

experiments, parapsychology will by then be quite beyond their reach. New centres for parapsychological research are being developed. Recent experiments in parapsychology have provided results which should carry the subject considerably ahead.

6. Parapsychologists remain aware of the limitations of their work. They do not claim that their results compel belief in ESP, only that the results compel attention to the strong possibility of ESP. And they ask for fair examination of the evidence and fair treatment of the investigators who present it. These requests are quite ordinary, and they are readily accorded parapsychologists when they engage in other lines of work such as orthodox psychology or psychiatry. In their roles as parapsychologists they should have similar rights of fair treatment.

IAN STEVENSON.

JOHN BELOFF.

D. J. WEST.

H. J. EYSENCK.

DEAR SIR,

The points raised by Stevenson, Beloff, West and Eysenck, other than value judgements and pronouncements on dogmatism, materialism, and scientific method, are fully answered in my previous letter or in my book (including my supposed 'error' in discussing the layout of rooms in the Social Science Building at Duke, see paras. 2 and 3 of my letter).

In the case of Stepanek, I have recently published further comments (1). Your correspondents will no doubt be given the opportunity to reply to this if they have anything of substance to say, and they will be free to produce, or conceal, facts as they see fit.

Your correspondents state that my proposed methods by which cheating might have occurred are 'extremely implausible when all the facts are considered'. Such a statement is easy to make, but can it be sustained? Take for example, the Pratt-Woodruff experiment. If your correspondents, after having considered all the facts can, between them, say anything further about my criticism of this experiment, they should communicate with one of the specialist journals in Parapsychology, since it would appear that they can provide information that others have completely overlooked.

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C. E. M. HANSEL.

REFERENCE

1. HANSEL, C. E. M. (1969), 'ESP—Deficiencies of experimental method.' *Nature*, 221, March 22nd, pp. 1171-2.

[This correspondence is now closed. Ed.]

PERSONALITY MEASURES AND THE ALPHA RHYTHM OF THE ELECTROENCEPHALOGRAM

DEAR SIR,

The paper by Drs. Broadhurst and Glass (*Journal*, February 1969, pp. 199-201) on personality and the EEG reports two major findings—(1) Extraversion-introversion is related to the per cent. time alpha, (2) Neuroticism is inversely related to the per cent. time alpha. Much of the paper is devoted to reconciling their findings to other recent work. Yet they point out that this work either contained clear limitations or was concerned with various theoretical speculations, perhaps only marginally relevant to Broadhurst and Glass's study. It seems unfortunate that such skirmishing precludes the authors from noting the main findings of a paper only referred to in passing (McAdam and Orme, *J. ment. Sci.*, October 1954, 100, p. 93).

This latter study reported findings essentially the same as those of Broadhurst and Glass. Utilizing an interview technique, an extravert-introvert personality dichotomy was related to the alpha index. Eysenck's Ranking Rorschach related neuroticism in an inverse manner with alpha index.

My concern, however, is not one of priority of results, as the 1954 paper only supported data reported by others as early as the nineteen-thirties. In fact, this is only one example of where factual evidence for or against such a basic association should, by now, have accumulated to an extent that firm conclusions could be made.

But the fashionable trend is to obscure what workers have actually obtained by a surrounding maze of hypotheses, operational definitions, counter speculations and so on. Bannister complains about 'research into schizophrenia' (Bannister, *Journal*, February 1968, p. 181). Yet it can be argued that the alternative is only 'research into not-quite-schizophrenia' remaining forever in a somewhat idiosyncratic limbo land. It indeed becomes difficult to see the wood for the trees.

There is a real case for the straight reporting of the incidence of various characteristics in the population. Variations in incidence can then be examined for groupings within the population, utilizing the major diagnostic categories and/or personality typologies. Such basic surveys could eventually produce a real accretion of information still not available, perhaps leading to theoretical schemes of real stature. Otherwise, premature hypothesis and counter hypothesis obscure the fact that one is being taken on an essentially circular tour, resulting, as in the present case, in the feeling, 'this is where I came

in'. I am not suggesting that hypotheses, speculation and interpretation should be proscribed, but they might well be relegated to footnotes.

