Brit. J. Psychiat. (1969), 115, 743–8

Correspondence

CLASSIFICATION AND GLOSSARY OF MENTAL DISORDERS

DEAR SIR,

The new (1965) Revision of the International Classification of Diseases, Injuries and Causes of Death (I.C.D.) has now been published. Mental disorders are listed in Section V, which contains twenty-six main psychiatric categories, 290 to 315, each of which is subdivided in up to ten subcategories. While the corresponding Section of the previous Revisions was adopted only in a few countries, the revised Section has been accepted internationally, including the United States, where a somewhat different classification had been in use.

The new Revision has met some of the criticisms levelled against the previous one. It is comprehensive and accommodates all types of mental disorders. It has long been recognized that a psychiatric classification cannot fulfil its function as a tool of communication unless the diagnostic terms are used as uniformly as is possible. To ensure this a glossary of operational definitions, i.e. of definitions to be used for the purpose of classification, is essential. Such a glossary has been prepared by the Sub-Committee on Classification of Mental Disorders of the Registrar General's Advisory Committee on Medical Nomenclature and Statistics under the chairmanship of Sir Aubrey Lewis. Here are the particulars: *A Glossary of Mental Disorders. Studies of Medical and Population Subjects No. 22. H.M.S.O., London 1968, price 6s.*

There is reason to believe that the Glossary is not being widely used. It would be a great pity if this valuable aid, which would also be of help to the postgraduate student, were to be ignored. Not only would comparisons of epidemiological data and of statistical observations concerning therapy continue to be almost meaningless, but also the possibility of improving the Classification and the Glossary by testing their usefulness would be missed.

For technical reasons, the changes in the Ministry's Diagnostic Cards which the Revised Classification makes necessary cannot be introduced before 1970. However, it would be of great advantage if psychiatrists were familiar with the Classification and the Glossary by the time it will be in official use.

My Committee has authorized me to appeal to psychiatrists to start using the Glossary, which embodies the new Revision of the Classification of Mental Disorders. The Research and Clinical Section of the R.M.P.A. hope that psychiatrists who have used the Glossary will comment upon it. The Committee will be pleased to receive any correspondence.

PETER SAINSBURY.
Chairman of the Executive Committee of the Research and Clinical Section.

M.R.C. Clinical Psychiatry Research Unit,
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FIFTH INTERNATIONAL CONFERENCE FOR SUICIDE PREVENTION

DEAR SIR,

I should like to draw your readers' attention to the above conference which is to take place at University College, London, from 24th to 27th September 1969. It is organized by the International Association for Suicide Prevention. Previous conferences were held at Vienna, Copenhagen, Basle and Los Angeles. Among the sponsors of the London conferences are the Presidents of the R.M.P.A., the Royal Colleges of Physicians and General Practitioners, the Royal Society of Health, the British Psycho-analytic Society and the World Federation for Mental Health.

The main themes of the conference are 'The present state of suicide prevention—an international survey' and 'Recent advances in antidepressant therapy'. Short papers on all aspects of suicide research and prevention are invited.

Requests for further information are to be addressed to Miss R. S. Addis, O.B.E., Conference Secretary, Fifth International Conference for Suicide Prevention, 43 Queen Anne Street, London, W.1.

E. STENGEL.
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ESP—A SCIENTIFIC EVALUATION

DEAR SIR,

The recent correspondence concerning parapsychology (*Journal*, November 1968, pp. 1471–80) departed from customary discussions of reviews and articles. We were surprised to find Professor Hansel invited to give his comments since we had written in response to your *review* of Professor Hansel's book,

https://doi.org/10.1192/bjp.115.523.743-b Published online by Cambridge University Press

not to the book itself or to Professor Hansel. Since, however, Professor Hansel had an opportunity to enter the discussion, we ask the right to comment on his letter which could well mislead readers further.

