are unexplained affective outbursts. There is no doubt that mentally handicapped patients require a standard of accommodation that provides a good and suitable permanent home. Nevertheless it would appear that the physical environment alone may not play a dominant part in determining behaviour. Number and attitudes of staff, 'esprit de corps', personal relationships and psychiatric treatment may well be of much greater significance.

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SEX OFFENDER THERAPY

DEAR SIR,

There are comparatively few therapeutic settings in which it is possible to conduct small group psychotherapy with sex offenders (of either sex) and members of the opposite sex. Most penal establishments are run on a one-sex basis. The literature on various forms of psychotherapeutic approaches in treating the sex offender is copious, *except* with reference to the small mixed group.

At this hospital we have been conducting such groups for the past five years and I would like to invite interested colleagues to communicate with me. Within the field of offender therapy there are relatively few openings for mixed small group psychotherapy. To be able to observe and monitor the behaviour of the man with a history of multiple rapes, in addition to the progressive disclosure of his inner world phenomena, in a small group where half the members are girls can provide vital clinical information. Such groups therefore provide the opportunity for enhancing a diagnostic dynamic formulation of the patient's psychopathology at the same time as furnishing the matrix for the sequential phases of the therapeutic process itself.

I am keen to collate data and pool the experience of those working in this field and would be grateful if they would kindly write to me at this hospital.

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PSYCHIATRY: MEANING AND PURPOSE: AN ANSWER TO DR BEBBINGTON

DEAR SIR,

Dr Bebbington's paper (*Journal*, March 1977, **130**, pp 222–8) raises a number of issues having different importance. The first and less important is whether the arguments he raises against those who make the distinction between causal explanation and meaningful understanding in examining the theory of psychoanalysis can bear the weight that he puts upon them. The contention is that physical reality can be observed and explained in terms of causal connections, whereas psychic reality can only be 'understood' by meaningful connections. In themselves these arguments are academic in the pejorative sense, and they can be answered fairly easily by saying that he has misunderstood the authors he has criticized, and indeed be accused of misquoting them. Whether this is correct must be an individual opinion, only reached by those sufficiently interested to read the works of those he criticizes. To me the philosophical hardware which Dr Bebbington throws at us, if I have not misunderstood him, has a soft impact. I do not wish to try to answer him blow for blow. He is, however, very concerned that psychiatrists should be scientists, because he thinks reasonably enough that this will influence what they do in the clinical situation. His main conclusion is that he would like to see the principle of Popperian refutability applied to psychoanalytical theory even though this 'would involve major change in the form of the theory'. But surely the 'theory', as put forward by Freud, has been subject to change and revision again and again, and who can tell now what the main tenets of psychoanalytic theory are? To quote Seeley (1967):

> 'The words were hardly cool on Freud's lips, the ink hardly dry on his pen, before "revisionism"—or as he looked upon it apostasy—set in, even in Europe. Unlike Christ, eleven-twelfths of whose disciples remained formally firm in the faith, Freud lived to see proportions almost reversed—Jung, Adler, Ferenczi, Reik, Rank and Stekel to mention only the most eminent.'

In the US there were the revisions of Sullivan, Horney and Fromm, and in the UK that of Klein. In Europe existential theory has flourished. But there

has been overall a marked shift of emphasis away from biological determinism towards cultural or social determinism; away from the pre-eminent position of the Id to that of the Ego, and with this, as Seeley (1967) emphasizes, a preoccupation with 'the self' as the main concern, and not with the fate of the instincts and the 'squabbles of the psychic Trinity'.

It is whether or not psychiatry (as opposed to psychoanalysis) is or should be concerned with 'the self', with individual psychic reality of patients, that is the important and implied question in Dr Bebbington's paper. The answer to this question, if it could be definitively given, could shape the future of psychiatry fundamentally. If the answer were in the negative, specialist psychiatry, as Eysenck has suggested, could well disappear. If the answer were positive, the provisions for the specialty in the DHSS blueprint for the consultant's role and responsibility are so inadequate as to be ridiculous, and their acceptance and attempt at implementation, despite some protest at the time, will in the future be seen to have been an act of betrayal. What are the grounds for these assertions? The second is self-evident; the first must be explained.