J. E. ORME.

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DEAR SIR,

It is not easy to see the point of Dr. Orme's comment on our paper on the EEG and personality. If we had not referred to his paper with McAdam then there would have been some justification for his drawing attention to its results and indeed for claiming a priority which we do not dispute. But, in fact, in our recent paper we have referred to this earlier work in the field and mentioned its results. It is true that we did not quote from it *in extenso*, nor did we further discuss its relevance to our findings since our paper dealt with normal subjects, whereas his subjects were chronic alcoholics. These are very different populations, and generalizations cannot readily be made between them except as the result of actual investigations of the kind that we have undertaken.

Moreover, as well as using per cent. time alpha, we also measured rate of change of potential (r.c.p.), alpha amplitude and alpha frequency. It is worth pointing out that since 1954 methods of EEG analysis have advanced considerably in accuracy and scope with improvements in electronics, recording and computing techniques. The nature of the EEG is better understood, and such experiments as ours, we hope, will lead to further understanding of its significance. Methods of assessment of personality have also developed beyond the controlled interview technique employed by Dr. Orme at that time.

However, it is when Dr. Orme discusses hypotheses and counter speculations that he becomes harder to follow. Does he really believe that scientists should present data unencumbered by reasons for collecting these data in the first place or interpretation of them once they have been gathered? To do so could be meaningless, confused and haphazard—in a word, useless. We wonder what Popper would make of his suggestion of the relegation of hypotheses to footnotes.

We fully support the plea for large fact-finding surveys, but also look eagerly for further theoretical syntheses, small scale experiments and replications of previous work. By a strictly quantitative investigation we have found in normal subjects an inverse relationship between extraversion and alpha prevalence and amplitude. That McAdam and Orme inferred from their investigation of chronic alcoholics a similar relationship strengthens both cases. It

remains true, however, that contradictory findings have been published, and the complex area of personality correlates of the EEG still required both factual and theoretical clarification.

Finally, Dr. Orme feels that he 'is being taken on an essentially circular tour'. We suggest that his tour has an added dimension and corresponds to the spiral progress of science.

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A. GLASS.

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PARASUICIDE

DEAR SIR,

The problem of nomenclature in studies of so-called 'attempted suicide' has certain affinities with migraine: both are recurrent, are associated with headache, and induce difficulties of focusing clearly.

The only point on which everyone seems to be agreed is that the existing term 'attempted suicide' is highly unsatisfactory, for the excellent reason that the great majority of patients so designated are not in fact attempting suicide. Numerous alternative terms have been proposed; none has found general acceptance. Yet we feel that the problem remains as urgent as ever and requires another effort at solution. The case against misleading nomenclature in psychiatry and the havoc it causes, especially to non-psychiatrists, presumably does not have to be argued.

Of the various proposals, that recently advanced by Professor Kessel (1965) has attracted most interest; he suggests 'deliberate self-poisoning' and 'deliberate self-injury'. This suggestion seems to us to fail for several reasons.

1. The patient may be deliberately self-poisoned, yet outside the group generally regarded as 'attempted suicide', as with a patient on an LSD 'trip' or just plain drunk.

2. The patient may be free of all toxicological evidence of poisoning yet still be within the group of 'attempted suicides', as might arise with a patient on double-blind drug trial who takes a number of placebo tablets with the clear intention of poisoning himself. (This point is important as a reminder that the patients' intention may have to be taken into account in reaching a diagnosis.) To label patients as 'deliberately self-poisoned' who are not poisoned in the generally accepted pharmacological sense would seem to be heading for yet more confusion.

Both 1 and 2 above are in essence pointing to the ambiguity of the word 'poisoning'.