

and pathological groups, this is only broadly true, exceptions occurring frequently enough to make the equation invalid. For example, cases of phenylketonuria have I.Q.s that cluster round the 30's yet extend into the 70's and 80's, and the range of intelligence of patients with chromosome anomalies stretches from almost zero to normal. Conditions resulting from the complex interaction of many factors cannot be completely assessed on a linear scale with respect to only one aetiological variable. Broad statistical tendencies can do no more than afford general guidance in dealing with individual cases.

It would obviously be desirable to delimit subnormality of intelligence. It must be remembered, however, that intelligence in the general population varies continuously. Subnormality, dullness and low normal intelligence are rough approximations delimiting areas along this continuum. Any sharp limits to these categories must be purely arbitrary and artificial. It is permissible for experts to make authoritative pronouncements of the reliability of tests, their validity and statistical characteristics. Once this is done, they are in no better position than laymen to say whether 1, 1.4, 2 or 3 S.D. should be used to delimit different categories of patients. That is, unless they can show that a certain I.Q. level has a very high correlation with effective social adaptation. This has not yet been done; all that S.D. measurements do is select a certain percentage of the general population. From the point of view of classification and records, particularly if these records are going to be used for research, it would be much more satisfactory if the I.Q. of the patient were given, and this information is available at present in the Ministry records.

If this psychometric information were combined with a clinical diagnosis, scientific work would not have to depend on legal classification designed with a different goal in view, and there would be much less confusion than at present.

The intention of those responsible for the Mental Health Act was, as Dr. Walk has clearly pointed out in his letter, that the legal provisions should apply to people selected according to broad criteria concerned with social adaptation. To impose arbitrary limits based on one facet of personality would not only contravene both the letter and the spirit of the Act, it would rigidly separate patients whose clinical needs might be similar and thus deny treatment to some.

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DEAR SIR,

The issues raised by Castell and Mittler (*Journal*, March 1965, pp. 219-225) are of great importance. Unfortunately the authors add to the already alarming amount of confusion existing over diagnosis, classification, and Mental Health Act interpretation in mental deficiency, in addition to encouraging planning based on legal definitions and false prognostic assumptions.

I remain firmly convinced that the terms Subnormality and Severe Subnormality should be strictly limited to the classification of patients dealt with under the Act. The use of these legal terms in a clinical situation immediately gives them at least two meanings. It further ties clinical practice to legal terminology—a situation which can surely have no supporters.

In that the Act mentions subnormality of intelligence without defining it, the consensus of professional opinion should clearly be the guide as to what constitutes this, and the authors are right to reiterate the need for agreement on the upper limit of this *clinical* condition. However, they seem to have overlooked the fact that if, as they state, other clinical and social criteria are important in defining the categories, then the upper limit of the intellectual parameter should be high enough to ensure that *no* patient who might be properly considered subnormal taking into account *all* criteria, would be excluded by this single numerical limit. Thus my view is in complete agreement with that of Heber (1960) who proposes a cut-off point at -1 S.D., which allows, as he points out, flexibility for diagnosis in borderline cases.

The authors appear to have misread the definition of Severe Subnormality. In addition to the points mentioned by Dr. Walk (June 1965, p. 547), the phrase "which includes *subnormality* of intelligence" is used. Thus there is no question of this category being limited by a separate, lower, ceiling.

The authors' discussion reveals a clinical attitude which should be eschewed. Methodologically it is unsound to associate a pessimistic prognosis with a diagnostic category based on behavioural performance, and then to demand revision of the diagnosis when the response to treatment shows the prognosis to have been incorrect. "Severe Subnormality" should be applied where the present behaviour satisfies the legal definitions. No such idea as "poor response to training" should be associated with it, encouraging an attitude of inevitable pessimism and therapeutic nihilism. More correctly, poor training produces little response. The tyranny of words is so powerful that the Ministry of Health is already building and planning separate small hospitals for

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 inter-correlated 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):