What are the basic data of clinical psychiatry? By this question is meant what are the data required by the clinician which will give him the best information from which to help his patient maximally not only in the short but also in the long term. Few will disagree with Dr Bebbington that 'the first element is one of naming the phenomena', which is syndrome diagnosis, assigning the disorder to one of the categories of the ICD. It is an exercise which for the majority (perhaps 80 per cent) of the patients met by the experienced psychiatrist does not present too great a difficulty within the first fifteen minutes of the interview. Dr Bebbington says this is the 'equivalent of basic data', and that it promotes our understanding. If this were all, the prescription of the appropriate medication would make psychiatry a very simple specialty (indeed hardly a specialty at all), with perhaps only 20 per cent of patients requiring further investigation, usually of a medical kind. Moreover, there would certainly be a case for the routine use of objective screening procedures such as the PSE (Wing), and we could dispense with the less structured and even unreliable mental state examination. But it obviously is not all; most psychiatrists would like an hour to see a new patient, and some would like several interviews. What are they trying to do? On the one hand they are concerned with the developmental history, which if accurate and corroborated gives an idea of the patient's personality, and this too is basic data, and certainly can promote our understanding. Then too they are concerned with social and environmental data, and this too is basic and promotes understanding. Thirdly, they are concerned with the patient as a biological organism and with the possibility of physical pathological processes which may have affected him. Lastly, but some would hold most importantly of all, they are concerned to understand the psychic reality of the patient—what are the elements of the patient's inner experience of himself and of his world, *what is the nature of his personal plight?*

I have suggested (Hill, 1970) following others (e.g. Jaspers, 1963; Rycroft, 1968), that we are concerned here with the meaning of experience, and that this cannot be derived from observation of the patient through the physical senses, but it can be by identification. Dr Bebbington disagrees and says 'our grasp of it (psychic reality) is based upon the interaction of our sense data and *our models of the mind* (my italics). Hence both the outside world and the psychic reality of others are perceived in the self and through our senses.' Dr Bebbington does not suggest which particular model of the mind he can accept as one generally applicable and having utility, but it is difficult to see how the validity of what is concluded could be tested against the actuality of the patient's experience. This would seem to have been lost altogether. A neurophysiological model may throw light on the controversy.

The nature of psychic reality can be looked at in this way. Every individual has available to him information continuously presented from three different sources. There is, first, the information about the external world available through the stimulation of the receptors in the special senses. Secondly, there is available information from the receptors in the body—those conveying information from the vestibular apparatus, the muscles, joints and viscera. Thirdly, there is information available from what has already been stored in the brain. This must of necessity be information concerned with the past. All this information, whatever the source, must, from our knowledge of the nervous system, be in the form of coded messages, the meaning of which, like the Morse code, depends upon the arrangements of its units in time and upon the spatial organization of the nerve fibres involved (Brain, 1966).

But an important point must then be made. While all this information is potentially available at all times, were we to be conscious of it we must be subject to a big buzzing confusion'. We are not and we do not perceive events, either from the external environment from the bodily space or from the internal world of ourselves *until that information enters consciousness.* Only a small amount at any one time does so and is allowed to do so by an act of attention

which inhibits, so we have come to believe, the access of information from other sources irrelevant to that to which we attend. It is evident that there is a continuous interaction between what we allow ourselves to perceive in the external world and what we allow ourselves to perceive from the internal world. *The product at any one moment of time is our psychic reality.* Being a monist, I assume that what is perceived from the internal world is also presented to consciousness in the form of coded neural messages and therefore subject to exclusion from consciousness by synaptic inhibition, just as information from the external world is held to be. But the amount of information potentially available to consciousness from the brain's data banks is enormous, vastly greater than that potentially available from the external world presented through the special senses. The amount of such data from the internal world can only be comprehended in terms of the total meaningful life experiences of the individual—a storage system of incredible complexity and comprehensiveness, since the information in it is subject to continuous rearrangement, reassessment of 'meaning' (i.e. altering the codes). (If we nostalgically attend only to the past, and not at all to the present, we are autistically preoccupied. If we attend to the present and not at all to the past we are alienated from ourselves and have pseudo-personalities.) Some internal information is more often used; some of it is apparently never or rarely used, i.e. presented to consciousness, and we are not aware that we are not aware of it. It is apparent that the availability of the information from the internal world is a variable; some by an effort of recall is so (the Freudian preconscious); some cannot be recalled by effort (the Freudian unconscious). It is a tenet of most psychoanalytical theorists since Freud that the non-availability to consciousness of this part of the information system from the internal world is due to a continuously active inhibitory process (repression), which diminishes under certain pathological and physiological conditions, e.g. psychosis and sleep.

Yet there is also the tenet, that elements of what is unconscious and directly unavailable to consciousness normally affect it in various ways. That this is so was first demonstrated in one of Freud's greatest works, the Psychopathology of Everyday Life. The psychoanalyst, to quote Rycroft (1968), is one who through his training 'knows something of the way in which rejected wishes, thoughts, feelings and memories can translate themselves into symptoms, gestures and dreams, and who knows, as it were, the grammar and syntax of such translations and is therefore in a position to translate them back again into the communal language of consciousness'.

The rearrangement and 'recoding' of information in the internal world must be carried out as a result of learning, i.e. of experience gained through perception of the external world with which there must be continuous interaction. In respect of information which is wholly cognitive we can assume that the recording is carried out by processes involving digital linkages, but in respect of recording information which is affectively laden the process is determined by linkages which are characterized by their 'meaning' alone.

