

(*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.

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#### 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.

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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.