415–18 (see p. 415). I find this omission the more surprising as we also indicated in this article that I was engaged on a large-scale investigation of the dermatoglyphics in mongolism and other types of mental deficiency, with and without psychosis, at that time. This study was published in *Brit. J. med. Psychol.*, 1944, 20, Part 2, pp. 147–60.

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EYSENCK PERSONALITY INVENTORY SCORES OF PATIENTS WITH DEPRESSIVE ILLNESS

DEAR SIR,

Drs. Kendell and DiScipio's article (*Journal*, June 1968, pp. 767–70) rightly draws attention to the popularity of the EPI and its precursor the Maudsley Personality Inventory as a measure of personality. It is unfortunate that their failure to use control groups or to take cognizance of the implications of earlier studies calls into question the validity of their conclusions.

Although the reasons leading to the authors' main conclusion (i.e. that the addition of a sentence to the EPI test instructions largely prevented depressed patients from obtaining spuriously high Neuroticism scores and spuriously low Extraversion scores on testing with the EPI) are not made explicit, they seem to be derived from two observations:

(1) On retesting patients with the EPI using the same variant of the form on each occasion but giving the additional instruction on the second occasion only, a fall in mean Neuroticism scores and a rise in mean Extraversion scores was found (Table I).

(2) On testing depressed patients before and after recovery using different forms of the EPI on test and on retest, but on each occasion giving the additional instructions, no significant changes in Neuroticism or Extraversion scores were noted (Table II).

With regard to the first of these observations there is already a good deal of evidence that, at least in the case of Neuroticism scores obtained using either the long or short forms of the MPI, there is a significant fall on retesting, whether or not there has been any dramatic procedure in the period between test and retest (Bartholomew and Marley, 1959; Levinson and Meyer, 1965; Shaw and Hare, 1965; Coppen and Metcalfe, 1965). Clearly the onus lies with the present authors to demonstrate that the changes in Extraversion and Neuroticism scores as tabulated in their Table I are dependent on the additional instructions to the test directions rather than a simple consequence of retesting.

With regard to the second of these observations, it is clear that the test conditions are so different that no direct comparison can be made with either the authors' Table I or the previous studies we have cited above. There are moreover grounds for anticipating a smaller change in Neuroticism scores and perhaps also in Extraversion scores on comparing Table II with Table I and with the other studies mentioned. Firstly, an unstated number of persons but possibly as many as thirty-two were compared on second and third testing, as opposed to the other studies and the authors' Table I where the relevant comparison was between first and second testing. Secondly, Table I and the other studies have concerned themselves with test and retest on identical forms, whilst Table II compares test and retest on variants of the EPI. An examination of Levinson and Meyer's (1965) and Coppen and Metcalfe's (1965) studies indicates that there is a much smaller change in both Neuroticism and Extraversion scores on comparing second and third testing with first and second testing; and although we are unaware of any other studies comparing mean Neuroticism scores on test and retest using variants of a form rather than replication using the identical form, yet if there is any merit in our earlier suggestion (based on our study of the short form of the MPI (Shaw and Hare, 1965)) that familiarity with the situation results in a lowering of Neuroticism scores, then clearly the use of an alternative form would imply a less familiar situation and therefore a lessened tendency for Neuroticism scores to fall on retesting.

It is possible that the combination of these two factors might account for the differences in the extent of the changes in mean Neuroticism and Extraversion scores between the authors' results tabulated in Table II and the more usual findings as in the authors' Table I and the other papers quoted. Here again a control group would have helped to clarify the situation.

Although it is not relevant to the authors' main theme, and although they have drawn attention to the possible effects of age and sex differences between samples as complicating factors in the assessment of the differences between the depressed and normal groups as tabulated in their Table III, it should perhaps also be pointed out that had there been an excess of persons in the depressed group who had on recovery taken form B of the EPI (and from a reading of their paper it seems possible that this may have been so) this would clearly have had a major effect. Similar considerations, of course, apply to the interpretation of differences in mean scores between the neurotic and psychotic depressed groups. Some reassurance that they were reasonably comparable

in age and sex structure and in terms of the form of the EPI taken would have been desirable.

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DEAR SIR,

Dr. Shaw and Dr. Hare suggest that the changes in N and E scores we obtained in depressed patients were due, not to the additional sentence we added to the test instructions, or, subsequently, to recovery from the illness, but to the fact that people always obtain lower N and higher E scores on retesting.

Their letter conveys the impression that there is clear evidence in the literature that N scores fall and E scores rise when people are given the MPI or EPI a second time, but this is not so. They reproach us for "failure to take cognizance of the implications of earlier studies", yet they themselves, rather curiously, fail to take cognizance of Knowles' (1960) study of the temporal stability of the MPI. Knowles gave this test to a group of ninety-two neurotics and normals on two occasions a year or more apart. Their mean N score was 25·2 on the first occasion and 25·5 on the second; their mean E score 23·1 on the first occasion and 22·5 on the second. The percentage change in N score is +1·2 per cent and that in E score =2·9 per cent. Both changes are trivial but are, of course, in the opposite direction to those required by Shaw and Hare's hypothesis. Of the studies which they quote in support of their hypothesis, Levinson and Meyer's patients were tested before and after a leucotomy, Coppen and Metcalfe's before and after recovery from a severe depressive illness, and many of Bartholomew and Marley's subjects were neurotics who at the time of retesting regarded themselves as "markedly improved as compared with the first testing in hospital". There may be no logical absurdity in attributing the changes in score that took place in these patients simply to increasing test familiarity, but, as in the whisky and water, rum and water, gin and water argument, there is a certain commonsense absurdity. Certainly none of these authors themselves interpreted their results in this way. Moreover, we understand that Coppen and Metcalfe have recently found that when patients become depressed a second time their N scores rise again and their E scores fall again, in spite of their increasing familiarity with the test.

The only work which does suggest that MPI or EPI scores change independently of any change in clinical state is Shaw and Hare's own study of 239 adults, randomly chosen from an electoral register, who were given the short form of the MPI on two occasions a few weeks apart. In this group the mean E score was almost identical on retest (7·22 compared with 7·19) and the mean N score fell from 5·39 to 4·79. Two comments seem pertinent to these findings. In the first place Dr. Hare and Dr. Shaw seem to be confusing statistical significance with practical importance. Their fall in N score is statistically significant because their sample is large, but it could be produced by less than one patient in three answering Yes to one more question. The changes in N and E score we obtained, on the other hand, require, on average, every patient to change his answer to five different questions. Secondly, Dr. Hare and Dr. Shaw's subjects were, as we understand it, tested by someone they had never seen before who arrived at their homes to ask them a lot of questions whose purpose and relevance must have been difficult for them to grasp. It is easy to visualize how such a situation might produce anxiety, and how this anxiety would be less on the second occasion. Our patients were in quite a different situation. They were in hospital, and even before the first administration of the EPI most would already have been required to complete a Cornell Health Questionnaire, a Mill Hill Vocabulary Test and Raven's Matrices, and so would have been only too familiar with the questionnaire situation. In many cases the test administrator was already well known to them, and the two forms of the test were given, not several weeks apart, but always on the same day and usually at the same sitting.

We have listed these considerations because we felt we ought to make a formal reply to Dr. Shaw and Dr. Hare's criticisms. But at the time our most