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Correspondence

AVERSION THERAPY OF HOMOSEXUALITY

DEAR SIR,

We would like to comment on a recent paper by Dr. John Bancroft (*Journal*, 1969, 115, 1417–31). Our comments fall into two sections; the first is concerned directly with Bancroft's report of his own work, the second with his references to our own work.

A. *Comments on the Bancroft paper*

1. *The treatment technique.* Several questions need to be answered. Firstly 'the patient was asked to produce erotic homosexual fantasies while looking at photographs of males'. Were the same set of photographs used for all patients, or were the photographs tailored to each patient's particular interests? Were the photographs presented in a hierarchical order of attractiveness, or in random order? Affectively toned stimuli order themselves in a hierarchical way, and it seems obvious that it is more efficient to present stimuli of lesser attractiveness before those higher in attraction (cf. the desensitization treatment of phobias in which the principle of graded exposure has been shown to be important, whether treatment is in fantasy or *in vivo* (Marks, 1969). Was there any control of the length of exposure to the homoerotic stimuli, or was this entirely a function of the time taken for the patient to produce a penile erection of the predetermined amount? The response which was punished in the Bancroft technique was an increase in penile erection of 0·6 mm., as measured by a plethysmograph. When the latter technique was initially described in the literature (Freund, 1963), and in its British form by Bancroft, Jones and Pullan (1966), it appeared a significant advance in the objective monitoring of subjective sexual arousal. McConaghy (1969) recently reported in this journal an extensive study of aversion therapy using the penile plethysmograph response as the dependent variable, but in a letter commenting on this paper Stern (1969) pointed out several shortcomings of the technique, including non-occurrence of erection despite exposure to both visual stimulation and subjective arousal.

Doubts are also raised by a report by Law and Rubin (1969), who showed that under appropriate instructions heterosexual subjects were able to inhibit previously maximal erections by up to 50 per cent, even in the presence of erotically stimulating motion pictures. Finally, in the paper under discussion Bancroft himself has commented that 'the use of a low, often subliminal level of erection as the response to be punished has frequently resulted in variable and paradoxical effects' (p. 1429). We conclude that not only is the penile plethysmograph response suspect as the response to be suppressed in treatment but it is of doubtful value as the dependent indicant of the effects of treatment.

A comment is necessary on Bancroft's method of increasing heterosexual interest. This was attempted by instructing the patients to fantasize to heterosexual stimuli. Again what was the length of exposure to the stimuli, were they tailored to each patient, and were they presented in hierarchical order of descending attractiveness? (*see* Feldman and MacCulloch, 1965).

A final comment on Bancroft's treatment technique concerns the length of time taken for treatment: between thirty and forty aversion sessions, each lasting 1 to 1½ hours; hence a minimum of thirty, and a possible maximum of sixty, hours of skilled time. In evaluating a treatment its *efficiency* has to be considered as well as its *efficacy*. Our technique averages 20–24 sessions each of 30 minutes in length. Recent developments in the detailed and systematic variation of the learning programme used in treatment (Feldman, *et al.*, 1969), and in the automation of these advances (MacCulloch, Birtles and Feldman, 1970), should further add to the efficiency of the technique through the more precise tailoring of treatment variables to the individual patient, and the saving of therapy time.

2. *The results of treatment.* Bancroft agrees (*see above*) that his penile response criterion provides a poor discriminative stimulus for the patient. This is a crucial point in any learning technique, both for the acquisition of an avoidance response and for the post-treatment resistance to extinction of the newly acquired response (see Campbell and Church, 1969, for an exhaustive documentation). Accepting, for the moment, Bancroft's criteria of improvement, seven of his patients showed 'significant changes in sexual attitudes following treatment' (p. 1430). Two of the remaining three had had no pre-treatment heterosexual interest—we would predict failure with our technique also (MacCulloch and Feldman, 1967), and we would also predict the final failure of one of the four relapsing patients on the same grounds. Thus far we are in agreement, but the final failure of the other

three relapsing patients (A, F and H) seems to be due to features peculiar to Bancroft's method. All three did have prior heterosexual experience and interest, and as far as can be judged from Bancroft's personality description (referred to again below) none showed to a marked extent the features of personality reported by us to give an unfavourable prognosis (MacCulloch and Feldman, 1967a). We can only agree with Bancroft that 'such conflicting or confusing results could well have an adverse effect on the process of attitude change. A clear unequivocal type of response is perhaps more likely to be effective. MacCulloch and Feldman's method may be superior in this respect', (p. 1429).

