

methodology. Therefore he condemns many other studies, outside the field of schizophrenia. Most of the specific criticisms, however, are directed at my paper.

The writing of tri-radius in Part I instead of triradius was an inexcusable aberration. When arch was qualified with the adjective simple, this was an attempt at clarification. I might possibly be accused of plagiarism, see Holt (1964) and Penrose and Smith (1966), but plead not guilty to the charge of creating a new nomenclature.

Anyone familiar with dermatoglyphics knows that the contradictions which he proffers are more apparent than real. They are covered by the references to techniques given in the Method. General rules only were outlined to orientate psychiatrists unfamiliar with this subject.

Authorities such as Cummins and Midlo (1961), and Penrose (1968a) state that the tented arch has a triradius.

Mr. David does not adduce any evidence which would sustain the charge of inaccuracy against three of the dermatoglyphic features; frequency of fingerprint patterns, total ridge-count and frequency of patterns in the third palmar inter-digital space. The comments he makes on the fourth feature, the atd angle, are pertinent.

It cannot be denied that the factors mentioned may contribute to the total variance of the atd angle. The major contribution comes from the feature it is designed to measure, the distal displacement of the axial t triradius. Penrose (1954) estimated the error in taking the prints and reading the angles to be about 1 per cent. Mr. David must know that unless palm prints are made with the fingers abducted it is often impossible to delineate all the dermatoglyphic characters.

The actual quantitative effect upon the atd angle of ageing must await longitudinal studies. Since my paper was submitted, Penrose (1968b) has advised giving the age of subjects when reporting on the atd angle. I am grateful for the opportunity to comply with this convention.

Category	Male		Female	
	No.	Mean age years	No.	Mean age years
Catatonic	17	26.8 ± 1.31	24	26.0 ± 0.99
Hebephrenic	97	22.5 ± 0.53	69	22.7 ± 0.64
Paranoid	63	34.8 ± 0.81	96	42.2 ± 0.67
Total	177	29.5 ± 0.47	189	33.0 ± 0.53

In my opinion the increase in the mean atd angle of the catatonic schizophrenics is most probably due to the distal displacement of the t triradius.

It is imprudent of Mr. David to state as fact that Raphael and Raphael (1962) published photographs of healed injuries as evidence of ridge dissociation. (We should now call this dysplasia, Penrose, 1968a.) I share his opinion that this is a rare phenomenon.

The findings in my paper do not admit of any firm conclusions. They do however provide a possible explanation for the diverse dermatoglyphic findings in schizophrenia. They also point to areas of future research. One of these, the possibility that the total finger ridge-count is a pleiotropic effect of genes, which also determine certain anthropometric characters, is at present under investigation.

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REFERENCES

- CUMMINS, H., and MIDLO, C. (1961). *Finger Prints, Palms and Soles*. New York: Dover Publications, p. 64.
- HOLT, S. B. (1964). "Finger-print patterns in mongolism." *Ann. hum. Genet.*, 27, 279.
- PENROSE, L. S. (1954). "The distal triradius t on the hands of parents and sibs of mongolian imbeciles." *Ann. hum. Genet., Lond.*, 19, 10-38.
- (1968a). "Memorandum on dermatoglyphic nomenclature." *Birth Defects Original Article Series*, Vol. IV, No. 3.
- (1968b). "Medical significance of finger-prints and related phenomena." *Brit. med. J.*, ii, 321-5.
- and SMITH, G. F. (1966). *Down's Anomaly*. London: J. & A. Churchill, p. 57.
- RAPHAEL, T., and RAPHAEL, L. G. (1962). "Fingerprints in schizophrenia." *J. Amer. med. Ass.*, 180, 215-19.

DEAR SIR,

Dr. Mellor's paper, "Dermatoglyphics in schizophrenia", is of particular interest to me, as I did some research on this subject many years ago. There is, however, no reference to my findings in Dr. Mellor's article.

I have described the characteristics of the papillary ridges in schizophrenia and mental deficiency with and without mongolism in my book *The Hand in Psychological Diagnosis* (Methuen, 1951), chapters 6 and 7. Though Dr. Mellor mentions H. R. Rollin's paper "Personality in mongolism with special reference to the incidence of catatonic psychosis", he omits to refer to an investigation on the same patients which I did in collaboration with Dr. Rollin, "The hands of mongolian imbeciles in relation to their three personality groups", *J. ment. Sci.*, 1942, 88,

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