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).

EYSENCK, H.J. (1952) The effects of psychotherapy: An evaluation. *Journal of Consulting Psychology*, **16**, 319–323.

HJELLE, L.A. & CLOUSER, R. (1970) Susceptibility to attitude change as a function of internal-external control. *Psychological Record*, **20**, 305–310.

MORSTYN, R. (1993) Some fallacies of statistical inferences about psychotherapy. *Australian and New Zealand Journal of Psychiatry*, **29**, 101–107.

OVERALL, B. & ARONSON, H. (1962) Expectations of psychotherapy in lower socio-economic class patients. *American Journal of Orthopsychiatry*, **32**, 271–272.

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

https://doi.org/10.1192/S0007125000183539 Published online by Cambridge University Press

methodology can be identified and improved. Measures of the clinical significance of change, for example, can be employed as well as measures of symptom severity.

I believe that the particular needs of individual patients are likely to be best served by systematic and critical enquiry across a broad range of research methods and treatment approaches. Morstyn (1993), by contrast, argues that the psychological processes in some forms of psychotherapy are simply too complex and individual in character to be represented by numerical variables, thus ruling out research methods that employ statistical analysis. These are deep philosophical waters but in accepting Morstyn's argument Cheng & Baxter risk granting some methods of treatment an immunity from public scrutiny. How would they seek to answer common sense questions regarding such matters as the likelihood of symptom remission following treatment? The myth that poses the greatest threat to research in this field is the one that elevates some forms of psychotherapy to a special and mysterious status.

MORSTYN, R. (1993) Some fallacies of statistical inferences about psychotherapy. *Australian and New Zealand Journal of Psychiatry*, **27**, 101–107.

R. DURHAM
Tayside Area Clinical Psychology Department,
Royal Dundee Liff Hospital
Dundee DD2 5NF

The points raised by Cheng & Baxter equate closely with our experiences as the analytical psychotherapists undertaking the clinical work with the AP group in this trial.

In particular once the trial was underway, it quickly became evident that few of the patients would have been offered analytical psychotherapy in our usual clinical practice, and in retrospect it was an error not to have included a measure of anticipated suitability for this type of treatment in our initial assessment.

L. TRELIVING
Royal Dundee Hospital
Tayside

T. MURPHY
Royal Edinburgh Hospital
Edinburgh EH10 5BP

A HUNDRED YEARS AGO

General paralysis

The old proverb – It is the pace that kills – finds nowhere a more striking illustration than in this disease which, under the hurry, anxiety, and excitement of modern life, is becoming more and more common. For some unexplained reason the affection is more common in this country than in many others, and is, comparatively speaking, infrequent in Scotland. This has been explained by Northern patriots as another illustration of the harmlessness of whisky as compared with beer, a fallacious argument, for the onset of general paralysis may have been preceded by no excesses, and, given other factors, may occur in total abstainers. But while much with regard to the etiology remains obscure, one factor is almost always to be traced, namely, hard work under conditions of excitement and responsibility.

Among the educated classes it is men, and occasionally women, in the prime of life, whose careers are arrested at the point when success is almost in their grasp. Among skilled artisans too, it is often the industrious earnest man who is singled out – the man who is always anxious about the quality of his work, and will work all night, if allowed, to finish his task. And there are many, though not so large a number as is usually imagined, in whom previous excess appears to have caused the onset of symptoms; but in many cases this conclusion is due to a confusion between cause and effect, since in most cases of general paralysis a certain amount of moral perversion is symptomatic of the early stage of the disease. The disease is practically confined to the first half of adult life, and those who have stood the strain until they are 50 need have little fear of being attacked by this terrible complaint.

The name of the disease gives little guidance to the symptoms met with, and many different clinical groups are classed under the same head. One feature, unhappily, is common to them all – the progressive character of the paralysis and the absence of any hope of recovery, medicine being powerless at present to do more than delay the inevitable result. In all forms, also, when the disease is once established, there is gradual steady failure of the physical and mental powers of the patient, until finally the paralysis becomes complete, and death closes the scene.

https://doi.org/10.1192/S0007125000183539 Published online by Cambridge University Press