likely that any recorded increase may simply be a reflection of the higher percentage of patients receiving regular medication or the increased frequency with which patients are seen. A possible additional hazard is a peak rise in the serum level of the long-acting phenothiazines between the second and sixth day after the injection. In our experience (Johnson and Freeman) there is quite likely to be a cluster of parkinsonian side-effects during this period. A smaller dosage given more frequently solves this problem.

Depressive mood swings in patients on long-acting phenothiazines may be aetiologically associated with the drug, as they may with any phenothiazine (Johnson, 1969; Dally, 1970), but affective changes occur frequently during the course of a schizophrenic illness, independent of any medication.

Suicide in schizophrenia is a complex topic. A schizophrenic illness frequently leads to suicide, and suicidal tendencies among admissions have been recorded as high as 20% (Slater and Roth, 1969). In a ten year follow-up (Markowe, 1967) the incidence was found to be approximately fifty times the rate for the normal population. It must be emphasized that the five cases of suicide reported by Alarcon and Carney (1969) have no statistical significance, since they were collected anecdotally and the number at risk from which they were drawn is unknown.

In Salford it has been our routine clinical practice to prescribe anti-parkinsonian drugs to all patients, and this probably explains our relatively low incidence of side-effects. In general no difficulty has been experienced in persuading patients to persevere with this type of oral medication. Although it would seem possible that such patients would not take their anti-parkinsonian medication where necessary, Simpson (1967) and Lowther (1967) have shown that patients on fluphenazine enanthate are motivated to taking anti-parkinsonian drugs even though previously they were unreliable at taking oral phenothiazines. A syndrome closely resembling a depressive illness has been observed in association with parkinsonian side-effects (Boardman, 1961), and it has been suggested (Ayd, 1966) that these symptoms subside promptly with administration of anti-parkinsonian drugs.

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[We have received other letters on this subject on the same lines. We have also been informed that Dr. West's original letter was identical with one published in the *Journal of the Royal College of General Practitioners* in November 1970, and that replies to her letter have since appeared in that Journal—Eds.]

MECHANISM AND MEANING

Dear Sir,

Professor Hill's article 'On the Contribution of Psychoanalysis to Psychiatry' (*Journal*, December 1970, pp. 609–15) centres on the distinction between the words mechanism and meaning. It is claimed that the methods of physical and biological sciences are concerned with questions of mechanism, whereas the psychoanalytic method is concerned with questions of meaning. I would like to show how this distinction, as he defines it, is fundamentally misleading, and in the end unlikely to be helpful.

First we must find out what the author means by 'meaning'. I can do no better than quote him; (1) 'The answers which scientific activity provides are always questions as to how things occur and not answers to questions why they occur. The latter are questions peculiar to human experience and are of a *different order of abstraction* (my italics). The first is concerned with mechanism, the second with meaning.' (2) 'Psychiatrists are aware that neither knowledge of how things happen in the body, even in the nervous system, nor the full analysis of the outward forms of behaviour is sufficient for their purposes. We are of course concerned with what happens at the highest level of organization—with psychic experience . . . But having acknowledged this we are confronted with the fact that almost immediately we will be asking questions about why things happen rather than how they happen. This involves the understanding of meaning, a principle of explanation which runs counter to the principles of explanation on which medicine as a science has hitherto been founded . . . a meaning is not a product of causes, but the creation of a subject.'

These two passages make it clear that what Professor Hill is concerned with in his definition of meaning are questions which are outside the scope of scientific explanation, that is philosophy, metaphysics or what you will. So the difference of order

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of 'abstraction' is the difference between the objectivity of science (in so far as that is possible) and the subjectivity of metaphysical speculation. It is here that we are subtly misled, and we must recognize that scientific explanation of how things occur *also* provides an explanation of why things occur in their own context; for example, the kinetics of the reactions between oxygen, carbon dioxide and haemoglobin exist because of the organism's requirements for a mechanism of this kind. We must assume that this is not what is meant, however ('a meaning is *not* a product of causes').

If this interpretation is correct, it shows a logical misunderstanding of psychoanalytical theory. Thus, how is it that a theory which concerns itself with ideas outside the realm of scientific activity can claim to explain in a logically acceptable way the functions of the mind? Surely it is the job of the psychiatrist to seek explanations of this kind. However, we must not be too alarmed, for if we examine the author's one example, it looks as if there is no disagreement: 'Thus a neurotic symptom has immediate cause . . . a more remote antecedent net of causes . . . and a purpose which is dependent on the future. Together they contribute its meaning. . . . If this is so then psychoanalysis should admit to being a causal theory in the teleological rather than the mechanistic sense.' But there *is* no essential difference between teleological as compared with mechanistic approaches—they are both underpinned by ideas of causation.

Finally, although now wary of the claims of the author, one is puzzled by the statement at the beginning of the second quotation. How is it possible to make an assertion like this when we have only just *begun* to make a full description of the nervous system and the outward forms of behaviour? When a full analysis is *achieved*, only then will we be in a position to decide whether these approaches are enough. They are in fact essential approaches, and, as our increasing knowledge of the function of monoamine neurone mechanisms has shown, they have considerable potential and have had much success. On the other hand, it is very hard to see how the author's kind of 'meaning' is to advance psychiatry, or be of relevance to psychoanalytic procedures.

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THE REPORTING OF RECENT STRESS IN THE LIVES OF PSYCHIATRIC PATIENTS

DEAR SIR,

In their article in the December 1970 *Journal* (pp. 635–43), Dr. Hudgens and his colleagues conclude that retrospective accounts of life events are likely to be invalid. They state that 'for results to be convincing, studies of the interrelationships of life events and illness should determine the rates of occurrence of stress, the temporal relationships between stress and the onset and fluctuations of illness, the emotional impact of specified events, and differences between patients and appropriate controls'.

They cite our own work on this topic (Brown and Birley, 1968), but fail to note or discuss the methodological features of the work which are highly pertinent to the problems they raise and which suggest totally different conclusions. They do not mention that we stated that there was good agreement between patients' and relatives' accounts when seen by different interviewers; that there was no essential change in our results if only one or other account was taken; and that we did interview a 'control group' (we prefer to call this a 'comparison group') of 325 people in their places of work and got good agreement between two interviewers on rates of events. Subsequently (Brown *et al.*, 1971) we have interviewed depressed patients and their relatives and have confirmed that agreement between accounts of relatives and subjects is high (79% agreement for events over a 12 month period). In fact, we now feel justified in interviewing only one informant: interviewing the patient gave 90 per cent of all events reported.

There are important differences in methodology between ourselves and Dr. Hudgens and his colleagues. We have been concerned primarily with temporal relationships, and therefore we have confined our attention, in the first instance, to events and onsets that can definitely be dated, and unlike the St. Louis group to the period prior to onset and not that prior to admission, which may occur some time after onset. Since there are several important sources of bias stemming from both the respondent and the investigator, we have found it essential to define in detail, *before* our main interviewing began, both the class of the event and the persons to be covered in questioning. To do this we have only been concerned with certain types of events occurring to certain types of people—the patient, his household and close relatives. The information itself has been collected by a two-step interviewing procedure. The possibility of an event is first established by routinely going through a standard list of questions about possible events: after this the investigator is allowed unlimited time and questions to establish just what occurred and the dating of the event.

This approach naturally excludes potentially important events, but we believe that by this design

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