Brit. J. Psychiat. (1965), 111, 195–198

Correspondence

RESTRICTED ORBITAL UNDERCUTTING

DEAR SIR,

In their article, "Restricted Orbital Undercutting" (*Brit. J. Psychiat.* (1964), 110, 609–640), Sykes and Tredgold have fallen into an error which is unfortunately still common in psychiatric reporting. They have failed to pay proper attention to spontaneous fluctuations in the psychiatric disability.

The fact that a patient functions well for ten years after a treatment tells a good deal of its efficacy *only* if the patient's overall function for several years before the treatment is also described. Dr. Sykes and Dr. Tredgold have made no effort to do this. Rather the authors define only the psychiatric and occupational status immediately prior to treatment.

The success of a treatment after previous alleged therapeutic failures is significant only if prior treatment is clearly spelled out. The authors confess that their description of prior therapy is not "watertight". They say in Table II that 68 out of 98 allegedly refractory depressives received significant benefit from lobotomy, but on page 610 they point out that 64 out of 91 of the same depressives got significant benefit from their first course of ECT. The reader is not told what proportion of the lobotomy successes occurred in the ECT failures.

I hope that either the above authors or some other British investigator will make their experience with lobotomy (probably the broadest and best followed up in the world) available to international psychiatry in meaningful form. To do this, however, (*a*) they must compare a given number of years of social function before lobotomy to a given number of years after lobotomy; (*b*) they must describe the post-lobotomy response of a group of patients *all* of whom are known and clearly defined treatment failures. In this way the group, if it exists, of patients truly able to benefit from lobotomy can be defined.

GEORGE E. VAILLANT.

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DEAR SIR,

We are grateful to Dr. Vaillant for reading part of our lengthy paper, though Miss Sykes is embarrassed by his gift of a doctorate; but we must refute his flat statement that we made no effort to describe the patients' overall function for some years before the operation. In fact we did so, and if he will look again at pages 610, 613, and 628, he will find some description; while Table III (which, being folded in, is easy to miss) gives the months in hospital in detail; these seem the exact points he suggests in his last paragraph.

We are indeed aware that spontaneous fluctuations occur. The point was that in this series all cases had been recommended operation on the grounds that the responsible clinician had come to believe that no further fluctuation for the better was now in the least likely, socially or clinically. Therefore although the psychiatric and social status had to be defined at the point of time immediately before the operation, this moment was not the *only* time considered. As to E.C.T. and its results, all but 7 of the depressives had had at least one course; many had had more; improvement was never more than temporary. The whole report was some three times the length of the paper published here; Dr. Vaillant would be very welcome to see it; but we did not find any significant correlation between response to any particular course of E.C.T. and progress after operation.

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"HOMOSEXUALITY—A PSYCHOANALYTIC STUDY OF MALE HOMOSEXUALITY"

DEAR SIR,

In a review of Homosexuality—A Psychoanalytic Study of Male Homosexuality published in the September issue of your Journal, Dr. F. Kräupl Taylor expressed doubt as to the reliability of our research, specifically the accuracy of progress reports on the homosexual sample contributed by fifty-eight psychoanalysts, all members of the Society of Medical Psychoanalysts. The findings seemed dubious to him since, according to his thinly veiled sarcasm, "the splendid results" were not consistent with the pessimistic conclusions of the Wolfenden Committee. The inferences one can draw from his critique are that the responding analysts were either naïve or dishonest or that they suppressed homosexual wishes and behaviour in their patients. He has suggested that had the patients been directly interviewed following discharge by one or two independent observers, a quite different statistic on shift from homosexuality to

https://doi.org/10.1192/bjp.111.471.195-a Published online by Cambridge University Press