

(7) The severity of impairment of psychomotor performance in mania is a reflection of the severity of the condition and thus is hardly surprising. This severity supports no theory other than that most psychiatrists would rather manage a depressed patient than a manic patient. Similarly one cannot assume that leukaemia is a more severe form of hypochromic anaemia, just because it is a more severe blood disorder.

(8) The relative frequency of mania and depression is a reflection of the expression of manic-depressive psychosis. A bipolar model does not demand equal distribution.

(9) The relative infrequency of pure manic states is one again a reflection of the expression of the disease.

Court's model is an oversimplified explanation. It makes for many unanswered questions. Is a mild hypomanic state more severe than a depressive stupor? Why don't most manic states respond to antidepressant drugs?, etc. As shown in the above answers, I am not in agreement with Court's predictions which he states are necessary for a bipolar model. However, I also feel that the bipolar model does not explain all the paradoxes that occur in manic-depressive psychosis. I myself would suggest that a bi-axial bipolar model could better explain these 'paradoxes'. In this model it is suggested that there is a primary disturbance of mood along a depressive-euphoric axis and a primary disturbance of motility along a retardation-hypermotility axis. Disturbances could occur along either axis, in different directions at the same time. Thus we see manic stupor, agitated depression, hypomania etc. Support for this idea is indirectly given by Mayer-Gross *et al.* (1969) who consider involutional depression as 'depressive affect and manic hypermotility'. The bi-axial bipolar model does not explain all the paradoxes of manic-depressive psychosis, but is a model which I feel is worth further consideration.

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#### REFERENCES

- COURT, J. H. (1972). *Brit. J. Psychiat.*, **120**, 133-41.  
 KLEIST, K. (1942). *Nervenarzt*, **16**, 1.  
 LEONHARD, K. (1959). In: *Aufteilung der Endogenen Psychosen*. Berlin.  
 MAYER-GROSS, W., SLATER, E., and ROTH, M. (1969). *Clinical Psychiatry*. London.  
 NEELE, E. (1949). Die phasischen Psychosen nach ihrem Erscheinungs und Erbbild.  
 WHYBROW, P. C., and MENDELS, J. (1969). *Amer. J. Psychiat.*, **125**: ii, 1491-1500.

#### DO MENTAL EVENTS EXIST?

DEAR SIR,

May I add a further response to the article by J. J. Ray (*Journal*, February 1972, pp. 129-32), who is to be congratulated on the ingenuity of his imagination but must be criticized for his conclusions. Watson was justifiably discredited for his denial of the existence of mental events. He was, for example, unable to account for his consciousness of the non-existence of consciousness. Ray also denies the existence of mental events, but for different reasons.

His physicalistic thesis would seem to be that because every so-called mental event may have a physio-chemical counterpart it follows that mental events are identical or, as Ray puts it, 'completely interchangeable' with their physical correlates. But if they were identical the connection would not need to be established by an experiment, it would be established by logic and nothing more (1). If Ray insists that mental events are to be translated into the class of physical statements he leaves us without any way of communicating in ordinary everyday language about meanings, values, purposes and the like. In doing so he fails to accept phenomena as they are but rather dictates what they shall be. His use of the meaning of words becomes arbitrary and monopolistic.

When, for example, I say to a friend about someone else 'he came to know something', I am not ordinarily saying, as Ray states, that the other person 'had an orienting and perceptual response to a particular event that caused structural alterations in the brain'. This is not to deny that his statement can express one meaning of the phrase, but it is difficult to see why we are not allowed to have other meanings.

In contradistinction, Ray writes of his man wired up to an oscilloscope looking at a series of objects shaded blue, and noting as he looks at his oscilloscope the one brain event going on which always coincides with his seeing blue, and which never occurs without his seeing blue. He considers that all people except some philosophers would agree that this man is right and his statement accurate when he says, 'Now I know what the perception of blue is made up of.' It could be contended, however, that a more accurate statement would be if the man said, 'Now I can see and to some extent know what goes on electrophysically in my brain when I perceive blue.' To claim what Ray says is right is to limit the use of the word 'know' to nothing but representations of physical events in the brain. His thesis also, of course, reduces the personal category of the 'I' who does the seeing and knowing to a similar representation.

It might help if he did some revision on N. Hartmann's hierarchical model of the structure of know-

ledge, which has been so well expounded and elaborated by Sir Peter Medawar. Knowledge here is seen as a building with a series of floors, one higher than the other. On the ground floor are the basic sciences. As we go up we come to biological, sociological, anthropological, and psychological levels, to name a few. Each level has its own laws, categories, language and new ideas which are not explicable in the language or conceptual resources of the level below. Biology cannot for example be interpreted in terms of chemistry. Higher levels of knowledge of the nature of man cannot be 'reduced' to the terms of lower level phenomena.

If Ray were to accept this model, he would respect each level for its own insights and interpretations and avoid a tendency to physicalistic imperialism in his use of such crucial words as 'know' and 'real'. His 'persuasive definitions', as C. L. Stevenson (2) would call them, suppress other insights into what can be known as mental events.

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1. AYER, A. J. (1956). *The Problem of Knowledge*, Chapter 5, Section V. Pelican Original.
2. STEVENSON, C. L. (1938). 'Persuasive definitions', *Mind*, *xlvii*, July, 331-50.

#### FREUD AND PHILOSOPHY

DEAR SIR,

The version of psychoanalysis advocated by Paul Ricoeur in his book *Freud and Philosophy*, reviewed in your April number, pp. 455-7, is, if the reviewer's account is accurate, a version Freud would hardly have recognized. Ricoeur, we are told, holds that psychoanalysis should not be regarded as an observational science, that its whole matter is endopsychic and makes no contact with the external world, and that its theory is to be accepted as an integrated whole.

Admittedly, there are analysts who have despaired of developing psychoanalysis as an observational science and have taken refuge in this solipsistic version. Freud held other views. Time and again he emphasizes the very tentative status of his theories and recognizes that scientific theories, like other living things, are born, live, and die. He writes: '. . . a science erected on empirical interpretation . . . will gladly content itself with nebulous, scarcely imaginable basic concepts which it hopes (either) to apprehend more clearly in the course of its development or . . . to replace by others. For these ideas are not the foundation of science (which) is observation alone . . . but the top of the whole structure and

they can be replaced and discarded without damaging it ('On Narcissism', 1914)'.

In his *Autobiographical Study* (1925) he speaks in the same vein, referring blithely to the 'speculative superstructure of psycho-analysis, any portion of which can be abandoned or changed without loss or regret the moment its inadequacy has been proved.

In asserting that analysts have 'skirted the "agonizing revision" that is called for', your reviewer shows himself out of touch with research on the relation of personality development to family interaction, a field in which psychoanalysts have played and still play a leading part and which promises to reshape psychiatry as well as psychoanalysis.

Psychoanalysis is a theory of personality development. Because all development is the resultant of genome interacting with environment, it is necessary for psychoanalysis to pay as much attention to environment as to developing personality. In the past this has been an area of weakness, but it need not remain so. There are many analysts working, with others, to make this deficiency good, and some who are also attempting to reformulate theory in a form in keeping with modern biology and better suited to an observational science. One such attempt is the writer's *Attachment and Loss*, a three volume work now nearly two-thirds complete. If this fails, others may do better.

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#### REFERENCES

- FREUD, S. Standard edition, vol. 14, p. 77.  
— Standard Edition, vol. 20, p. 32.

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