**Reviewer’s report**

**Title:** Directional and fluctuating asymmetry in finger and a-b ridge counts in psychosis: a case-control study

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**Reviewer:** Eadbhard O'Callaghan

**Level of interest:** A paper whose findings are important to those with closely related research interests

**Advice on publication:** Accept after discretionary revisions

This is an interesting study of dermatoglyphics in psychosis. It's main finding is that while directional asymmetry in cerebral morphology is reduced in schizophrenia, this is not reflected in the specific dermatoglyphic variables that these investigators chose to examine. As this is, broadly speaking, a study with 'negative results' it is important to know if the study had sufficient power to detect a difference between the two groups if such a difference truly existed. Regrettably, no power calculation is provided.

**Background**

In paragraph four, the authors note that several groups have reported reduced a-b ridge counts in schizophrenia, but do not acknowledge or reference the similar number of (albeit older) studies that failed to find such a difference.

In the final paragraph of the background, the authors note that different measures of FA have been used in the past. It would be helpful and timely if they revised their next sentence to indicate which methods they used.

**Methods**

The diagnostic assessment is presented in a convoluted manner. Based on the information in the methods section is sounds like a convoluted process, involving the Psychosis Screen (itself derived from the CIDI and the Psychosis Screening Questionnaire), the DIP (a modified version of the SCAN) and OPCRIT. It is not clear what happened when the latter two instruments disagreed: were patients re-assessed or simply omitted?

**Results**

In the second paragraph of the results section, they indicate that persons with schizoaffective disorder
were included in the affective disorder group. It could be argued that this ‘affective disorder’ group comprises quite a heterogenous group of patients. This is particularly relevant in light of the twin paper by Cardno et al (AJP, 2002) indicating that the environmental component in the aetiology of schizoaffective disorder is not shared by bipolar disorder or schizophrenia (unlike the genetic components, which overlap). It therefore follows that in studies of environmental factors in psychosis (such as this study), schizoaffective disorder should either be analysed as a separate entity or else omitted.

Discussion
The authors discuss the finding that males with non-affective psychosis had more FA in finger ridge count on the index finger compared to controls and affective psychosis. Regrettably, they do not discuss what this may mean developmentally - is there any possible biological rationale for this? In the absence of a biological rationale, the finding is very weak: if they had corrected for multiple tests it would no longer be significant. Indeed, given the p-value of 0.04, it is likely that if the schizoaffective patients were omitted or included in the schizophrenia group (as is often the case, eg. in Rosa et al, 2001), then the finding would almost certainly have become non-significant. While the authors acknowledge that they did not correct for multiple tests, they should emphasize the limitations of this finding more clearly. In addition, while they do suggest that the inconsistencies in the literature may relate to different methodologies used to measure FA, they should also mention that the literature as a whole may reflect the true situation, ie. that no consistent differences exist.

In the final lines of the discussion, the authors state that this study weakens the case that early developmental disruptions impact equally on the brain and the ectodermal features of the finger and palm. They should qualify this by stating that their evidence is based only on analysis of selected measures (a-b ridge count, asymmetry, etc).

**Competing interests:**
None declared.