have been concerned only with parent death, whereas studies of these contaminating conditions have tended to be concerned with a variety of forms of loss, and show (i) that the incidence of parent death is certainly no higher than it is in depressed patients, and (ii) significant relationships are more likely to be found with parent loss due to causes other than death. This is only to be expected, for patients with character disorders are likely to have parents with such disorders who are more likely to separate or get divorced.

It is no longer clear what the constitution of Professor Munro's 1966 depressed series was. Though he claims to have excluded all cases of neurotic depression, he states in his paper, 'Originally an attempt was made to distinguish between endogenous and neurotic depression, but since all cases were severe enough to warrant hospital admission this really only differentiated severe from moderately severe depression . . . terms such as 'endogenous', 'psychotic', 'reactive' and 'neurotic' depression are too vague to be satisfactory.' How then did he identify cases of neurotic depression to exclude them, and how is this compatible with his current assertion that 'most psychiatrists agree that there is a clinical entity which is usually known as endogenous depression'? It would be important to know what proportion of these hospitalized depressives was eliminated.

In my own series all hospitalized depressives were included but were divided according to the severity of depressive symptoms. Though the term is difficult to define, I would imagine that neurotic depressives accounted for quite a high proportion of the moderately depressed group. As patients needed to be practically psychotic to be included in the smaller, severely depressed group, it is probable that all of these would be categorized as cases of endogenous depression. The incidence of early bereavement was significantly higher in the severely depressed compared with the moderately depressed group. A similar finding was reported by Beck, Sethi and Tuthill (1963). There is a complete refutation of the claims by Munro and Griffiths (1969) that 'the nearer the diagnosis is to "endogenous" or "manic depressive" depression the less important is parental deprivation in the actual aetiology'.

My point regarding groups of mixed diagnosis is this: one can make no special claim for an association between endogenous depression and early bereavement unless one has compared such cases with other depressives, appropriately matched (approximately was a printer's error), depressives with non depressives and psychiatric patients with the general population. Mental illness is not a meaningless term, and there are good reasons why psychiatric patients as a whole may have experienced more bereavement than other people. Hill and Price (1967) point out that 'apart from any effect in causing illness, it would be natural for an orphan to seek help from a psychiatrist where another patient, equally distressed, might turn to a parent'. Or, as the work of Barry, Barry and Lindemann (1965) suggests, orphaned individuals may grow up seeking help from all and sundry and tend to develop clinging, dependent personalities.

The objections I have raised are not so much semantic as methodological. Psychiatric research is still largely a jungle. Progress is slow, but will only be made by mapping one's course meticulously and proceeding by careful steps.

JOHN BIRTCHNELL.

MRC Clinical Psychiatric Research Unit,
Graylingwell Hospital, Chichester, Sussex

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DEAR SIR,

All these statistical articles about parental deprivation and mental illness are doubtless very interesting (and possibly even meaningful) to statisticians. To those of us who are more used to dealing with people than figures they give the impression of emanating from the same synthetic country (in this case, Cloud-cuckoo-land might be the most appropriate name for it) as do those curious census reports that indicate that the average family consists of 2·2 adults and 2·6 children, and has 1·7 pets. All these surveys seem to be based on the convenient if fallacious belief that all families are nice neat little nuclear units each bombinating in an emotional vacuum. This may be the pattern for the future, but it is not true yet, thank God—as I discovered by bitter practical experience in my early days as a psychiatric S.H.O., when, having spent a large part of the interview taking a nice orthodox family and social history, I would subsequently discover from more general discussion that in fact the key figure of childhood was often not a parent or sibling at all, but someone whose blood-relationship was more distant or non-existent. I should have thought it was perfectly obvious that a child is much more likely to be disturbed by the death of a much-loved grandparent (or other relative, particularly if living with the family) than by that of a father whom they scarcely know, e.g. from service or other work abroad, or a mother who is out at work all day. In fact, the bereavement need not be a human one at all: if we are really honest, many of us would admit to feeling far more grief over the loss of a pet than for the death of one whom convention should oblige us to regard as our nearest and dearest. Perhaps it is because this is so obvious that it seems to be overlooked unanimously—but to my mind, the failure to consider these points makes all these studies virtually invalid.

PAULA H. GOSLING.

31 Arlington Road,
Eastbourne,
Sussex

PSYCHOTHERAPY WITH FAILURES OF PSCHYOANALYSIS

DEAR SIR,

In the February issue of your Journal (pp. 195–200), Melitta Schmideberg complains about the intensive analyses given by psycho-analysts to patients who may not benefit from this type of treatment. She also states that analysts do not use other forms of psychotherapy. Mrs. Schmideberg admits resigning from the International and from the British Psycho-Analytic Society some time ago. This must have happened about 20 years ago, for it is very obvious that she has no knowledge of developments during that time.

What she says applies to the years between the two wars when a small number of trained psycho-analysts had to meet all demands for treatment and confined their work to the method they had learned to master. A lay member like Mrs. Schmideberg's mother, Mrs. Melanie Klein, would not wish to apply other psycho-therapeutic approaches, nor were lay analysts trained to apply them at that time.

During the last 20 years the number of psycho-analysts employed in the Health Service has steadily increased, and at present the majority of medical analysts and analytically trained psychologists are working part-time in hospitals, in out-patient and in child-guidance clinics. They are skilled in choosing the method of treatment most likely to benefit a specific case. Besides, it is well-known that full analysis is not easily available.

When Dr. Edward Glover published his book in 1952, it was true that little research had been carried out at that time. This is no longer the case, and analysts, myself included, would carefully assess the suitability for analysis of each patient seen for diagnostic purposes.

What Melitta Schmideberg overlooks is the fact that there are neurotic as well as physically ill patients who cannot be cured. If they are enabled to work and to keep their place in the social environment this may be a therapeutic achievement as much worthy of effort as keeping a patient crippled by arthritis from becoming bed-ridden.

All new treatments are greeted with exaggerated hopes, as for instance ECT, lobotomy and behaviour therapy. So was psycho-analysis, which has remained the most consistent theory of mental functioning and the best therapy for the psycho-neuroses.

Like Edward Glover, I feel we cannot always set our sights too high and expect perfect cures, but Melitta Schmideberg apportions blame if patients are not cured by analysts and implies that they would have been cured had they come to her in the first place. A moot question.

HILDA C. ABRAHAM.

Paddington Clinic and Day Hospital,
217–221 Harrow Road,
London, W.2

DEAR SIR,

Dr. Abraham's somewhat curiously worded statement 'Mrs. Schmideberg admits resigning from the International and from the British Psycho-Analytic Society some time ago. This must have happened about twenty years ago . . . ' is incorrect. I resigned in 1964, when I returned to London for good after having lived and worked in New York from 1949 to 1961.

I agree with many of Dr. Abraham's points, e.g. that psychonalysis has been oversold, that psycho-analysts should have medical training and be able to use other therapeutic approaches as well, that patients should be carefully diagnosed before they are accepted for treatment, that there are neurotics who cannot be cured, and that partial improvements are also valuable.

However, it has not been substantiated that 'psycho-analysis . . . is the best therapy for the psychoneuroses', and, while it is probably true that 'psycho-analysis has remained the most consistent