Brit. J. Psychiat. (1969), 115, 121–6

Correspondence

UNILATERAL E.C.T.

DEAR SIR,

The letter by Drs. Cannicott and Amin (*Journal*, November, 1968, p. 1483) on the subject of unilateral E.C.T. cannot be allowed to pass unchallenged. Their plea for the adoption of this technique as the "standard practice" rests on the shakiest of evidence. It is not correct to assert, as they do, that "all studies have shown that besides being as effective clinically in relieving depression it is much more comfortable and less traumatic for the patient". There are in fact a number of studies which have either failed to confirm the over-enthusiastic reports on this method or have produced equivocal results, (McAndrew *et al.*, 1967; Levy, 1968; Strain *et al.*, 1968). Many of the earlier investigations are open to serious criticism on methodological grounds (Strain *et al.*, 1968), and even later studies are not without their defects, e.g. the Orientation Tests in the paper by Valentine *et al.* (*Journal*, August, 1968) do not appear to have been carried out using a double-blind design. There are suggestions that patients receiving unilateral E.C.T. require more treatment or take rather longer to recover, and there are also greater risks of producing skin burns since the shorter interelectrode distance increases the chances of a short circuit across the skin between the electrodes.

A more balanced assessment would be that the unilateral method is an interesting one which would repay further systematic and objective study, particularly in patients who are "at risk" as far as the development of serious memory disturbance is concerned, e.g. the elderly. It would appear that this technique does tend to produce less memory impairment, but that its advantages fade away rather rapidly, as can be seen by the comparison of the results on test 3 and test 4 in the paper by Zinkin and Birtchnell (*Journal*, August, 1968, p. 973).

I submit that the evidence at present does not warrant the abandonment of the standard bilateral method which when used with the correct indications is one of the safest and most effective methods of treatment in psychiatry

The imminent appearance of an American textbook with a chapter on unilateral E.C.T., which the authors announce with such flourish, cannot be accepted as serious scientific evidence in favour of a new method of treatment.

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DEAR SIR,

I was very interested in your articles on unilateral E.C.T. and while accepting that it appears to be a progressive step I feel that there are simpler and more basic steps we could take to make E.C.T. a more acceptable procedure for many of our patients.

In a recent paper on the subject (1) I gave the results of a study analysing the factors which patients objected to most. It was surprising to note that memory impairment was well down on the list of results, and that factors such as "waiting for treatment", probing "for veins", "hearing other patients having treatment" and "being conscious yet unable to breathe" were much more prominent.

Another cause for anxiety is the concept of having electricity passed through the brain. This anxiety is usually allayed in most patients when it is explained to them that it is not the electricity that matters but the convulsion. Most surgical patients have the rudiments of their treatment explained to them, so why not those who are having E.C.T.?

At present in the Nassau Mental Health Service it is impossible to obtain the help of an anaesthetist without straining the health system to its limits, so all E.C.T., except in exceptional circumstances, is still done straight. Surprisingly, however, our patients make few complaints of the kind met with in England, where patients have the benefit of anaesthetic and relaxant agents. I am at the moment repeating this study (to be published later), and it is already clear that in our centres far fewer are upset by E.C.T.

While appreciating that a cultural difference exists, we must realise that the very nature of this treatment provokes apprehension. It seems that

many complaints stem from our manner of applying it, and perhaps we should revise some of our routines. Despite the relative simplicity of the technique, it remains an ordeal for many of our patients.

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PHYSICAL ILLNESS IN PSYCHIATRIC PATIENTS

DEAR SIR,

I was most interested in the article by Drs. Maguire and Granville-Grossman (*Journal*, November, 1968, p. 1365). It highlights a problem that will probably increase if the present trend in first admissions in the over-sixties is maintained.

One of the most worrying features is the high number of cases undiagnosed prior to admission; this must result in inferior or incorrect treatment in some cases. In my own recent study of 250 consecutive admissions to a city mental hospital (Johnson, 1968). I restricted my attention solely to those cases which could be diagnosed, or highly suspected from the routine physical examination on admission. Fifty-three cases (20 per cent.) were diagnosed as having a physical illness requiring teatment. In thirty admissions (12 per cent.) it was thought that the physical state was an important aetiological factor in the presenting psychiatric symptom. Twenty-four of these cases (80 per cent.) were undiagnosed at the time of admission. In two of these admissions it is possible that earlier diagnosis of the physical illness might have saved the patient's life. One patient was moribund on admission from haematemesis, and the other suffering from broncho-pneumonia and multiple injuries. Of the thirty cases with physical illness as an important precipitating factor, eighteen (60 per cent.) were over the age of sixty.

The plea made in the article for a thorough routine physical screening of all psychiatric admissions is certainly substantiated by these figures.

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PSYCHODYNAMIC CHANGES IN UNTREATED NEUROTICS

DEAR SIR,

May I be permitted to reply to the letter from N. McConaghy (*Journal*, September, 1968, p. 1197)?

I must admit that this made me think for about forty-eight hours before seeing the solution that was clearly implied in our paper. In order to show this, it is necessary to repeat McConaghy's reasoning in summary. This was as follows:

(1) We admit that there appears to be no detectable difference in the percentage of symptomatic improvements between series of treated and untreated patients;

(2) We claim that symptoms are a response to identifiable stress;

(3) We suggest that psychotherapy enables a patient to handle stress without getting symptoms; but

(4) *Since there is no reason to suppose that treated and untreated patients differ in the degree of stress they experience,* one of our propositions (1), (2), or (3) must be incorrect. Though McConaghy did not say so, the obvious candidate is proposition (3).

The fallacy in this reasoning lies in the passage in italics in (4) above. McConaghy implies that exposure to stress is beyond the patient's control. Of course this is not so. A patient who has *not* recovered from his basic anxieties will tend to withdraw from stress; one who *has* recovered will not need to withdraw from it, and indeed should actively seek it—most of the stresses postulated in our paper are a necessary part of normal life. A series of *symptomatically and dynamically* improved patients should therefore experience a greater degree of stress than a series that is *symptomatically* improved only.

It is thus perfectly possible for the symptomatic improvement rates in treated and untreated series to be similar, and yet for psychotherapy to be effective. This would apply even if the improvement rates in the two series were known to be exactly equal, which obviously they are not; and if in dynamically unimproved patients there were always a one-to-one relation between stress and symptoms, which is obviously not so. Both of these two facts weaken further the kind of reasoning that McConaghy uses.

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