Perhaps the point of our letters can best be made by referring to Professor Hansel's complaint that his nine errors in 22 lines (in his description of the Stepanek experiments) were not pointed out in detail. Now our purpose was not to educate Professor Hansel, but to show the innocent reader of his book that Professor Hansel was more careless in details than he claims parapsychologists are. It was necessary to do this since Professor Hansel's numerous errors of fact, as well as his tendency to biased selection of material, were not pointed out in your review. If Professor Hansel so easily accepts the fact that he was wrong in, for example, his description of the layout of rooms in the Social Sciences Building at Duke University, he might accept as also true that he was misinformed about the Stepanek experiments. In that case he had only to read the published reports of the investigations with Stepanek (or address himself to any of the authors of these reports) and he would quickly learn his errors. We begin to suspect, however, that Professor Hansel is not so much interested in actual details of experiments as he is in making general statements about the experiments or about parapsychologists.

One such general statement which we cannot allow to pass is his sentence: 'Having seen the grievous pitfalls into which ESP experimenters can fall when they merely have to count the numbers of hits made by their subjects. . . .' Who has seen these grievous pitfalls? Can Professor Hansel point to any errors (other than such small counting errors as may occur from time to time in any research) which substantially affect the results claimed? No serious critic has ever claimed that the case for ESP can be rejected on the basis of errors in counting hits. Hansel himself in his book accepts the figures given for the experiments he attacks, and admits that the numbers of reported hits are far beyond chance expectations. He aims his main criticism at other points. Yet he is so eager to score points off parapsychologists that he permits himself to direct another gratuitous insult at colleagues in science. This seems quite inappropriate under any circumstances, but especially in one who had himself made so many errors of details.

To turn, however, from the consideration of unimportant details to the substantive issues, these seem to us to be the following:

1. Professor Hansel assumes that the case for ESP rests largely—almost entirely, he seems to say—on the four experiments he criticizes. We deny this, and remind readers that, although parapsychologists generally consider the experiments Professor Hansel criticizes important ones, these do not stand alone even among experiments. And apart from experiments there exists a large body of evidence from spontaneous experiences which strongly suggests (although it does not prove) the existence of such phenomena as telepathy.

2. Professor Hansel alleges that in two of the four experiments the subject cheated and that in the other two one of the investigators cheated. He does not anywhere provide unequivocal evidence proving that cheating actually occurred. He can only say it *might* have occurred. This we cannot deny; but it is most improbable that it did occur. Judgements in science nearly always require assessments of probabilities, not statements of certainties. Professor Hansel's proposed methods whereby cheating might have occurred are extremely implausible when all the facts are considered.

3. In alleging that colleagues cheated during the conduct of scientific experiments, Professor Hansel departs from customary standards of conduct vis-à-vis other persons. We think it fair to ask: would Professor Hansel allege cheating by scientists if confronted with surprising evidence in any other line of scientific work? Almost certainly he would not. If not restrained by the laws of libel, he would at least know that it was exceedingly improbable that his colleagues in other branches of science had cheated, so much so that he would not feel justified in making such allegations in the absence of conclusive evidence that cheating had in fact occurred. If this is correct, we can only conclude that Professor Hansel, and those who join with him, believe somehow that they are justified in resorting to methods of attack against parapsychologists which they would not themselves approve in other contexts. And this must surely be, as you imply in your letter endorsing Professor Hansel's position, that parapsychology is grappling with extremely important issues, issues which arouse emotion in critics as much as in parapsychologists.

4. Following the last point made, it may be helpful if we reassure those who feel that the whole of orthodox science must topple if parapsychologists are correct in their main contentions. Most of present scientific theory will survive the coming vindication of the parapsychologists. Only dogmatic materialists need feel threatened, and dogmatists ought not to be scientists.

5. One of the purposes of our letters was to draw attention to the fact that Professor Hansel is twenty to thirty years behind the times in his attack. Parapsychology has made advances along a number of fronts since the time of the experiments he criticizes. If Professor Hansel or his successors wait twenty to thirty years before criticizing current

https://doi.org/10.1192/bjp.115.523.743-b Published online by Cambridge University Press

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.

C. E. M. HANSEL.
Department of Psychology,
University College of Swansea,
Swansea, Glam.

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

https://doi.org/10.1192/bjp.115.523.743-b Published online by Cambridge University Press