

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

be the same as practice. If, then, we look at practices, we find that the Act's new classifications are indeed being used for clinical and administrative purposes, whether we like it or not. How else is it possible to account for the finding that 97·2 per cent. of the 964 patients surveyed had in fact been classified under one of the Act's categories (Table II)? Another example of the administrative use of these categories was our quotation from the Hospital Plan, but we might equally—perhaps more aptly—have quoted from the Ministry of Health's own guidance on the new terminology: referring to the four categories, the Ministry's memorandum explains—

“The Act itself distinguishes these groups only in connection with the powers of compulsory detention in hospital or guardianship in the community. The terms are, however, likely to come into general use in the administration and planning of psychiatric hospital services and local authority mental health services.” (Estab. 3039/25, January 1960.)

In the light of this general usage, it seemed to us worthwhile to examine current practices in the interpretation of those aspects of the new terminology most relevant to the work of psychologists. Although it may be possible partially to evaluate a piece of legislation by referring to the supposed intentions of its framers—as Dr. Walk does—or by saying how one believes it ought to be used—as do Dr. Bavin and Dr. Shapiro, surely another, at least equally important, criterion must be that of actual usage?

2. *Definition of subnormality of intelligence*

It is unfortunate that Dr. Shapiro should have misread our argument sufficiently to believe us to be advocating low intelligence as the *sole* criterion of mental defect. Nowhere in our paper do we suggest this; we make it quite clear that other criteria are also important in defining the Mental Health Act's categories. It seems to us that the Act regards subnormality of intelligence as a necessary but not sufficient criterion. Some of Dr. Shapiro's arguments suggest that he has doubts about retaining it as a criterion at all; otherwise, he would not regard as naïve our surprise that the classification of Subnormality is being used in the same way as was that of Feeble-mindedness in the old Acts. We would argue that if the phrase 'subnormality of intelligence' was to have any public, communicable meaning, then a change in usage might have been expected with its introduction; in particular, it might have been expected that the phrase would not be used to describe patients who did *not* show subnormality of intelligence. Our concern is prompted by the finding

that rather over a quarter of the patients surveyed to whom this description had been applied, had I.Q.s over 80. None of our critics comment on this finding.

3. *The borderline between subnormality and severe subnormality of intelligence*

On the question of the borderline for Severe Subnormality, all three correspondents criticize us for suggesting that this classification implies, among other things, very low intelligence. Drs. Bavin and Shapiro remind us that in its definition of Severe Subnormality the Act does not specify a separate, lower intelligence ceiling. Dr. Walk relies on his interpretation of the views of the Royal Commission, thus passing over an important difference between these views and the provisions of the Act—the Act's introduction of the classification of Subnormality. In addition, his excerpts from paragraphs 192-3 of the Commission's Report omit two sentences, one in which reference is made to an I.Q. below 50 or 60 “as being a pointer strongly indicative of a personality so seriously subnormal as to make the patient incapable of living an independent life” (a view which accords well enough with other research findings we refer to), and the other sentence in which the Report says, of the term 'severely subnormal personality', that “it always involves marked limitation of intelligence as well as other personality defects”. Furthermore, at least one of the members of the Commission did not seem to subscribe to Dr. Walk's view: in listing the factors that influenced the Commission to use the term Severe Subnormality, Thomas (1962) refers to “the recognition that within the range of *three* standard deviations as adjudged by standard intelligence tests, virtually the whole segment of mental retardation produced by the lower levels of biological variation would be eliminated from the group”. The very paragraphs from which Dr. Walk quotes, when parts of them were later incorporated in the Ministry's memorandum on the new terminology, gave rise to interpretations quite opposite to his own, expressing concern that “the dividing line set somewhere in the 50-60 range by the Royal Commission seems to be moving to 70 and perhaps even to 75” (MacMahon and Clarke, 1960); to this concern, our findings give some foundation.

Both Dr. Bavin and Dr. Shapiro agree that it is desirable for there to be professional consensus as to what constitutes subnormality of intelligence. Dr. Bavin prefers the A.A.M.D.'s system to our suggestion; we do recognize his point to some extent in our reference to an extreme upper limit of -1.4 S.D. units. His limit seems to us rather high, including as

it does some 16 per cent. of the general population, even when no allowance is made for errors of measurement; in the W.A.I.S., for example, the Standard Error for ages 18-54 years is ± 2.6 I.Q. points. Dr. Shapiro's argument on this point seems to rest on the assumption that we advocate low intelligence as the only criterion of Subnormality and Severe Subnormality—an assumption we have already shown to be wrong. As to his preference for the I.Q. over S.D. unit measurements for research and record purposes, our objection is that I.Q.s on different tests may not be comparable, whereas S.D. measurements are of necessity comparable across tests.

Finally, we would repeat that, whatever the intentions behind the new legislation or the official advice given as to its implementation, *in practice* the variations in usage are large—to us they seem too large, and suggest a need for greater agreement.

An attempt to reach closer agreement on such a matter need not interfere with efforts to seek a more satisfactory and functional classification, as for example Dybwad (1965) advocates.

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