point himself. He tells us that when parents handle this phase as a child's normal identification process and not as the threat of a competing adult then 'castration anxiety' in the child is minimal or undetectable.

Now, if it's undetectable, who's to say it's there?

My view is that if castration anxiety is undetectable in the normal child then the first (and most parsimonious) explanation that might occur to one is that it's simply not there.

It seems to me that those who consistently uncover such minimal evidence, or declare castration anxiety to be present but undetectable, may well be those with a vested interest in the theoretical posture that the Oedipus complex is universal and essential to the genesis of the male child's sexual identity.

It is, of course, unfair of Dr. Rubin to blame the British for generating 'Who's Afraid of Sigmund Freud,' as my mailing address at the end of the paper might have suggested to him had he detected it. The fact is that I was trained in the United States, live there and practise there. All that the British can be reasonably blamed for is publishing a controversial paper, an offence the American journals were scrupulously careful not to commit in this instance.

There is one criticism Dr. Rubin makes which I am constrained to reveal affords me narcissistic injury, his use of the term 'hatchet job' in describing my paper. Resident though I am in violent America, I find that criminatory appellation wounding.

You see, I had the fantasy that my attack on these aspects of American psychiatry which trouble me was more in the nature of a dissection, cutting perhaps but surely not so crude as the blows of a hatchet. I thought I was being subtle, even occasionally allusive in a way that an educated man might find pleasing.

Oh well! Next time I shall simply have to try harder.

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IMIPRAMINE AND OTHER DRUGS IN ORGANIC EMOTIONALISM

Dear Sir,

I am grateful to Dr. R. G. Priest, St. George's Hospital Medical School, for pointing out an error in our paper 'The Use of Imipramine ('Tofranil') and Other Psychotropic Drugs in Organic Emotionalism', which appeared in the *Journal* for March 1969.

There should be two carbon atoms opposite nitrogen in the formula for imipramine, and similarly for amitriptyline, i.e. those rings should be 7-member and not as shown with 6.

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'BEHAVIOURAL' TREATMENT OF NON-CONSUMMATION DUE TO VAGINISMUS

Dear Sir,

I have read with great interest the report of Dr. A. J. Cooper (*Journal*, June 1969, p. 721) entitled 'An Innovation in the "Behavioural" Treatment of a case of Non-Consummation due to Vaginismus' and would like to describe a case treated with a similar technique.

The patient, aged 24 years, was referred by the Gynaecology Department where she was sent in consequence of non-consummation after four years of marriage, the patient wanting to be assured that 'she had all she should have'. She was examined under general anaesthetic with a dilator retained so that when she recovered she could see for herself that the passage was large enough to accommodate a dilator. This did not make any difference to the sexual side of her marriage, although she was keen to have sexual intercourse but was unable to do so.

The patient, like that of Dr. Cooper, was a pleasant, co-operative woman, who, however, appeared to have a hysterical type of personality. On the E.P.I. she scored as neurotic and slightly extraverted (N = 17, E = 14). She described her condition by saying 'I am frightened of my inside'. She regarded her husband as being sympathetic and understanding, and apart from her sexual difficulties she had a perfectly happy married life.

Relaxation was initially achieved by intravenous Sodium Amytal. During this session she said that as a child she was brought up strictly by her mother, who regarded sex as dirty and sinful and told her to keep away from boys. She was also sexually assaulted by a man at the age of four, and learnt the facts of life from other girls in enormously distorted ways, which horrified her.

After several abreactive sessions she felt considerably improved, cheerful and relaxed, but still unable to have sexual intercouse, although she made several attempts at it. The thought of introducing a dilator was even more frightening to her, but she was agreeable to try with a tampon; this was done initially

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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References

N. McConaghy (1969). *British Journal of Psychiatry*, **115**, 723.

Marks, I. M., and Sartorius, N. H. (1967). 'A contribution to the measurement of sexual attitude.' *J. nerv. ment. Dis.*

Feldman, M. P., and MacCulloch, M. J. (1965). 'The application of anticipatory avoidance learning to the treatment of homosexuality.' *Behav. Res. Ther.*

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