The results therefore provide a new instance of an already observed phenomenon (Phillips, 1974; Surtees, 1982).

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SOMATIC SYMPTOMS OF ANXIETY MOULDED BY EARLY EXPERIENCES

DEAR SIR,

Two patients who had spent some of their childhood in German concentration camps, presented with depressive disorders which responded to tricyclic medication. Both initially complained of burning sensations; one, a lady in her fifties, had severe burning sensations in her arms and the other, a man in his forties, burning sensations and pains in his legs. In both cases the symptom was quickly relieved with benzodiazepine anxiolytics. The similarity in the constellation of the features was striking and suggested that the horrifying early experiences had moulded the anxiety symptoms.

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RE: THE DIAGNOSIS OF DEPRESSION IN OLD AGE

DEAR SIR,

The paper by Dr Elaine Murphy (*Journal,* February 1983, **142,** 111–19) would appear to be an excellent example of a study in prognosis. There is one important omission. There is no detailed account of the treatment of these patients and yet treatment is a very major factor in prognosis. Treatment of depression is not constant and there are effective and ineffective treatments. Electro-convulsive therapy is the most effective measure in the treatment of the severely depressed, particularly in those with delusional features. Even then, the administration of a course of ECT is not constant, for the number and frequency of treatments can influence the prognosis.

It is also likely that a number with severe physical illness would be excluded from having ECT because of potential hazard and so it is not surprising that a poor prognosis was associated with severe physical illness. As 30 out of the 124 died, and only one from suicide, within the first year, it would suggest that these physical illnesses were very severe and that there must have been among those that did not die a number who were also seriously ill. While death cannot be regarded as a good outcome, it is wrong to attribute death from physical causes to the depression, especially as depression is a common feature of organic disease both cerebral and systemic.

Dr Murphy herself in her comments on age and sex (p. 113) states that, "Age did not affect prognosis: older patients were just as likely to make a full recovery as younger ones." Yet the paper concludes that prognosis of depression in the elderly is poor. It would be fairer to say that if the patient has a serious and fatal illness and is probably considered unsuitable for an adequate course of ECT the outcome is unfavourable.

Even the administration of tricyclic anti-depressants can be a hazard in the elderly because of their vulnerability to the anti-cholinergic action of these drugs and it would be of interest to know what dosage of drug was tolerated and how many had to have the drug discontinued.

I stress these points, for in my long experience of treating psychotic depression, I consider the prognosis still to be excellent, regardless of age. In this I agree with Dr Murphy. My concern is that her general conclusions are not supported by her data and that effective treatment of a recoverable illness may be denied people merely on the grounds of age. Dr Murphy's paper does emphasize the importance of a thorough physical screening of the elderly because a number of physical conditions which may well be precipitating the depression are treatable and anti-depressant measures for these conditions would be entirely inappropriate.

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BRIEF PSYCHOTHERAPY IN FAMILY PRACTICE

DEAR SIR,

The study by Brodaty and Andrews ("brief psycho

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therapy in family practice: a controlled prospective intervention trial" (*Journal*, July 1983, **143**, 11–19) raises two points worthy of comment.

1. The authors found no difference between psychotherapy, general practitioner treatment and the control condition. However, they observe that the research design was "vulnerable" because, among other reasons, "the small number of subjects completing treatment increases the possibility of Type II error". They go on to say that "this possibility seems remote as there was not even a tendency towards a differential effect between the treatment groups".

   In fact, the possibility of Type II error (i.e., failure of an experiment, test or study to detect a difference that does in fact exist) in their study is not at all remote. Considering only the comparison between psychotherapy and control groups, and applying the average effect size obtained by Smith and Glass (1977) from their meta-analysis of 96 treatment trials of dynamic psychotherapy to the SCL-90 scores given by Brodaty and Andrews, it can be shown, using formulae available in standard medical statistics texts (e.g., Armitage, 1971), that the probability of type II error is in the region of 25 per cent. It can also be shown that in order to have a 95 per cent chance of detecting a psychotherapy/control difference similar to that derived from the 96 outcome studies assessed by Smith and Glass (1977), about 75 patients in each group would be needed.

2. Brodaty and Andrews screened 1510 consecutive family practice attenders: of these, 700 had high General Health Questionnaire scores and of these, 48 were thought suitable for and accepted specific treatment (i.e., not controls) within the context of the trial. Thus, at best, the results of such a study are applicable to only 7 per cent of those patients with significant psychiatric morbidity who present to general practitioners, and thus of limited relevance to the practical management of psychiatric disorder in general practice.

   Similarly, the MRC trial of dynamic psychotherapy found (Candy *et al*, 1972) that only 7 per cent of referred patients were considered suitable for entry into the trial. Such findings support the observation of Cawley (1971), that "the controlled therapeutic trial is not the only, or necessarily the best, way of examining the case for dynamic psychotherapy".

   The role and value of a psychodynamic approach in primary health care seems *par excellence* an issue which could usefully be studied by alternative approaches. Among such alternatives, techniques designed for the evaluation of health care delivery (see Illsey, 1980) merit serious consideration. Such techniques have been applied, in a series of studies by workers in the General Practice Research Unit at the Institute of Psychiatry, to a similar problem, *viz.*, the evaluation of social casework in the primary care setting.

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CORRECTION

The first sentence in the summary "Are there Anticompulsive or Antiphobic Drugs?" by Isaac Marks (*Journal*, October, 1983, **143,** 338–47) should read "19 controlled studies" not "19 uncontrolled studies".

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