3. *The method of assessment.* Bancroft describes a point scoring system for assessing sexual behaviour; 'the categories used, and the relative weightings given to them have been based on the clinical judgement of the writer' (p. 1418). Further '[they] should be considered as no more than a convenient way of presenting clinical information'. That is, they are an attempt to quantify the clinical interview; reliability and validity have yet to be demonstrated. At present it seems premature to carry out statistical operations on the scores assigned, or to attach weight to the results of such operations, e.g. the correlation between improvement score and age (Table II, p. 1421). While Bancroft is to be complimented on his attempt to describe his patients' sexual histories during each period of the life span, his 'scale' is not different in nature to a Kinsey rating, which is similarly based on the clinical interview.

4. *Description of personality.* Bancroft finds unacceptable the Schneiderian system of personality classification, which we have used consistently in our various publications; he prefers a nomenclature of unstated origin consisting of terms such as 'passive' and 'unassertive'. Nor do we find it easy to interpret such descriptions of parents as an 'ineffectual' father and a 'dominating' mother. The Schneiderian system does have the merit of being used by the World Health Organization, and by psychiatrists in Scandinavia and Germany.

B. *Comments on Bancroft's references to the work of MacCulloch and Feldman*

1. 'They provide relatively little detail of the manner of change occurring in their patients.' This phrase follows 'Feldman and MacCulloch have provided considerable detail about their electrical aversion technique (1965) and adequate follow-up data' (both in the same paragraph on p. 1417). How much detail is 'relatively little'? In fact, the 1965 paper gave a very detailed account of pre- and post-treatment sexual behaviour of *each* of the 19 patients reported on in that paper. However, we also warned the reader that the follow-up period was still relatively short for many of the 19, and a longer one was needed before firm conclusions could be drawn. It was in a *later* paper (MacCulloch and Feldman, 1967b) that we published the results of a minimum follow-up of one year on an unselected series of 43 homosexual patients. Space precluded individual reports, but we supplied details of the nature of the pre- and post-treatment sexual behaviours of the series. An even fuller account of this series, together with details of a controlled trial of the treatment of homosexuality (first described at a Symposium at the Middlesex Hospital in the summer of 1969) will appear in a forthcoming book (Feldman and MacCulloch, 1970).

2. 'As yet we are largely ignorant of the factors which decide such outcomes' (i.e. which homosexual patients respond to treatment). Bancroft attributes to us the statement that two factors predict a favourable outcome by our method: age and personality. The first point was mentioned in our 1965 paper (Feldman and MacCulloch, 1965) and was only a preliminary, subjective impression. At a later stage (MacCulloch and Feldman, 1967a) we showed that with age *controlled for*, personality still predicted outcome in terms of changes in attitude scale scores (see Feldman, *et al.*, 1966 for a description of the scale). Being aged over 40 does not *per se* militate against reacquiring favourable heterosexual attitudes, but we have found, as might be expected, that the older patients have more difficulty in acquiring heterosexual *partners* following treatment. Our personality finding was that, with other things equal, patients displaying disorders of personality of the weak-willed and attention-seeking types were most unlikely to respond successfully. Recently J. F. Orford (Feldman and MacCulloch, 1970) has gone some way to combining this clinical assessment of personality with scores on the C ('ego-strength') scale of Cattell's 16 PF questionnaire. Currently, we are attempting completely to replace the clinical interview by standard personality questionnaires. The clinical interview has been useful in showing the desirable direction of research on the relationship between personality and outcome; ultimately one requires a method of assessment which is completely communicable and generalizable. The phrase above, 'other things equal', refers to our finding (MacCulloch and Feldman, 1967b) that a major factor prognostic of success is a history of heterosexual interest (we term such individuals secondary homosexuals, as opposed to primary homosexuals who lack any such interest throughout their life span). As this finding was reported in 1967 and was obtained on a series of 43, it is surprising that Bancroft does not mention it in his

paper. In conclusion (Feldman and MacCulloch, 1970) two factors predict failure by our method, and do so independently: firstly, primary homosexuality; secondly those individuals who despite being secondary homosexuals have a personality disorder of the weak-willed or attention-seeking types.

