different result, perhaps one more favourable to analytic psychotherapy. But it is less clear why they believe such an investigation would necessarily be more valid or meaningful. Our purpose was to investigate the relative effectiveness of three well-known psychological therapies in terms that made sense within the clinical setting in which we worked. This setting, typical we suspect of most psychiatric services, is one in which resources do not permit lengthy psychotherapy as a matter of course, where the majority of patients are mainly concerned with symptom reduction rather than with personality change, and where careful matching of patient and therapist is not usually practicable. Our study was not intended to be an exercise in proving that "Brand A is better than Brand B"; it was rather a collaborative effort between psychotherapists of different persuasions and training to investigate the scope and limitations of those psychological treatments that are currently available in a typical NHS setting. By carefully specifying the characteristics of the patient sample we hoped to make it clear to which population our results might be generalisable.

The more fundamental doubts that they express about the validity of studies of this kind seem to hinge on a sense of unease about a method of investigation in which individual changes are necessarily obscured by statistical comparisons of group means and variances. Experimental control over subject variables by random allocation to groups is, of course, inherently insensitive to individual change. There is always some overlap between the performance of different groups, and the statistical tests of significance available to try and sort out this problem do not tell us much about clinical significance. But surely there are a number of ways of addressing problems of this kind without concluding, as Morstyn (1993) has done, that the whole enterprise is founded on a 'bogus new scientism' and should be thrown out. The limitations of the experimental method can be balanced with single case studies, naturalistic investigations, process studies, and the like. Weaknesses in current