specialty, but who happens to be well acquainted with the history of the Royal Commission and the Mental Health Act. It is much to be hoped that our Mental Deficiency Section will, in due course, formulate and publish its views on this important matter.

A. WALK

## HOMOSEXUALITY—A PSYCHOANALYTIC STUDY OF MALE HOMOSEXUALITY

DEAR SIR,

I wonder what experience Dr. Kräupl Taylor has had of the analysis of homosexuals which permits him to condemn in such outright fashion the work of Dr. Bieber and his colleagues. (*Br. J. Psychiat.* Sept. 1964, p. 744). If he studies the literature, he will find that the experience of a great many psychiatrists accords more with that of Dr. Bieber and his co-workers than with his own views.

Successful cases have been published in the past few years by Hadfield (1), Oversey, Gaylin, and Hendin (2), Ellis (3), Glover (4), and myself (5). Older cases were published by London (6), Naftaly (7), Lilienstein (8), Laforgue (9), Stekel (10), Serog (11), Frey (12), Virchon (13), Bircher (14), Sumbaer (15), Sullivan (16), Poe (17), Karpman (18), and many others.

Oversey, Gaylin and Hendin published three cases treated by analytical psychotherapy in which the patients attained complete heterosexuality, confirmed by observation over some years. Ellis treated 28 male and 12 female patients who were homosexual, with an overall change of 64 per cent. towards heterosexuality; indeed, of the males who had some desire to become normal (23) 80 per cent. became distinctly or considerably more heterosexual. Ellis's terminology may be ambiguous, but there was undoubtedly a marked change. Whitener and Nikelly give an overall prognosis in all types of psychosexual disorder (which must include many homosexuals) of 50 per cent.

I have published a series of cases of homosexuality (19), and out of 23 patients had 16 successes confirmed by follow-up, four cases which showed only social success, inasmuch as they lived asexual lives, and three failures. My successes were confirmed by follow-up. Glover has published a series in which 44 per cent. of the patients showed no further homosexual impulses (but treatment was complicated by hormone therapy). Some 51 per cent. of the bisexuals lost their homosexual impulses.

Since it is well known that cases treated privately do much better than those treated in clinics, I cannot see why Dr. Bieber and his colleagues should not have the successes they claim.

The causal situations which they describe as producing homosexuality are similar to those I have described in my book (19). There I stated that "One may say that it is only those who have never treated a case of homosexuality, or have treated it wrongly, who have never had a cure", and this I still believe to be true after 35 years of treating these patients.

CLIFFORD ALLEN

*Moorcroft House, 39 Whitchurch Road, Chester.*

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DEAR SIR,

I cannot see the logic in Dr. Clifford Allen's argument. Even if his impressively long list of references proved a high proportion of psychotherapeutic cures in homosexuals—and they certainly do not prove anything of the kind—how could this possibly confirm the work of Bieber and his colleagues who do not claim to have achieved what is usually called a therapeutic success. May I refer Dr. Clifford Allen to Dr. Bieber's previous letter

(*Brit. J. Psychiat.*, February 1965, 195) in which he explicitly states: "The sexual state at termination was reported and *that was all we reported*. We did not claim that the shift was permanent" (Dr. Bieber's italics). They did not even attempt to find out how long the shift lasted. Weeks? Months?

Dr. Clifford Allen states quite properly that his "successes were confirmed by follow-up". I therefore presume that he is as interested as I am in establishing the truth, and that he will join me now in requesting Dr. Bieber and his colleagues to round off their investigation by a follow-up study. But will they have the courage to do so? After all, this might bring down to realistic proportions the therapeutic successes they have never claimed, but which are attributed to them so generously.

F. KRÄUPL TAYLOR

*The Maudsley Hospital,*
*Denmark Hill, S.E.5.*

## THE EFFECT OF SODIUM AMYTAL ON AUTONOMIC AND MUSCLE ACTIVITY IN PATIENTS WITH DEPRESSIVE ILLNESS

DEAR SIR,

We are prompted to comment on the recent paper by Martin and Davies, "The Effect of Sodium Amytal on Autonomic and Muscle Activity in Patients with Depressive Illness" (February, 1965, pp. 168–175). In this and in earlier study (1962), the authors reached the unwarranted conclusion that the digit-doubling method of determining sedation threshold is unsatisfactory.

As introduced by us in 1960 the procedure involved combining the digit-doubling task with intravenous sodium amytal administered as a continuous infusion. In their first attempt to repeat the work, Martin and Davies, using instead a discontinuous injection procedure, not surprisingly found end-points of sedation difficult to detect, due to fluctuations in consciousness. Changing, in their second experiment, to a continuous infusion method, the authors rather surprisingly abandoned the digit-doubling technique on the grounds of its previous inefficiency! In fact, the only study to replicate the original procedure exactly (Moffat and Levine, 1964), substantially confirms our own experience over several years in nearly 300 subjects that the technique is a simple, reliable method of determining the sedation threshold.

Surprisingly, too, Martin and Davies do not discuss the peripheral action of sodium amytal which is known (Goodman and Gilman, 1955) to impair transmission through autonomic ganglia and have a direct influence on blood vessels. Such effects may seriously invalidate the use of barbiturates for manipulating "arousal level" as monitored via autonomic indices.

Finally, may we add that it is difficult to evaluate the study adequately in view of the imprecise description of the clinical material and the lack of clarity in presenting the statistical analysis, particularly the correlations between the various physiological measures.

GORDON S. CLARIDGE,
*Lecturer in Clinical Psychology.*
R. N. HERRINGTON,
*Lecturer in Psychiatry.*
*Department of Psychological Medicine,*
*University of Glasgow.*

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DEAR SIR,

Our conclusion that the digit-doubling method of determining sleep thresholds is unsatisfactory for use *with depressive patients* is based on our data. Neither Claridge and Herrington nor Moffat and Levine used depressive patients. Is it our conclusion that is unwarranted—or possibly theirs?

Our findings would surely surprise no one, since co-operation, retardation and verbal responsiveness are severely impaired in some depressive patients, as indicated in our first paper (1962, pp. 469 and 472) and our second (1965, p. 171), and as discussed independently by Moffat and Levine. Even when good co-operation is achieved initially from depressives, they often find it "too much of an effort" to continue with the digit-doubling task. Subsequent checks have shown that a poor and erratic performance on this task occurs with severely depressed patients even *in the absence of sedative drugs*.

It obviously needs to be stressed that we have never aimed to replicate the work of Claridge and Herrington, but that of Shagass on *depressed* patients; however, we gladly incorporated their digit-doubling technique in our first experiment in the hope that it would introduce a more objective method of determining sedation thresholds. We abandoned it because it became obvious that the method was inapplicable to some of our depressives.