Brit. J. Psychiat. (1970), 116, 569–75

Correspondence

FEARS AND PHOBIAS

DEAR SIR,

In his review of Isaac Marks' *Fears and Phobias* Dr. Aitken raises an interesting point when he says (with approval) that the author 'makes it clear that any explanation must account for all the facts'. Thus, we are told, is he able 'to castigate for their *naïveté* both the classical psychoanalytic and the behaviourist models'. Dr. Marks (or should it be Dr. Aitken?) must be congratulated on discovering an entirely new law in the logic of scientific methodology; one moreover, which at one stroke would rule out of court practically all the scientific laws, theories and generalizations ever proposed. If indeed any explanation must account for all the facts then poor old Newton could never have proposed his theory of universal gravitation; didn't he fail, in spite of his most anguished endeavour, to make his law account for such a simple thing as the movements of the moon? And did not the French physicists continue to point out phenomena clearly not explained by his laws? It is perhaps fortunate that he and other working scientists were ignorant of the Marks/Aitken rule, as otherwise, their *naïveté* shattered, they would have refused to commit their theories to paper.

It is of course quite customary in science, and indeed universal, to propose theories which cover some of the phenomena, in the hope that eventually, after much research and with many modifications, they may cover all; such hopes are usually asymptotic, but they are the lifeblood of science. This is precisely what the behaviourist model is doing at the present time; to call it naïve for not encompassing every known fact (and alleged or imaginary facts as well) is simply to put it on a par with Newton's, Einstein's or any other scientist's theories. It does differ in one essential respect from the psychoanalytic theory in that it is clearly falsifiable. In so far as specific predictions are falsified, the theory will have to be changed; this too, is not unusual in science. May I suggest that the Aitken proposal for only accepting theories which account for all the facts is a defence mechanism useful for retaining theories which account for none of the facts.

H. J. EYSENCK.

Department of Psychology,
Institute of Psychiatry,
De Crespigny Park,
London, S.E.5

TRYPTOPHAN PYRROLASE—A BIOCHEMICAL FACTOR IN DEPRESSIVE ILLNESS?

DEAR SIR,

Dr. Curzon's interesting paper (4) provides a succinct review of the evidence incriminating tryptophan pyrrolase in depression and the part played by adrenocortical hormones. With much of his views I am in agreement, but I must take issue with him on three things.

1. Dr. Curzon suggests that it is tactically 'more reasonable' to study 5HT rather than tryptamine in depressive illness. I contend that exactly the reverse is true. Dr. Curzon first cites the well-established presence of 5HT in brain, whereas tryptamine detection requires administration of tryptophan and/or monoamine oxidase inhibitors (MAOI). To infer from this that the presence of tryptamine is 'abnormal' entails the belief that the tryptophan decarboxylase disappears in the absence of MAOI and is present only after such drugs have been given; strange chemistry indeed, and I am sure not seriously entertained by an experienced biochemist like Dr. Curzon; in fact existing data suggest that if anything MAOI inhibit decarboxylases. If tryptamine is still thought 'suspect', the only alternative is to suppose that tryptophan is not normally present in the brain as a substrate. This can be refuted also, for ten years ago Price and West (13) not only demonstrated the presence of tryptophan throughout the brain stem but also pointed out with much perspicacity that its concentration did not follow that of 5HT; in the pons the ratio of tryptophan to 5HT was at least ten times that found in other regions. It must be recognized that the administrations of tryptophan and/or MAOI are no more than convenient devices which compensate for the insensitivity of detection techniques for tryptamine, and such devices have been used to aid detection of other amines in the past for exactly the same reasons. Recently, Björklund *et al.* (1) have devised a highly sensitive and specific technique for tryptamine and have already demonstrated its presence in the pituitary. Tryptamine, then, is no more an artefact than 5HT, and this being so neither the susceptibility to current measuring techniques which 5HT possesses nor quantitative differences hold relevance for functional significance. To study 5HT on these grounds is not 'tactically more reasonable', just technically much easier.

'Reasons' must therefore be sought in the other