

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

whatever may be lost in 'completeness' is more than compensated for by its reliability and validity, and that it gives a *minimal* estimate of the importance of life changes and crises in precipitating major psychiatric disorders. Our findings suggest that such events are of considerable importance (Birley and Brown, 1970; Brown and Birley, 1970). We have discussed in detail our reasons for concluding that these events could not have been brought about by the insidious onset of the patient's disorder.

We must leave readers to judge whether this difference is due to different and more precise methodology, as we believe, or merely to a different bias. But we freely confess to a different bias. Like Dr. Hudgens and his colleagues we are biologists, but we believe that the biology of 'biological psychiatrists' is altogether too simple. When we consider the remarkable waywardness of the spirochaete of syphilis, the epileptic discharge, and the schizophrenic gene in 'causing established mental disorders', we feel that it is biologically respectable to investigate the hypothesis that external circumstances, such as an occurrence which may make adaptive demands upon the person, may also contribute to the development of mental disorder, but we would avoid the words 'causing' and 'established' as begging too many biological questions.

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NEUROTIC SYMPTOMS, PERSONALITY AND PERSONAL CONSTRUCTS

DEAR SIR,

May I comment on Dr. D. J. Smail's stimulating paper, 'Neurotic Symptoms, Personality and Personal Constructs' in your December issue (pp. 645-8)? It

is important because it adds fresh substance to our knowledge that personality factors influence the form in which emotional disturbances are presented by people in trouble, and alerts us to the way (therapeutic or anti-therapeutic) in which they are responded to by the people they come to for help. There are individuals whose avoidance of awareness of the inner subjective realities of themselves and others is so great that objectification is used as a substitute and defence rather than as a way of validating and modifying them. Their feelings and imagination are pushed away as unmanageable threats. Some become patients, some become professional helpers, including psychiatrists. Both groups pose practical problems, and Dr. Smail does a great service in pointing to them, and showing us one sophisticated way of studying them.

With regard to patients, it might not be out of place to mention the relevance of his work to psychosomatic disorders, partly in the hope that he or others will extend it into this field. A short time ago, in a study of some eighty patients with eczema (Brown, 1967; 1970), I found a similar dichotomy between two-thirds who were fairly obviously emotionally disturbed (almost always *before* the onset of the rash) and admitted spontaneously or on questioning to newly emerging psychological symptoms, and one-third who denied such symptoms. The two groups were validated clinically and with psychological tests, and I used the terms Unstable and Superstable to denote them. The Superstable patients appeared not to be more psychologically normal than the others, but abnormal in a different way; in fact in some ways they were more abnormal, and it was the Unstable group that resembled control groups in the balance of psychological and physical complaints. The Superstable group approximate to Smail's neurotic group who tend to produce somatic symptoms and in being relative 'thinking extraverts', and to the similar group described by Foulds (1965) in being unaware of aggression and tending to blame others.

From our data it seems likely that the undoubted personality factors interact with social factors too. The Superstable group showed a borderline tendency to be male and in the lower social classes, and it seems likely that in addition to the individual's defence structure, social pressures encourage somatic rather than psychological/symptom formation. A reduction in the need to maintain self-esteem in the face of such pressures, as the lower class men get older, perhaps explains the finding of a reversal in the eczema patients (but not dental and psychiatric groups) of the expected negative correlation between age and evidence of emotional instability and symptomatology (on the Eysenck Personality In-