

*Case history*

J.A., aged 46, was mute, apathetic with a vacant expressionless facies, and often showed impulsive aggressive outbursts. He was unemployable, ate too much and became obese. The case was diagnosed by several psychiatrists as one of catatonic schizophrenia and no special treatment was tried.

In February 1972 his case was reviewed, his diagnosis confirmed and it was decided to treat him by fluphenazine decanoate 12.5 mg. every fourteen days along with orphenadrine 50 mg. t.d.s. and perphenazine 4 mg. t.d.s. For the next three months he did not show any improvement in his clinical state, when his fluphenazine dose was increased to 25 mg. weekly. But within the next two weeks he developed ataxia, and fluphenazine was stopped. His condition deteriorated further, he refused to move even in bed but obeyed simple commands. He showed flexibilitas cerea and refused food and drink. There was excessive salivation and urinary overflow incontinence. The condition was considered to be one of catatonia induced by phenothiazine therapy.

After about three months from cessation of fluphenazine therapy, the patient gradually showed improvement in his condition and started eating solid food. By the next two months the patient was back to his original clinical state namely spending most of his time sitting in a chair, mute, and with an expressionless face. He refused to work, but would eat and dress himself.

Phenothiazines act on the extrapyramidal centres of the midbrain. Catatonic complications of phenothiazine therapy have been described previously. It has been postulated that phenothiazines block the action of noradrenaline (NA) in the midbrain and that there is excessive rise in other catecholamine namely 5-hydroxy-tryptamine (5-HT). In the mid-brain area there is normally a higher level of 5-HT than NA, and the result is the production of extrapyramidal symptoms.

It can be assumed that in catatonic schizophrenia the basic 5-HT level of the brain is raised. Fluphenazine decanoate injection rapidly increased the 5-HT level in the midbrain and thereby aggravated our patient's catatonic symptoms.

It may be concluded that long acting phenothiazines are unsuitable for catatonic schizophrenia.

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#### PARENT-CHILD RELATIONSHIPS AND HOMOSEXUALITY

DEAR SIR,

Your November issue ((1972), 121, 525-8) has a report by Graham Robertson on 'Parent-child relationships and homosexuality'. In it he declares that in our book, *Homosexuality—A Psychoanalytic Study*

*of Male Homosexuals*, my colleagues and I emphasized the mother-son relationship, but, he implies, we did not stress the disturbed father-son relationship as he says Bene did.

Actually, ours was the first study to emphasize and statistically demonstrate the critical importance of the detached and hostile father in the aetiology of male homosexuality. For example: 'We have come to the conclusion that a constructive, supportive, warmly related father *precludes* the possibility of a homosexual son; he acts as a neutralizing, protective agent should the mother make seductive or close-binding attempts' (p. 311). We devoted an entire chapter to the father-son relationship and I would suggest that Robertson should read it. In all my subsequent writings on the subject, I have underscored the central role a disturbed father-son relationship plays in the genesis of male homosexuality.

Robertson also criticizes our study as being based on 'second hand' information. These purported 'second hand' data were given by a group of 77 highly qualified psychoanalysts who answered hundreds of items tapping parent-child relationships in the cases of 106 homosexuals compared with 100 heterosexuals. Each analyst knew his patient's background and history in fine detail. Our volume was published over ten years ago, and since then I have taken careful histories in my psychiatric examinations of more than 800 male homosexuals; this number has included patients representing all socio-economic strata and the major ethnic groups. I have also examined about 30 parent pairs of male homosexuals as well as children and pre-adolescents who were in a high risk population for homosexuality. The findings described in our study were completely supported by my later information.

Robertson, on the other hand, obtained his material from 'the rather limited information available from the case notes' of a group of out-patients of two hospitals. Because of this limitation the author was able to use only 13 items to tap the complexity of parent-child relationships. Clearly, Robertson's methodology is utterly lacking in reliability and rigour, yet he declares that our most carefully researched findings fit 'rather too neatly into a classical dynamic mould, and one suspects a certain degree of bias in the reporting of the analysts'. Further, Robertson falls back on the skewed sample argument: 'It should be recognized that Bieber *et al.* used an abnormal section of the homosexual population for their study, i.e. they relied on reports about only those men who could afford to undergo lengthy psychoanalysis and were thus using an above-average population with regard to educational background.' In 1960 Gordon Westwood reported a study of 127

British male homosexuals, most of whom were working class men and only very few had ever had psychiatric treatment. Those questions that tapped parent-child relationships produced information completely in accord with our own. In 1969, Snortum *et al.* and Evans independently reported on their studies of non-patient male homosexuals. They had no contact with any member of our research team but used those items from our published work that concerned parent-child relationships. The type of patterns each author noted in a heterogeneous non-patient sample matched the descriptions of our own sample.

Shortly following the publication of our book, the London *Times Literary Supplement* of 17 August 1962 had this to say: 'The conclusions reached are of great interest . . . the authors' views are supported by evidence which has been collected in such a way that subjective bias is excluded as far as possible; and until further equally careful studies have either supplemented or disproved this work, it must be allowed to stand.' Robertson's study does not appear to qualify for this role.

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#### MMPI PERFORMANCE IN CHRONIC MEDICAL ILLNESS: THE USE OF COMPUTER-DERIVED INTERPRETATIONS

DEAR SIR,

Goldstein and Reznikoff (2), exploring the adequacy of computer interpretations of the MMPI

performance of chronically ill renal patients, suggest that such a group may erroneously be labelled as hypochondriacs when they are in fact merely describing their medical disorder. This conclusion is based upon their finding that a sample of patients receiving haemodialysis treatment for chronic renal failure had significantly higher mean scores on the hypochondriasis, depression, and hysteria scales than did a group of general medical patients; and on differences on the frequency of the appearance of computer-derived interpretative statements in the protocols of the two groups.

While we do not dispute the potential for the misinterpretation of any psychological test, we do contend their misuse is not a function of whether the particular instrument is scored and interpreted by computers or by men. If the MMPI is used only as a means of 'labelling' patients, it is a misuse. When used to assist the physician in understanding and identifying certain psychological adjustments the patients are making to their life situation, including their physical illness and its treatment, he, not the computer, must accomplish the integration of pertinent data. To do otherwise is a misuse.

We further feel that the data presented by Goldstein and Reznikoff are not sufficiently compelling to conclude that their chronic patients were erroneously labelled. To begin with, the significant elevations on the hypochondriasis, depression and hysteria scales for the haemodialysis group appears to be based upon a comparison with a control group. Although the authors are to be commended for their use of control subjects, it should be noted that the testing of this group was done 'in the convalescent stage'. The haemodialysis group must be considered in a treatment phase. We cannot help but wonder if an MMPI given to the control group immediately prior to their being treated might not have shown more somatic concern and anxiety; if so, the differences between the groups would possibly not have been significant. One might ask if the mean scores of the haemodialysis group on these scales were above

TABLE I  
*Proportion of haemodialysis and general medical patients having statement*

Statement	Per cent		t	Significance
	Haemo-dialysis patients	General medical patients		
Moderately depressed, worrying, preoccupied ..	59	23	2.46	.05
Considerable number of physical complaints, etc. ..	41	14	2.03	.05
Consider psychiatric diagnosis .. .. .	36	27	.65	N.S.
Great number of chronic physical complaints, etc. ..	36	23	.99	N.S.