3. *The importance of the learning technique.* Bancroft discounts the importance of 'modern learning theory' in understanding aversion therapy. So do we; there is no such thing as 'modern learning theory'. What does exist is a vast body of knowledge on the many variables relevant to the learning *process* (Campbell and Church, 1969), and the therapist designing behavioural learning technique should not neglect this. As mentioned earlier, Bancroft suggests that the superiority of our technique may possibly reside in its use of an unequivocal type of response. We agree with Bancroft that changes in behaviour following treatment follow from changes in attitude which are induced by the behavioural technique used in treatment, and that theoretical formulations of attitude change are important in accounting for the long term efficacy of treatment. We elaborate this in detail in Feldman and MacCulloch (1970). An excellent survey of the large experimental literature on attitude change, together with a detailed analysis of its relevance for therapy, is given by Goldstein, Heller and Sechrest (1966).

4. *Generalization from treatment to real life.* Bancroft finds it puzzling that patients generalize from within treatment avoidance to real life—when they 'know' they cannot be shocked. Eysenck (1968) has cited many reports, both from animals and humans, of the phenomenon of 'incubation', that is, an increment in response when the CS (neutral stimulus) is presented alone, in the absence of the UCS (averting stimulus). At the meeting on aversion therapy held at the Middlesex last year, referred to above, we showed a film in which the phenomenon was demonstrated in two volunteer human subjects; both continued to avoid and to show physiological 'anxiety', on the presentation of a previously attractive stimulus *after* shock electrodes had been removed and they 'knew' they could not be shocked. Clinical evidence should not be ignored—if humans ceased avoiding when they objectively 'knew' no danger threatened, phobias would not be a source of psychiatric referrals.

There can be little doubt that generalization from treatment to real life does occur; what is desirable is to explain *how* it occurs, and why it does so in some patients more than others, irrespective of whether an instrumental or a classical learning technique is used (Feldman and MacCulloch, 1970). As treatment progressed many of our patients spontaneously reported finding themselves in real life looking away from previously attractive males and looking at previously less attractive females, and also *rehearsing* in fantasy both sequence of events in treatment and the real life behaviours which represent the reproduction of the responses acquired in treatment. Indeed, we consider that both laboratory research workers and behaviour therapists have grossly neglected the events *between* sessions of treatment which are uncontrolled by the therapist. However, this does not mean that we should go to the other extreme and ignore the body of knowledge on learning within treatment sessions; merely that additional concepts are necessary for a complete account of the generalization process. In order to provide such an account we have extended Eysenck's concept of incubation, as well as Festinger's notion of cognitive dissonance reduction (Festinger, 1957), to account for between-treatment increments in responding (Feldman and MacCulloch, 1970). A further point, on which there is extensive experimental evidence (Kimble, 1969), is that the greater the similarity between training stimuli and real life stimuli the greater is the amount of transfer of training which occurs.

5. *Heterosexual learning.* We agree with Bancroft that the restoration of heterosexual interest is as important as the reduction and, hopefully, elimination, of homosexual interest; indeed, the two go together. We also agree that therapy must involve supporting the patient during the difficult period immediately following treatments. In order to make such support as effective as possible, research on heterosexual social skills is urgently needed, so that the therapist can re-train his patient in the essential social preliminaries of heterosexual behaviour—at present vague and non-specific advice is all that is available. Bandura (1969) has elaborated on the importance of positive social reinforcement in a recent book based on his influential social learning theory.

In conclusion, we were pleased to see Bancroft's paper; carefully conducted research in this field is needed—the referral rate for all types of sexual deviation is unlikely to be less than that for the much more extensively studied phobias. Moreover, the success rate of psychotherapy is rather low (Curran and Parr, 1957), although psychoanalysts (Bieber *et al.*, 1962) have shown more optimism. However, the latter technique is immensely time-consuming, so that aversion therapy has promise from the points of view of both efficacy and efficiency.

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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 = 0·6–0·85). 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