the "Severely Subnormal", deprived of adequate facilities for investigation, treatment and research. This retrograde separation of a section of the retarded population from the mainstream of optimistic, forward-looking activity of the large, comprehensive Mental Deficiency Hospitals is a direct result of assuming that there is some fundamental difference in therapeutic opportunity between the two grades of patient. The danger, which the authors point out, that a patient might be denied appropriate treatment and training because he had been misclassified and sent to the wrong hospital is best prevented by having comprehensive hospitals with no dichotomy. Even maximal discrimination between categories will always result in some error—with personal tragedy for the unfortunate individual. The traditional unified hospital service under one clinical team denies facilities to none, and produces the greatest ease of transfer and flexibility in the training programme. The Ministry itself is confused, for the arguments it gives in favour of District General Hospitals are the exact opposite of those advanced for the fragmentation of Mental Deficiency Hospitals.

In short, Heber's ceiling for intellectual deficit at −1 S.D. is more realistic than that of Castell and Mittler; legal terminology should not be used for clinical practice or planning clinical services; nomenclature should be precisely used after definition for a specific objective if the inherent technical difficulties in Mental Deficiency are not to be compounded and confounded by semantic promiscuity.

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Reference

Heber, R. (1960). "The concept of mental retardation: definition and classification." *Proc. London Conf. Scien. Study Ment. Def.*, 1, 236–242.

SLEEP PATTERNS AND REACTIVE AND ENDOGENOUS DEPRESSIONS

Dear Sir,

In their interesting paper (*Journal*, June 1965, pp. 497–501) Costello and Selby criticize the findings of Kiloh and Garside (1) on the grounds that they "may simply reflect the knowledge [i.e. of clinical tradition] and need to arrive at a diagnosis of the clinicians producing the case histories", but do not say how their own "independent interviewer" approached the problem of differential diagnosis.

If their interviewer employed a relatively simple, single criterion, such as the presence or absence of an environmental precipitant, then it is not surprising that Kiloh and Garside's findings were not fully borne out. For these authors did not use any single criterion, but diagnosed their cases on the basis of the feature-pattern as a whole (a common procedure in psychiatry). Their subsequent statistical analysis showed that the clinical differentiation of the two syndromes arrived at by this means was not arbitrary or intuitive, but in fact corresponded with the mathematical composition of the matrix of intercorrelated items. "Precipitation" was only one item among many, and its correlation with diagnosis fell well short of unity (0·654).

If, on the other hand, Costello and Selby's interviewer himself took account of a number of features, then we need to know about *his* attitude to traditional views, and in particular, what importance, if any, he attached to the sleep pattern? Also, to what extent may he have been influenced by knowledge of the investigation being carried out on his patients? All these factors could have affected the final groupings. Indeed, if Costello and Selby are right and clinicians' observations are too fallible to lead to reliable diagnoses, then it seems doubtful if their own study justifies any conclusions about the sleep patterns in so-called reactive and endogenous depression.

Actually, one of the purposes of Kiloh and Garside's study was to put diagnosis in depression on a surer footing by studying the frequency and inter-relationships of individual symptoms. As they point out, the clinical diagnosis, although made in every case, was doubtful in 51 out of 143, presumably because the feature-patterns were not sufficiently clear-cut for a confident clinical judgment; it does not seem, therefore, that much "reinforcing desired responses" from the patient actually took place. Nevertheless, all cases were included, and their analysis showed that the data must be due to two separate factors, interpreted as a general illness factor and a bipolar factor corresponding to neurotic versus endogenous depression. Costello and Selby, it may be noted, omitted 32 of their 73 cases for reasons that are not stated.

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Reference

1. Kiloh, L. G., and Garside, R. F. (1963). *Brit. J. Psychiat.*, 109, 451.

Dear Sir,

I wish to make a number of points in relation to the letters of Drs. Kay (above) and Garside (*Journal*, August 1965, p. 773):