under Sodium Amytal, with gradually reducing dose at successive attempts which were made on alternate days. After a few weeks she was able to insert the tampon herself without Sodium Amytal and retain it for many hours. The procedure was repeated several times at home in her husband's presence. This gave her confidence and she was able to engage in full coitus.

As in Dr. Cooper's case, the limited goal of treatment—namely, relief of vaginismus and subsequent consummation—was achieved, but with the use of tampons, which was more acceptable because it was something 'most women do'.

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AVERSION THERAPY FOR HOMOSEXUAL IMPULSES

DEAR SIR,

I feel compelled to comment on the paper by Dr. N. McConaghy (*Journal*, June 1969, p. 723) which compares aversion therapies for homosexuals. His paper is an interesting and welcome one, as I fully agree with the sentiment that 'further controlled studies and the use of more objective methods of assessing response' are necessary.

A present study conducted by myself (publication in preparation) shows two main contradictory findings with Dr. McConaghy's paper. In the first place I question his *a priori* assumption that a measure of penile erection in an experimental situation is an objective index of sexual orientation. Pilot studies on our patients have confirmed intuitive ideas that erection of the penis (even when unencumbered by apparatus) is liable to be influenced by so many uncontrollable factors (e.g. mood plus anxiety of subject) as to be totally invalid as an index of sexual orientation.

Instead of penile plethysmography, I have used two other indices: a semantic differential was used to measure sexual attitude before and after treatment (a modified form of that described by Marks and Sartorius); the second index was obtained by measuring the time spent in looking at projected male and female nude slides in a situation where the patient could change the slide whenever his interest in it flagged.

Measurements were taken before, during, and after treatment, of the time spent in looking at a variety of these slides and differences before and after treatment were assumed to be a result of the treatment.

The slides were selected to emphasize the cognitive cues for masculinity and feminity.

Using these two indices, as well as subjective reports of patients, I intend to report the results one year after treatment. A group of patients who have been treated with a form of anticipatory avoidance therapy, using faradic aversion, confirm the findings of Feldman and MacCulloch that anticipatory avoidance learning appears to be the training method most resistant to extinction. Our group of 20 patients have been followed up for six months at the date of writing.

My main objection to Dr. McConaghy's paper is that he has not paid enough attention to this factor of extinction.

My own findings suggest that if variable ratio schedules of reinforcement are used extinction of a conditional response is slower. Faradic aversion allows variable ratio schedules of reinforcement to be used. Slow *extinction* of a learned response, not fast attainment, seems to be the most important therapeutic advantage of faradic aversion over apomorphine.

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DEAR SIR,

I find it hard to understand why Dr. Stern refers in his letter to my '*a priori* assumption' that a measure of penile erection in an experimental situation is an objective index of sexual orientation. It was made clear in the paper that this was not an *a priori* assumption, and the reference to the data on which it was based was given (McConaghy, 1967). What may be the source of the apparent contradiction with Stern's findings is that it was not penile erection that was used as an index, but penile volume change. As was pointed out in the paper, most subjects have little awareness of the nature of these penile volume

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