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DEAR SIR,

Drs. MacCulloch and Feldman raise many points. For the sake of reasonable brevity I shall not be able to deal with them all but will take the main ones in turn.

A1. When erection occurs the level of erection is significantly correlated with subjective ratings of sexual arousal ($R = 0{\cdot}6$–$0{\cdot}85$). This is partly due to the fact that the awareness of erection contributes to the subjective experience of sexual arousal. In fact erections can occur in situations which are not experienced as sexual (Bancroft 1970a), but it is likely that most people interpret erection as a sexual response, and it is for this reason that the measurement of erection seemed relevant to this treatment. If the idea of aversion is to associate some aspect of the deviant behaviour with an unpleasant stimulus then it makes sense to the patient, and it did to me when I first designed this method, that the shock should be associated with erectile response to a deviant stimulus or fantasy. Experience has shown that my initial assumptions were naïve, but the complex and paradoxical effects produced by this technique do require explanation and underline how little we understand about such situations. This particularly applies to the facilitation of heterosexual erections by the aversive procedure, an effect which has also occurred even more strikingly in a later study and appears to be of clinical relevance (Bancroft, 1970b). I shall be discussing these points more fully in a forthcoming paper on the methodology and validity of penis plethysmography.

The length of treatment is an interesting point. In the study in question I had, again naïvely, anticipated that the use of an objective measure of change would provide me with a clear-cut end point for treatment. This was not to be so, and the actual end point was often arbitrary. In a later comparative study (Bancroft, 1970b) I used a set number of thirty sessions. Most of the changes during the course of treatment occurred within the first fifteen sessions. It thus seemed possible that the last fifteen were superfluous or would have been better used in other ways. I would, however, hesitate to approach this as a problem of productivity, using modern techniques such as automation, until it is clearer which components of the treatment situation are the important ones. I do not believe that MacCulloch and Feldman or anyone else for that matter have yet clarified this in relation to aversion therapy.

A3. Had there been a reliable and valid rating scale which covered the area of sexual behaviour relevant to treatment I would have used it. In its absence I did the best I could. I maintain that I was able to communicate more useful information in this

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