tial response may be regarded as very small. Thus sub-grouping of patients results in wide confidence limits on any estimate of a treatment effect.

Fortunately some remedy to this limitation is at hand. Since the publication of our paper another study (the Leicester trial) has been completed. This trial had some comparabilities of design to our own and the outcome was in certain respects similar. By combining the samples of the two trials it may be possible to arrive at a firmer conclusion concerning the possible predictors of response to real ECT, e.g., whether they include delusions or retardation separately or together, or some other clinical feature. With the collaboration of the Leicester workers such an analysis is now in hand.

We hope that in due course it will contribute an answer to the question that interests Dr Stuart, other clinicians and ourselves, of what are the reliable predictors of response to real ECT.

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What Price Psychotherapy?

DEAR SIR,

The nub of Dr Ryle's argument (*Journal*, August 1985, **146**, 209–210) in his criticism of Professor Shepherd's advocacy of controlled evaluation of psychotherapy is that psychotherapists find the question "Is psychotherapy effective?" inappropriate. This is true insofar as psychotherapy makes no pretention to being a specific therapy and it is then correctly placed among other activities—teaching, parenting—for which Dr Ryle judges the question of effectiveness to be unproductive: a list which, from this point of view, could be extended to include friendliness, kindness and mutual succour. However, where a specific effect, which exceeds that of placebo, is claimed the validity of the claim is crucial: a matter of public concern, for example, when NHS funding of psychotherapy is under consideration.

The issue is fudged by Dr Ryle's inclusion of psychiatry among activities where he judges the question of effectiveness is not appropriate. Many specific therapeutic methods incorporated within psychiatry, somatic, psychological and social, have been either submitted to controlled evaluation or the need is acknowledged. Broadly (e.g. excepting behaviour therapy) the effectiveness of psychotherapy *qua* therapy has not been demonstrated although this has been attempted (Candy *et al*, 1972) and failed for lack of patients judged by psychotherapists as suitable for controlled comparison. Thus their conviction that psychotherapy has a self-evident specific therapeutic effect and the attitude to evaluation it engenders in psychotherapists are obscurantist and explain the commendable persistence of Professor Eysenck and Professor Shepherd in dragging the matter into public view—which Dr Ryle so deplores. That, given the will, the effectiveness of psychotherapy can be weighed in the balance is evident from the study of Sloane *et al* (1975). It is to be hoped that Dr Ryle will see the necessity and wisdom of encouraging such efforts.

It is also worth pointing out that Prioleau's (1983) paper is a re-analysis of studies cited in a report purporting to demonstrate the effectiveness of psychotherapy but which, incredibly, included placebo effect as a mode of therapy. His paper arranged this material so that the effect of psychotherapy was *compared* with placebo effect. The shortcoming of the Prioleau study, which Dr Ryle cites, arises from the dearth of material on which to judge the effectiveness of psychotherapy. He suspects that the attention it has received is attributable to its conclusions. He is right. These tentative as they are, cannot be brushed under the carpet without consigning the credibility of psychotherapy to the dust heap also.

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Munchausen's Syndrome

DEAR SIR,

We should like to report an unusual case history which seems to illustrate an emerging syndrome—Munchausen's syndrome presenting with psychiatric illness.

The patient, a 34 year-old man, presented at a

busy casualty department in a distressed state, apparently having taken a serious overdose of paracetamol, diazepam and alcohol. His story was supported by a letter, headed 'Robert Brothers Circus', in which his employer explained that the patient, a lion tamer with the circus, had seemed unwell for some time, and had recently made three attempts to end his life—once by throwing himself under the wheels of a moving lorry, once by inciting his lions to attack him, and now by overdose.

He was assessed medically and gastric lavage and N-acetylcysteine infusion was instituted. He was admitted to a medical bed and the following day was interviewed by a registrar in psychiatry, who, thinking him to be suicidal, transferred him to a psychiatric hospital.

There he gave a history of six months' increasing unhappiness, with poor concentration and interrupted sleep. He had been preoccupied with a large sebaceous cyst above his left eye, which had made him feel self-conscious in the circus ring—indeed he had attempted to hack this off with a razor blade. A crisis had occurred three months before the overdose when his mother, with whom he shared a caravan, had died suddenly of a heart attack. Since then he had been in a shocked and bewildered state. A diagnosis of bereavement reaction in an abnormal personality was suggested.

He was observed in the ward setting, where he was encouraged to express his grief, and where his behaviour seemed congruous with the harrowing history—until six days later when the charge nurse telephoned the circus about an incidental matter. We learned that his story was an elaborate fabrication—the circus management explained that the patient had never been in their employ, but that over a five year period they had been contacted by several hospitals where the patient had been admitted posing as a lion tamer. It was confirmed that the referral letter was in the patient's own handwriting (and the heading printed by him) and that he had been discharged from the neighbouring psychiatric hospital only two days previously. On confrontation our patient clung to his story steadfastly, but went "absent without leave" later in the day.

Although Asher originally described three well known varieties of Munchausen's syndrome—namely "the acute abdominal", "the haemorrhagic" and "the neurological type"—more recently a psychiatric type seems to be emerging. However, it seems to be a relatively unusual presentation, and perhaps this is why we were not on our guard. We are familiar with "hospital addicts" who use psychiatric symptoms to gain admission for food, shelter and company but this did not seem to be our patient's motive—as he turned down the invitation to stay in hospital, presumably in favour of practising his deception elsewhere.

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DST Results and Platelet MAO Activity

DEAR SIR,

The article by Schatzberg *et al* (*Journal*, June 1985, **146**, 633–637) on a possible correlation between platelet MAO activity and 4 p.m. post-dexamethasone cortisol levels in patients with a depressive disorder prompted us to re-examine our data on these parameters. We have simultaneously done the DST and the determination of platelet MAO routinely on over 400 patients admitted to our hospital. In the whole group we found no correlation between the two supposed biological markers. Evidently most patients were suspected of having a depressive disorder as a DST was requested. Even in a subset of patients with routine diagnosis of major depressive episode, based on the clinical intake evaluation done by psychiatric residents supervised by senior psychiatrists, no correlation between non-suppression in the DST and high platelet MAO activity could be detected.

Fortunately, out of this group a psychiatric resident and his supervising psychiatrist appeared to have selected, for another study and blind to the biochemical data, a number of patients fulfilling the DSM-III criteria for major depressive episode. Looking for a possible correlation between the two markers in the latter group, which consisted of 32 patients, mean age 43.6 ± 11.5 (s.d.) years, range 24 to 61 years, 7 males, 25 females, a striking similarity between our results and those by Schatzberg *et al* became evident. After dividing the group of patients along the median of the MAO activity, as they did, and defining suppression in the DST as a post-dexamethasone cortisol level at 4 p.m. of less than 0.14 μmol/l, we found 14 (44%) non-suppressors, 11 of them in the high MAO group. Of the suppressors, 13 were in the low MAO group. Analysis of the data using the Fisher exact test showed a highly significant difference ($P = 0.011$) in the non-suppression rates between high and low MAO patients.

All patients had received 1 mg of dexamethasone at 11 p.m. and the exclusion criteria for the DST were those given by Carroll *et al* (1981).

We confirm the results presented by Schatzberg *et*