*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.

*Lexington, Kentucky.*

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.

R. F. Tredgold.

*Old Common, Cross in Hand, Sussex.*

## "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-b Published online by Cambridge University Press

heterosexuality might have emerged, thus implying that a group of patients who were reported as having become exclusively heterosexual probably remained homosexual; hence, the patients lied to their gullible psychoanalysts; or the psychoanalysts may have recognized their failures but reported dishonestly; or the patients rapidly reverted to homosexuality after their separation from the psychoanalyst.

Dr. Taylor seems to be concerned about methodological sophistication, and rightly so; however, his own knowledgeability is open to question or he may have overlooked our chapter on methodology. Now the aim of our research was to uncover as many of the pertinent variables in the aetiology of homosexuality as was possible within the limits of the study. Obviously, the sexual status of homosexuals at the completion of treatment was of interest. The sexual status at termination was reported and *that was all we reported*. We did not claim that the shift was permanent (although in my professional experience it has been). The capacity to alter sexual adaptation during psychoanalysis is clearly of theoretical and clinical importance. Further, according to the rules of scientific criticism, one cannot merely accuse serious researchers of obtaining unreliable results without offering the evidence of replicated work.

It is of especial significance that no American behavioural scientist nor British colleagues with whom the study was discussed questioned the validity of our findings, which I described at the Sixth International Congress of Psychotherapy in London, August, 1964. At the Congress, Dr. Charles Socarides, an American colleague, reported a 50 per cent. shift in his own patient population, while another American delegate, working in a psychiatric hospital, described results similar to our own.

Dr. Taylor has offered us the compliment of having made a brilliant error by comparing our imputed acceptance of patients' retrospective distortions with Freud's classical mistake about the aetiology of hysteria. Again, Dr. Taylor reveals that he has not read our work or has failed to comprehend it. In a chapter on homosexuality among adolescents which describes a study conducted by two members of our research team at Bellevue Psychiatric Hospital in New York City, the parents of adolescent homosexuals were interviewed directly. The same configurations in parent-child relations were noted as were reported by the homosexuals in our sample.

I can only conclude that Dr. Taylor has had poor results in his treatment of male homosexuals; otherwise, he could hardly be so sceptical of analysts who treat homosexuals or of the capacity of homosexuals to change. His views may be more clearly understood in the light of a review of our research which appeared in *The Times Literary Supplement* in August, 1962:

"Both psychiatrists who are ignorant of analytic therapy, and homosexuals themselves, have a vested interest in preserving the idea that homosexuality is a genetic variant—for the former can assure themselves that the psychotherapy that they are not equipped to practise is bound to be ineffective, whereas the latter can console themselves with the belief that their abnormality is neither a reflection on their upbringing nor a failure on their own part to reach emotional maturity."

Finally, we deplore Dr. Taylor's failure to have entertained an alternative hypothesis: i.e. that our findings are reliable. To do so is not only of heuristic value in an area of psychopathology not yet entirely understood, but is also consonant with the spirit of scientific exchange.

IRVING BIEBER.

*New York City.*

DEAR SIR,

Dr. Bieber and his psychoanalytic colleagues reported unusually good therapeutic results with homosexual patients. Nobody else had ever claimed such successes: 19 per cent. of exclusively homosexual patients had become exclusively heterosexual. It seemed to me that the authors would be understandably proud of their achievement and therefore welcome an opportunity to prove the validity of their findings. For that purpose, I suggested another follow-up, but by observers who could contact the patients themselves this time.

There was an obvious flaw in the investigation of the authors. They had relied entirely on information supplied by the patients' psychoanalysts, though they were well aware that this information might be inaccurate. It was subject, as they admitted, "to distortion by the patient, to interpretation by the psychoanalyst, and presented in a brief answer on a questionnaire" (p. 30). However, any doubt about the information was brushed aside with the assurance that the psychoanalysts had been well trained. Yet training and expertise are usually regarded only as indispensable qualifications for a scientific investigator; they are no proof of the validity of his findings.

Dr. Bieber is obviously of a different opinion. He regarded my request for an independent follow-up as casting a slur on the veracity of the patients and the competence of the analysts. In his indignation, he has stooped to arguments *ad hominem*, which have no place in a serious discussion.

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