

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