Brit. J. Psychiat. (1965), **111**, 1225–1228

Correspondence

"MALVARIA"

DEAR SIR,

A new chemical syndrome called "malvaria" has recently been described (1, 2, 3) in which, it is suggested, an unknown chemical present in urine is identified as a purple spot after paper chromatography and spraying with Ehrlich's reagent. According to Hoffer and Mahon (1) this mauve factor is present in most of the urine of schizophrenics, whilst O'Reilly, Ernest and Hughes (3) found a high correlation between it and "thought changes". Such a concept implies that the source of the unknown substance is endogenous and not secondary to some dietary constituent, which possibility appears to have been ignored by both O'Reilly *et al.* and by Hoffer and Osmond.

It is well recognized that many urinary constituents are derived from the diet, others from bacterial action in the gut and subsequent absorption (4). Von Studnitz, Engelman and Sjoerdsma (5) showed that most of the urinary phenolic acids excreted by the normal subject were derived from exogenous sources. Commenting on the possibility of urinary chemical abnormalities in schizophrenia, Baldessarini and Snyder (6) stress that such abnormalities have not been confirmed in studies on subjects on a plant free diet.

Ehrlich's aldehyde reagent, used to identify the mauve factor, is not specific for any class of compound (7). Moreover, the chemical technique used by Hoffer (2), whose method O'Reilly *et al.* quote verbatim, is so incomprehensible as not to permit repetition.

However, by analogy with the work of Von Studnitz *et al.*, and of Perry on schizophrenia (8), it is clear that in research of this nature a rigid dietary control is essential. In its absence, the mauve factor might well be simply an indole whose increased excretion is secondary to a decreased faecal output (constipation, decreased food intake or dehydration). This hypothesis could account for O'Reilly *et al.*'s finding for instance, that 50 per cent. of their cancer patients, many of whom were receiving morphine-like drugs, were "positive"; or were they too experiencing "thought changes"?

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SLEEP PATTERNS AND REACTIVE AND ENDOGENOUS DEPRESSIONS

DEAR SIR,

In his letter (*Journal*, September 1965, page 905) Costello maintains that "clinical tradition shaped the collecting of the case history data". The data used in the paper by Kiloh and myself (1963) were not case history data; each patient was interviewed by either Professor Kiloh or Dr. Ball (Kiloh, Ball and Garside, 1962) and they used "an interview technique which was reasonably well standardized" (Kiloh, 1965). Costello, presumably, must have misunderstood his communication from Kiloh (1963).

It is true, of course, that the clinical opinions of investigators may affect the data they collect, but it is very difficult to avoid entirely the possibility of this kind of bias. Even so-called objective data have to be scored, recorded, manipulated and interpreted by someone. Our data were collected in a careful and systematic manner, and the subsequent analysis indicated a remarkably close fit between diagnosis and second factor loadings, even though the factor analysis included 51 additional doubtful cases. To

superimpose this kind of result upon the case material would have necessitated superhuman arithmetical powers, which (if they will forgive me) I am sure Professor Kiloh and Dr. Ball do not possess.

I do not intend to answer Costello's statistical arguments point by point. Whatever he may say, the fact remains that Costello and Selby (1965, page 499) state "The findings presented here confirm the original findings (Costello and Smith, 1963) suggesting no difference between reactive and endogenous depressives in their sleep patterns." This, it seems to me, is clearly a claim to have confirmed the null hypothesis, which (as I pointed out in my previous letter (*Journal*, August 1965, page 773) their data do not and cannot do.

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DEAR SIR,

Dr. Costello's detailed reply (*Journal*, September 1965, page 905) calls for some comment. My concern with his criticism (1) of the paper by Kiloh and Garside (2) arose from the suggestion that their results were influenced by bias in the recording of data. Since those who take the trouble to do the kind of work under discussion (as we are doing at present in Newcastle) are likely to start with the premise that qualitatively different kinds of depression do exist, the notion that preconceptions can invalidate results must be examined with care.

Actually, Costello's statement "our intent was to compare sleep pattern data obtained from case histories with sleep pattern data obtained in standardized interviews" (although based apparently on a misunderstanding) seems to imply that provided standardized interviews are used, data can be collected without bias. I would agree. Certainly there can be no question of the necessity to define terms and to standardize methods of eliciting and recording information as exactly as possible. Indeed the great advantage of the method by which the presence or absence of individual features are recorded, and their intercorrelations subsequently calculated, is precisely that contamination of data and uncertainties about diagnostic procedure are reduced to a minimum. Since this was in fact the method of Kiloh and Garside, their results in regard to sleep pattern must carry more weight than those of Costello and Selby, whose diagnostic groups were constructed by the old-fashioned clinical method in which, as they point out, the data are liable to contamination. For, unfortunately, we remain in total ignorance of how their independent interviewer arrived at his diagnoses, and what importance he gave to sleep patterns among the other features. On the other hand Kiloh and Garside's data show the factor loadings, on their bipolar factor, of both initial insomnia and early wakening, as reported by patients in standardized interviews. Naturally, the *actual* amount of sleep achieved by patients is a different question.

It is worth adding that while Kiloh and Garside's data showed a close fit between clinical diagnosis and the factor loadings, this is less important than the demonstration that a bipolar factor does exist. Further study of this factor may well lead to modification of current ideas about the classification of depressions.

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INTELLIGENCE OF PATIENTS IN SUBNORMALITY HOSPITALS

DEAR SIR,

The letters from Drs. Bavin, Shapiro and Walk (*Journal*, June and September 1965) raise three main issues.

1. *The distinction between legal and clinical classification*

Dr. Bavin urges that the terms Subnormal and Severely Subnormal should be limited strictly to the classification of patients dealt with under the Act and should not be used for the planning of clinical services; Dr. Shapiro refers to the dangers of equating legal terminology with clinical classification. What they advocate may well be desirable, but we must also ask whether it is reconcilable with current practice. It was concern with current practice that led us to conduct our survey; although not uninterested in official intentions, we were chiefly concerned with actual usage in the implementation of the new Act—and it must be evident that principles may not