or control groups, or have been given due consideration in our accounts of the 9 month and 2 year follow-ups. We sympathise with the aim of the Schizophrenia Association of Great Britain of identifying the biological basis of the illness, and eventually a cure. However, until that is achieved, it seems to us worthwhile exploring ways in which families can be helped to cope with this devastating illness. In fact, we were able to change the family environment in the desired direction in three-quarters of the families we tried to help. This resulted in a zero relapse rate at 9 months and a 14% relapse rate at 2 years, for those patients who remained on medication. Mrs Hemmings has chosen to concentrate on the minority of experimental families whom we were unsuccessful in helping. The patients in these families did very badly, a fact which causes us great concern. In the light of our accumulated experience of working with the families of schizophrenic patients, we now consider that the families in our trial would have benefited from more contact with us, rather than less, as suggested by Mrs Hemmings.

Regarding the generalisability of our findings, we have never claimed that our results can be applied to families other than those in which a schizophrenic patient is in high contact with a high EE relative. However, three other trials of family treatment have been published (reviewed by Leff, 1985) which cover a wide range of patients and families, each of which achieved benefits for the patients very similar to ours.

J. LEFF
R. BERKOWITZ

MRC Social Psychiatry Unit
Friern Hospital
London N11 3BP

L. KUIPERS

District Services Centre,
Maudsley Hospital
London SE5 8AF

D. STURGEON

University College London

Reference

LEFF, J. (1985) Family treatment of schizophrenia. In *Recent Advances in Clinical Psychiatry, Vol. 5* (ed. K. Granville-Grossman). Edinburgh: Churchill Livingstone.

Depression in General Practice

DEAR SIR,

Dr Sireling and his colleagues recently published two papers on Depression in General Practice to which we should like to respond (*Journal,* August 1985, **147,** 119–136). In them, different groups of "general practice depressives" are compared and contrasted with themselves and with a sample of psychiatric out-patients chosen for inclusion in an antidepressant drug trial. We believe their conclusion that GP depressives are "considerably less severe, with fewer depressive symptoms and shorter illnesses, as well as less primary and less endogenous" than psychiatric out-patients, is one forced upon them by the choice of their samples and by the problems they encountered with low patient recruitment.

To begin with, all patients who had recently received a prescription of antidepressants or had seen a psychiatrist were excluded from the study. We suggest that this strategy will have effectively "creamed off" many of the more severely depressed patients who might otherwise have legitimately been included under the rubric "general practice depression".

Thereafter three groups of "general practice depressives" were obtained. The first two consisted of patients who had been identified as depressed by the GPs, who had been newly prescribed antidepressants (group A) or any other forms of treatment (group B), and had thereafter been notified to the investigators. The decision of a GP to prescribe antidepressants or other treatment (including a simple follow up appointment) is not the same as a diagnosis of depressive illness. It is interesting therefore to note that 20% of the antidepressant group and 50% of the "other treatment" group failed on interview to receive a diagnosis of depressive disorder (major or minor) at all. Despite this, the authors make comparisons of severity between these psychiatric out-patients and the *entire* cohort of patients seen in groups A and B, not just those who had a diagnosis of depressive disorder.

The third group (group C.) of "missed major depressives" too may have been biased towards milder disorders and disorders of a certain kind because of the problems the authors experienced with low patient recruitment: 11% of age-eligible patients failed to complete the screening questionnaire (GHQ), and of those who scored above the threshold, 34% declined the subsequent diagnostic interview. We suggest that the authors will therefore have "lost" a significant number of depressed patients in their sample—especially those who are more severely ill, who are male, and who are somewhat atypical. In our own study of depressive disorder in primary care (Blacker & Clare, in preparation) we obtained a 97·5% GHQ completion rate and 93% interview response rate in a sample of just

over 2300 patients. We found that those patients who were unwilling to complete a questionnaire or be interviewed, were highly likely to be suffering from some form of emotional distress (such as bereavement) or psychiatric disorder (typically alcoholism, major depression, and/or personality disorder). We also found a significant number of patients who had quite severe illnesses such as DSM-III melancholia. Sireling's depressives seem rather mild by comparison and we wonder therefore, since they had a good spread of practices, where the severe cases went? Evidence that Sireling and his colleagues have probably missed a substantial number of potential "cases" in their screening is shown by the low GHQ-positive score rate (24%) they obtained. Generally speaking most other studies report above-threshold rates on the GHQ somewhat in excess of this suggesting therefore that the 11% who declined to complete the questionnaire in the present study may well have been suffering from some form of psychological disturbance. In our sample of 2300 London general practice patients we found that 52% of patients scored above the threshold (4/5) on the GHQ!

This emphasises, we believe, the need for high patient-recruitment rates in epidemiological studies of this kind as well as the problems with interpreting data from studies in which these rates are low. That primary care depressives when taken *as a whole* should be found to be less severe than psychiatric service depressives is not perhaps surprising. One will always find this differential of severity in a health service where referred patients are "creamed off" from a relatively dilute pool of disorders in primary care and concentrated in psychiatric outpatients. The question remains whether those more elusive depressives who defy recognition by the GP, who decline to participate in research studies, who consult less frequently, or who belong to different areas of general practice (such as domiciliary consulters or those who "go to" repeat prescription) are suffering from disorders which might be described as severe and who might, despite this severity, defy referral to psychiatric services? The statistics on suicides, most of whom have been found to have consulted the GP shortly before death but few of whom are in contact with psychiatric services, are one indication that this "elusive" group may be substantial!

C. V. R. BLACKER
A. W. CLARE

St Bartholomew's Hospital
West Smithfield
London EC1A 7BE

Dr Sireling and Professor Paykel Reply

DEAR SIR,

We are grateful for the opportunity of replying to the comments by Dr Blacker and Professor Clare. They clearly believe that general practice depression is more severe than we found it to be, but they have produced no evidence for their belief, and their methodological criticisms have little bearing on the question of severity. We will deal with their comments in turn.

They suggest that the exclusion of patients who had seen a psychiatrist or received antidepressants in the past three months would have "creamed off" many severely depressed, and that the approximately 10% of depressed patients whom general practitioners refer to psychiatrists cannot be legitimately regarded as "general practice depressives".

We would agree that the decision of the general practitioner to prescribe is not the same as a diagnosis of depressive illness: it is precisely because of the ambiguity of the word "illness" that we chose to adopt the operational definition "depressed enough to require treatment". This surely is legitimate "general practice depression". In the same way, the psychiatric outpatients were all regarded as requiring antidepressant treatment—to distinguish between those who were and were not "ill" in either sample would be arbitary. If Blacker and Clare only regard as depressives those patients given a diagnosis of "depressive disorder" (on criteria developed by psychiatrists on the basis of condition seen in psychiatric patients), how would they label the large numbers of general practice patients depressed enough to require treatment, including antidepressants, but not meeting the formal criteria for "disorder"?

We would agree that the patient recruitment rate in our study was lower than we would have liked, but we had to strike a balance between consent and coercion in the surgeries, which may have led to an under-estimation of the frequency of missed depression. Blacker and Clare produce no evidence that the *severity* of cases, as opposed to their frequency, would be underestimated, particularly since all our cases had further to satisfy the Research Diagnostic Criteria for major depressions. As they have not yet published their study, it is impossible to comment on their methodology, but demographic and sampling differences may help explain the disparity between their positive GHQ rate of 52% and ours of 24%—most British studies of consecutive surgery attenders find a rate closer to ours than to theirs, for example, Goldberg and Blackwell (1970) report a rate of 32%, and Goldberg, Kay and Thompson (1976) a rate of 25%.