patients classified as "Subnormal" might have to be discharged at the age of 25, although they were unfit (or not yet fit) to live in the community because of their inability to guard against exploitation. The definition of "Severe Subnormality" was therefore altered at this stage in order to include such patients. Here again, no one was thinking of incurability: it was a question of allowing time for further training with a view to independence.

In the conclusion to their letter the authors stress the need for greater agreement on the principles of classification. This, of course, is a matter for those working in this field; but I would suggest that any agreement on the use of legal (as distinct from clinical) terms must be within the bounds of what is stated in the law.

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DEAR SIR,

Drs. Castell and Mittler (*Journal*, December 1965) probably do not receive in their departments of psychology the official directives of the Ministry of Health. If they did, they might qualify their statement that "the Act's new classifications are indeed being used for clinical and administrative purposes".

The Ministry, which spawned 'mental subnormality', speaks with several voices. It is true that I occasionally receive from it communications addressed to me as "Medical Superintendent of a hospital for the subnormal and severely subnormal", the Ministry forgetting on these occasions that I might have a few psychopaths as well. The Statistics Branch of the Ministry ask for details of patients not only as "subnormal" or "severely subnormal", but also classified according to the type of "mental retardation".

The Architects' Department of the same Ministry has, however, its own views (Hospital Building Note No. 30), and must be congratulated on producing a classification unlike any other and probably unique. It is:

1. Severely subnormal, low-grade
2. Severely subnormal, medium-grade
3. Subnormal, low-grade
4. Subnormal, high-grade

To those who speak the English language all this may be sensible, unambiguous and crystal-clear. Foreigners to whom it is explained regard it as madness. As Dr. Bavin (*Journal*, June and September) and I (*Brit. med. J.*, 30 January, 1964) have suggested, could not the hideous and inaccurate terms "subnormal" and "severely subnormal" be reserved for those few patients who are legally detained? We might then get a little way out of the bog.

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CRYPTOMNESIA AND PLAGIARISM

DEAR SIR,

In his most interesting and valuable paper on "Cryptomnesia and Plagiarism" (*Journal*, November 1965, p. 1111), Dr. F. Kräupl Taylor mentions two points which, although peripheral to his main theme, are of sufficient general interest to justify further comment.

Firstly, he says that the term "cryptomnesia", in its use to denote the emergence of hidden memories in trance states, has fallen into such disrepute that it should now be restricted to "the appearance in normal consciousness of memories which are not recognized as such subjectively". It was, however, spiritualistic interpretations of trance phenomena which fell into disrepute, rather than the phenomena themselves. Also, hidden memories which emerge in trance states are just as "cryptomnesic" as those which emerge in normal consciousness—whatever the dictionaries may say. The proposed new use of the term would appear, therefore, to be too restrictive.

Secondly, Dr. Taylor asserts that "more sober" students of cryptomnesic phenomena "discount" the belief that a trance medium can reproduce the memories of dead people. Confidence in discounting this belief is based, however, not on factual evidence which disproves it, but on confidence in the conceptual framework of currently orthodox psychological theory—which excludes its credibility on *a priori* grounds. Moreover, if telepathic phenomena exist, this disputed ability of trance mediums would be an obvious possibility, requiring no spiritualistic hypothesis. Indeed, some students of the recently-published Cummins-Tennant automatic scripts, and of Professor C. D. Broad's searching commentary on them (Toksvig, 1965), may understandably conclude that there is weighty evidence to support it. Really sober students will hesitate, no doubt, to accept this belief as having been conclusively established, but they will also, surely, be sufficiently sceptical of speculative theory to refuse to "discount" it.

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https://doi.org/10.1192/bjp.112.483.210-a Published online by Cambridge University Press

REFERENCE

TOKSVIG, S. (Ed.) (1965). *Swan on a Black Sea: A Study in Automatic Writing*. London: Routledge.

