Brit. J. Psychiat. (1969), 115, 251–6

Correspondence

DERMATOGLYPHICS IN SCHIZOPHRENIA

DEAR SIR,

It is unfortunate that the study of dermatoglyphics is confined to so few members of the medical profession, and that fingerprints and allied phenomena are not yet widely accepted as being of great interest in a variety of conditions. One such condition is schizophrenia, and certainly any work that might provide more precise information about the genetical aspects of this disease would be most welcome. Unfortunately, the paper by C. S. Mellor (*Journal*, November 1968, p. 1387), leaves a very great deal to be desired.

For example, early on, the author states that: "Tented arches, included with simple arches, have more acutely curved dermal ridges and a tri-radius in the centre of the pattern." Firstly, "simple" arches, as he calls them, are universally known as "plain" arches. Why does the author introduce a new nomenclature here? Secondly, he persistently refers to a triradius as a "tri-radius". Bisecting this word is quite novel, but the author does not justify this innovation in preference to the term which until this paper has been quite acceptable. Thirdly, he includes tented arches with "simple" arches, which is reasonable, says they have a "tri-radius", which is highly controversial, and then in Part II of the paper says "The ridge-count for an arch is 0, because the arch has no triradius" which is contradictory. By this stage of his paper the reader will note he now refers to "triradius". Perhaps the author has grown tired of his new word? Perhaps it means something quite different? The latter quotation is in itself incorrect, since not only do tented arches, included with "simple" arches, have a triradius (according to the author), but also there are many loops in which there is a single recurving ridge, a triradius, but a zero ridge count (Penrose, 1968).

In Part II of the paper, the author describes his results measuring the atd angle. So much information has been omitted from this section that his atd angle results, as published, are quite valueless. It is well known that the atd angle is in fact a measure of a large number of variables. These include pressure used in taking the palm print (Penrose, 1954), breadth of the palm, age of the subject, lateral displacement of the triradius t, and the degree of adduction or abduction of the fingers when the palm print is taken. The latter may make a difference of as much as 8 degrees in two prints taken of the same person with the fingers maximally and minimally abducted. This is not to mention the one variable that the atd is designed to measure, namely, the degree of distal displacement of the palmar axial triradius t. The author has only partially excluded one such variable, age, in his paper. The significance of the rest of the variables are left entirely to the reader's imagination. Since it is theoretically possible for the cumulative maximal effect of all these variables to be of greater magnitude than any ordinary atd angle itself, it is very hard to see how any useful conclusions can be drawn from the published results.

The paper states that "The aim of the present investigation was to find an explanation for the widely different findings of previous investigators". Instead of doing this, the present paper has unfortunately only added to the confusion. One may at least be thankful that the author did not go as far astray as to publish photographs of fingerprints of schizophrenics showing "ridge dissociation", an incredibly rare phenomenon, which in fact were perfect examples of injuries which had healed by granulation, most probably burns (Raphael and Raphael, 1962).

One must conclude that, until the dermatoglyphics of schizophrenia are studied accurately, no conclusions can be drawn about the significance, if there is any, of the findings published to date on this extremely important subject.

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RAPHAEL, T., and RAPHAEL, L. G. (1962). "Fingerprints in Schizophrenia." *J. Amer. med. Ass.*, **180**, 215–19.

DEAR SIR,

The import of T. J. David's letter is that most papers on dermatoglyphics in schizophrenia do not warrant serious consideration. It is not the psychiatric concepts which he has questioned, but the dermatoglyphic

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 (1968*a*) 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 finger-print 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 (1968*b*) has advised giving the age of subjects when reporting on the atd angle. I am grateful for the opportunity to comply with this convention.

	Male		Female	
Category	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, 1968*a*.) 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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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**,