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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I welcome, however, Dr. Bieber's unexpected announcement that he and his co-authors "did not claim that the shift [to heterosexuality] was permanent". He stresses now that all they reported was the temporary sexual status of the patients at the termination of treatment.

It certainly was not made clear in the book that the therapeutic claim applied only to the period before treatment came to an end, and that there was no follow-up of the patients. In fact, the reader was led to think otherwise. In Table XI–1 (p. 276), for instance, the authors speak of the sexual status of patients as reported "at the *final follow-up inquiry* in June, 1960" (the investigation having been started in June, 1952). When describing Case No. 166 (p. 296), the authors are even more explicit: "*At last follow-up, three years after completion of psychoanalysis*, the patient reported . . . ". (My italics in this paragraph.)

I realize now that the authors studiously avoided using the word "cure", but the way they expressed themselves could easily be misunderstood. Take this statement (p. 318), for example: "Many homosexuals became exclusively heterosexual in psychoanalytic treatment." It will be interpreted as claiming actual cures, as we have all become conditioned to the assertion that psychoanalysts only report therapeutic results which are reliable and lasting.

I am sure the authors will be quoted as having demonstrated that many homosexuals can be cured through psychoanalysis. A follow-up enquiry would therefore seem to be all the more obligatory now to put the record straight. As there may be justifiable objections to independent observers, I suggest that the analysts might get in touch with their former patients to ascertain their present sexual status. Some seem to have done so already. It is only a second-best solution, but it has the advantage of being more easily organized.

F. KRÄUPL TAYLOR.

The Maudsley Hospital, S.E.5.

DISFRANCHISED?

DEAR SIR,

As far as absent voter arrangements for hospital patients were concerned, it appears that H.M.(55)65 was still the authoritative document at the time of the recent General Election. Under this authority a patient in a psychiatric hospital situated within the same parish, borough or urban district as the qualifying home address at which he was on the electoral roll, was not entitled to absent voter facilities if he could not be allowed out without detriment to his own health or the public's interest, to attend a polling station either alone or with friends or relatives.

The same restriction did not apply to the physically ill, and yet it is conceivable that under these provisions an informal patient, for example, with severe phobias of crowds and open spaces, who had been advised to remain in hospital for the time being in the interests of his own health, could thus have been unreasonably disfranchised. There might also have been some detained patients, not allowed out of hospital in the public's interest, who were both capable and desirous of expressing their political opinion, although similarly excluded from doing so.

J. P. CRAWFORD.

Stone House, near Dartford, Kent.

CALCIUM THERAPY IN DRUG-INDUCED PARKINSONISM

DEAR SIR,

In a recent article (Lichtigfeld, 1964) it was proposed that the interference with the action of calcium might be a factor in the production of drug-induced Parkinsonism. Evidence was presented supporting the view that hypo-calcaemic states may interfere with nerve cell function in the basal ganglia. It was concluded that calcium administration may control some of the extra-pyramidal symptoms caused by tranquillizers.

Further to this idea, some work done on post-encephalitic Parkinsonian patients lends weight to the above suggestion. The use of maintenance doses of calcibronat has been shown to reduce the number of post-encephalitic oculogyric crises occurring in these patients (Duensing and Meyer, 1938; Renner, 1941; Brandt and Brandt, 1956). Intravenous injections of calcibronat often aborted the crises and sometimes shortened them (Duensing and Meyer, 1938). Whether the bromide component is absolutely essential to the success of the combination is open to question, as the following case history illustrates. Duensing and Meyer mention a case (No. 10) that did not respond to bromide therapy. Injections of calcium were then given on alternate days for some weeks, and this treatment regime produced two 16-day periods of complete freedom from crises over a three-months period. The attacks resumed their old frequency when calcium medication ceased, but they responded to calcibronat when this therapy was started at a later date. Therefore it is likely that the addition of other sedatives to the calcium would have as good an effect as the bromide.

The site(s) of action of calcium in the brain has been the subject of much work. Apart from sites previously mentioned, it appears that the calcium content of the ventricular fluid may also play an

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