BEHAVIOUR THERAPY

DEAR SIR,

A clinician even vaguely familiar with the literature and practice of "behaviour therapy" cannot help but be dismayed at an article like that of Marks and Gelder in your July 1965 issue, "A Controlled Retrospective Study of Behaviour Therapy in Phobic Patients". Although one must be impressed by the care exercised in matching treatment and control patients in terms of deviant behaviour, age, and so forth, there is absolutely no control in terms of actual treatment. To be specific, on p. 564 the authors point out that the behaviour therapy patients often received as wide a variety of ministrations as relaxation-hypnosis, systematic desensitization, barbiturates, and, yes, two E.C.T.s and one leucotomy. How can one overlook this utter disregard of the most elementary and basic criteria of experimental design? All the numbers in the world (e.g. duration of treatment, outcome of treatment on a five-point scale, etc.) are meaningless as a result.

A further criticism is in the use of the term "behaviour therapy" to refer to Meyer and Gelder's technique of gradual *in vivo* exposure or "practical retraining" as they call it. It is especially puzzling to see this unjustified generalization of the phrase "behaviour therapy" in the same article which, in its first paragraph, points up the widely differing nature of psychotherapeutic techniques which are subsumed, for better or for worse (and, in my opinion, for worse) under the rubric of behaviour therapy or conditioning therapy. I have myself been involved in a treatment programme quite similar, in parts, to Meyer and Gelder's (Lazarus, Davison, and Polefka, 1965); we never considered our successful therapy as any sort of vindication of "behaviour therapy", however.

Let me commend Marks and Gelder for their emphasis on the importance of non-desensitization or non-practical retraining factors in treating even relatively simple neurotic disorders. After controlled experimental studies have established the actual conditioning bases of "behaviour therapy techniques"—and this kind of work has only just begun, references below—we will do well to examine any "non-learning" factors of which, I suspect, most practitioners considering themselves behaviour therapists are keenly aware. Arnold Lazarus of South Africa has stressed these non-specifics for several years now. On the other hand, it seems premature to *assert* that learning principles cannot be found to account for aspects of therapy which go beyond the desensitization couch or the syringe loaded with apomorphine.

Ambitious attempts at evaluating various therapies are surely to be encouraged and reinforced. However, it is misleading to publish articles which are so unsatisfactory on methodological grounds. As a fellow "behaviour therapist", I can only hope that investigators like Drs. Marks and Gelder will be more careful in specifying the referents of their terms.

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REFERENCES

DAVISON, G. C. (1965). "The influence of systematic desensitization, relaxation, and graded exposure to imaginal aversive stimuli on the modification of phobic behavior." Unpublished doctoral dissertation, Stanford University, Stanford, California.

LANG, P. J., LAZOVIK, A. D., and REYNOLDS, D. J. (1966). "Desensitization, suggestibility and pseudotherapy." *J. abnorm. Soc. Psychol.* (in press).

LAZARUS, A. A., DAVISON, G. C., and POLEFKA, D. (1965). "Classical and operant factors in the treatment of a school phobia." *Ibid.*, **70**, 225–229.

DEAR SIR,

Dr. Davison's comment on the design of our study misses the point. It was a retrospective inquiry; we collected all the phobic patients who had received behaviour therapy in this hospital from 1960 to 1963. Since we found that it had been customary clinical practice to use behaviour therapy as part of a wider plan of treatment (which sometimes included drugs and occasionally E.C.T.) we collected a control group, with similar clinical features, who had received a similar amount of drugs and E.C.T. The one patient who had a leucotomy and behaviour therapy was matched by a similar patient who had had a leucotomy but no behaviour therapy. Comparison of the two groups revealed the contribution of behaviour therapy over and above that of the other treatments.

The design undoubtedly shows the effect of an active treatment: for example, it shows up the useful effect of behaviour therapy in less severe phobias, and of modified leucotomy in severe agoraphobia (to be published). We cannot accept, therefore, that our findings result merely from poor design.

Dr. Davison has decided that practical retraining should not be called behaviour therapy. Unfortunately he has not provided his definition of the