> I express this proposition this way (Hill, 1970):
> 'The causal connections between past sequences of psychic experiences are themselves dependent upon their future significance for the individual, and this is constantly changing. They are related to the ever-changing human environment, what is, what can be anticipated and what is wished for.'

It does not seem unreasonable that just as the psychotic distorts or misinterprets data from the external world, so also he may do so with data presented to him, almost gratuitously as it were, from his memory banks. Psychic reality is therefore different from physical reality. Above all it is timeless, and its elements are not locked to the dimension of time. Everyone's psychic experience is unique. Both may be available to consciousness, but in terms of the quantity of information available only physical reality can be comprehensively surveyed. If perception is defined as information entering consciousness through the sensory receptor apparatus, then only physical reality can be perceived. If the information systems upon which our own psychic reality depends cannot be perceived by ourselves, can another perceive it? I suggest that it cannot be so, but it can be 'understood' and the best way to achieve this is, as it were, to put oneself in the other's (the patient's) 'psychic position'. This is the act of identification which is not observation and must be contrasted with it.

Psychiatrists can learn sensitivity to another's psychic reality, and training can promote this. It is essentially a human attribute capable of development. We cannot achieve it, as Dr Bebbington suggests, by having a 'model of the mind'; which we can apply to others. The psychic experience of another is always unique. Only the privileged and trained can have access to it. Those who can achieve this will, I suspect for the foreseeable future, be much in demand.

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FAMILY AND SOCIAL FACTORS IN THE COURSE OF SCHIZOPHRENIA

DEAR SIR,

In their letter (*Journal*, April 1977, **130**, p 417) in response to an earlier letter from ourselves in which we criticized certain aspects of their work concerning the influence of relatives on relapse in schizophrenia, Dr Leff and Professor Brown have cleared up some of the issues which troubled us.

It is true that in the 1972 paper by Brown *et al*, we had taken first episodes as meaning first admissions, a thing it is easy to do in reading an account of a study which uses 'key admission' as the base for research design and analysis. In fact, no mention is made in the 1972 paper, or in Vaughan and Leff's 1976 paper, as to what proportion of their sample were first admissions. The authors have now pooled their data and analysed first and readmissions separately, and have found that High Expressed Emotion (EE) in relatives towards the patient is predictive of relapse in both cases.

However, the matter is not entirely resolved, because the authors have missed the central point of our criticism, which is that the raters were, in the case of readmissions, probably rating factors (those composing EE) used to predict outcome (relapse in the nine months after discharge) in the knowledge of outcome. This conclusion is based on the fact that in the 1972 study relapse led to readmission in 83 per cent of cases, and the analysis of correlations between the variables showed that previous admissions were highly correlated with relapse, ranking second to high EE. The raters can hardly have been unaware of the pattern of previous admissions.

Thus the authors have established the important fact that their findings concerning EE apply to first admissions, but there must remain a reservation about the ratings of EE for readmissions.

Professor Brown and Dr Leff evidently do not think much of a pencil-and-paper test compared to their painstaking ratings of the components of EE. However, now that we know that high EE predicts relapse in first admissions and, as indicated in our letter (*Journal*, January 1977, p 102), that our self-rating interpersonal perception technique have some power to predict relapse during the nine months after discharge in a sample of 40 first admissions, it seems likely that our test was measuring some of the same factors as contributed to the rating of expressed emotion. In our test, the patients and parents score the test at the same time in the same room, and this generates considerable feeling and emotional commitment in the great majority of cases. In this situation, we have found that both patient and parents will score on paper terms about themselves and each other which it can be difficult to get them to express in words. This probably accounts for our test giving significant results in several realms, and it looks promising in the present context. The advantage of a technique which does not require a trained rater and which can be administered in about 30 minutes, is obvious.

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DEAR SIR,

Dr Scott and his colleagues are fighting a rearguard action against logic. They agree that we have demonstrated that high Expressed Emotion is predictive of relapse *within* the group of patients who have had previous admissions. Yet they maintain that knowledge that the patients were readmissions enabled us to predict outcome and hence biased our assessment of Expressed Emotion. Knowledge that a patient has had previous admissions enables one to predict a worse outcome than in the case of a first admission. But *within* a group of readmitted patients it does not help one to determine who will do well in the subsequent nine months and who will relapse. This is exactly what the measure of Expressed Emotion does enable one to do.

It is worth emphasizing that we built into the design of our studies a precaution against any extraneous factors biasing the assessment of Expressed Emotion. Checks were made on the reliability of the ratings by having another rater assess the taped interviews blindly. In the most recent study, Leff rated a random selection of Vaughan's audio tape recordings without knowing which families they