

way than by using Kinsey ratings, and furthermore have made it easier for other workers to compare their results with mine if they choose to do so.

A4. Having been chided for introducing one correlation involving the above unvalidated ratings, it surprises me that MacCulloch and Feldman have published a whole paper on the statistical analysis of an equally dubious statistic (MacCulloch and Feldman, 1967). Their use of the Schneiderian typology is a form of description based on the clinical interview. I am unaware of any validity or reliability studies on this system. Furthermore, they do not describe in what way they apply this classification. Is it done before the treatment starts, and thus apparently based on one clinical interview only, or is it based on their experience of the patient during the course of treatment? If so the use of such labels as 'weak-willed' or 'attention-seeking' to describe those who do not respond to aversion therapy is of limited value. Terms such as 'passive' or 'ineffectual' may be no better, but I make no pretence in using them. Their origin, incidentally, is the English language, and I feel no need to apologize for using that as an aid to communication.

B1. There is more to treatment than technique and follow-up data. The 'manner of change' refers to the changes occurring during the course of treatment, not only during sessions but between them also. One of the aims of my paper was to give a fuller picture of such changes, which I considered to be important in understanding the mechanisms involved.

B2. I apologize for not having stressed the importance of previous heterosexual experience to the outcome of treatment, as I think this is one of the more valuable prognostic indicators. I am much less convinced, however, that it should be used to distinguish two aetiological types of homosexual.

B3 and B4. I am sorry to hear that MacCulloch and Feldman have not heard of Modern Learning Theory, but pleased to know that they are coming round to my way of thinking about attitude change. I would, however, respectfully offer them a word of warning. It is relatively easy to explain events, particularly if one uses a bit of avoidance learning, a bit of cognitive dissonance and a bit of incubation, but much more difficult to predict them. The value of their theoretical cocktail therefore depends on its usefulness in making testable predictions, particularly those of clinical relevance.

I entirely agree with them that clinical evidence should not be ignored. In my experience of approximately 60 cases treated with aversion therapy, only one has shown any convincing evidence of conditioned anxiety akin to a phobia. It is for this reason that I

consider conditioned anxiety to be relatively unimportant in aversion therapy.

B5. Finally, I must comment on their last paragraph. Curran and Parr's paper is often cited by those who wish to belittle the efficacy of psychotherapy for homosexuality. If this paper is read carefully—and the relevant details are extremely brief—it is not possible to say whether the treatment was aimed at reorientating the patient's heterosexuality or at helping him to adjust to his homosexuality. MacCulloch and Feldman should read the papers of Ellis (1956), and Mayerson and Lief (1965) for better results. Recently I pooled together all the available series of psychotherapy and aversion therapy for homosexuality, and found 186 cases showing a 42 per cent improvement in the former and 124 cases showing a 39 per cent improvement in the latter. Furthermore, it was the shorter, more directive, method of psychotherapy which gave the better results. MacCulloch and Feldman have already stressed that in many cases their brief course of aversion needs to be followed by social skill training which will certainly add to the treatment time.

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UNILATERAL AND BILATERAL ECT

DEAR SIR,

We have read with interest the report of Dr. E. Sutherland *et al.* (*Journal*, September, 1969, p. 1059) entitled 'E.E.G., Memory and Confusion in Dominant, Non-Dominant and Bi-Temporal E.C.T.' Certain issues of the study are unclear and warrant our questions.

While the authors refer to dominant and non-dominant hemispheres, we did not find an indication

of the number of right-dominant and left-dominant subjects in the sample. How many left-unilateral, and how many right-sided treatments were administered? Was the probability of a homolateral EEG slowing after unilateral ECT approximately equal in both hemispheres, irrespective of dominance?

We were puzzled by Table III which shows the assessment of type of ECT from a blind comparison of EEG before and after the course of ECT. That table shows that 24 out of 59 records were incorrectly classified, but fails to indicate what these records portrayed. We are particularly interested to know if any of the nine incorrectly classified records after bilateral ECT were lateralized, and to what side. We have examined records before and after a course of unilateral or bilateral ECT in 85 depressed subjects. The electroencephalographer was not aware of the type of ECT administered. The slowing after unilateral ECT ($n = 34$) was pronounced over the side of the placement of treatment electrodes. Bilateral ECT ($n = 51$) elicited left-sided slowing primarily. This expected finding is shown in the attached table.

Another question relates to the comment that the authors did not see any 'evidence to suggest that EEG changes were correlated with clinical improvement or otherwise'. We would like to know how EEG quantification was done to determine this relationship. In earlier studies, EEG slowing was shown to be a necessary, though not sufficient, condition for the behavioural response to ECT (Roth *et al.*, 1952; Fink and Kahn, 1957); and these results were arrived at principally because the authors attempted more than a descriptive estimate.

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EEG changes with bilateral ECT

Accentuation of the slowing	Pre-ECT	Post-ECT	Total
Left	6	28	34
Symmetric	40	18	58
Right	5	5	10
Total	51	51	102

($\chi^2 = 22.6, p + 0.001$)

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FINK, M., and KAHN, R. L. (1957). 'Relation of electroencephalographic delta activity to behavioral response in electroshock.' *Arch. Neurol. Psychiat.*, 78, 516-25.

ROTH, M. (1952). 'A theory of ECT action and its bearing on the biological significance of epilepsy.' *J. ment. Sci.*, 98, 44-59.

DEAR SIR,

Thank you for giving us an opportunity to reply to this interesting and important letter.

1. In our sample, in which EEG measurements were completed, we had 57 left-dominant and 2 right-dominant subjects. The 2 right-dominant subjects were given bilateral ECT (quite by the chance of random selection), and consequently all 22 patients given 'dominant ECT' had left-sided ECT and all 18 patients given 'non-dominant ECT' had right-sided ECT.

2. The details of changes after unilateral ECT are shown below:

22 patients given dominant ECT (i.e. left-sided).	18 patients given non- dominant ECT (i.e. right-sided).
14 correct (left-sided slowing).	11 correct (right-sided slowing).
5 bilateral slowing. 1 contralateral slowing. 2 no change.	2 bilateral slowing. 2 contralateral slowing. 3 no change.

3. The changes after bilateral ECT were:
10 correct forecasts (bilateral slowing)
5 showed right-sided slowing (all left-dominant patients)
2 showed left-sided slowing (left-dominant patients)
1 showed no change (left-dominant patient)
1 had a temporal lobe abnormality (left-dominant patient).

The 2 patients who were right-dominant were correctly forecast, i.e. had bilateral slowing after bilateral ECT. We have no evidence, therefore, to support Drs. Volavka and Abrams' finding of dominant slowing after bilateral ECT.

4. Finally, our measurements of EEG changes and clinical improvement were:

EEG changes: minimal, moderate, marked.

Clinical improvement: no improvement, improvement, much improved.

It was found that those patients who showed a marked EEG abnormality after ECT did not necessarily show the greatest clinical improvement, and of 6 patients who showed no detectable EEG change after ECT 4 showed 'improvement', 1 was 'much improved' and 1 showed 'no improvement'.

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