disorder? At present this is not known. However, if this were the case one would expect a group of patients switched 'blind' to a placebo to relapse more quickly than a group of patients stopping lithium on their own initiative. The latter would probably be glad to be rid of any 'familiar side effects' rather than be disconcerted by their disappearance. In fact, the relapse rate of both groups is similar (Schou, 1970).

Observer bias in a double-blind trial is dependent on the doctor recognizing the active drug by its side effects. In patients on maintenance lithium it is difficult to decide whether symptoms, often transient, such as tremor, nausea, diarrhoea and polydipsia, are due to lithium, to suggestion or to minor coincidental disorders. Thus it was not surprising to me that patients in the placebo as well as the lithium group reported minor side effects. Accurate guessing as to whether a patient was in the lithium or placebo group was not possible, and at least at a conscious level the trial appeared totally blind to me. In a double-blind trial which significantly favoured lithium's prophylactic action no side effects were observed in either group (Baastrup *et al.*, 1970).

The existing evidence does not support Professor Blackwell's contentions, and indeed tends to refute them.

However, Professor Blackwell claims to find support from the data of my trial, but in his use of the data he makes the following two errors:

1. His calculations include only 4 of the 9 lithium patients and 7 of the 9 placebo patients. His conclusions are generalized to all the patients on lithium and the placebo, and the fact that he ignored the 5 patients with the longest remissions on lithium leads to a distortion unfavourable to lithium.

2. He treats the 'length of remission' as if it were the length of an entire cycle (= from first day of one episode until first day of next episode). From the pre-treatment (i.e. pre-lithium) episode frequencies he calculates the mean length of the pre-treatment cycles and then compares them to the 'lengths of remission' on blind lithium and placebo. But the 'length of remission' in my study (= number of days from starting trial until an episode) is only *part* of a cycle, for the reason that patients started the trial during intervals between episodes.

From his erroneous application of the data Professor Blackwell infers that the cycles of patients on blind lithium were the same length as their pre-treatment cycles, and that the cycles of patients on the placebo were shorter than their pre-treatment cycles. The conclusion which he should have drawn is that in *some* patients on blind lithium *part* of their cycles were as long as their *entire* pre-treatment cycles; and in *some* patients on the placebo *part* of their cycles were shorter than their *entire* pre-treatment cycles. This conclusion has little value. Furthermore, if Professor Blackwell's erroneous conclusion had been valid, it would have been open to the interpretation that the placebo patient's cycles were shortened in accordance with the natural history of recurrent affective disorders (Grof *et al.*, 1970).

Until the points which Professor Blackwell has raised have been satisfactorily elucidated by further research, clinicians will have to assess the present evidence for lithium's prophylactic action and balance the risk of lithium toxicity against the possible risk of withholding lithium. It is a reminder of the dangers of recurrent affective disorders that in a group of 29 patients studied over a four-year period the only death occurred in a patient who stopped taking lithium: eight months later, after she had experienced two further episodes, she was found drowned in her bath. In the past she had made several suicidal attempts (Melia, 1970b).

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TREATMENT OF PHOBIC PATIENTS WITH ANTIDEPRESSANTS

DEAR SIR,

It would seem that both authors of 'Treatment of phobic states with antidepressants' (Kelly *et al.*,

Brit. J. Psychiat., **116**, 387–98) and the Editors of this *Journal* (Correspondence **117**, pp. 119) have misunderstood Dr. Mawson's criticisms of the above paper (idem Vol. 1A p. 117), and misunderstood him in a particularly interesting fashion.

Mawson states that the above investigation was not conducted in such a way as to furnish evidence for the final conclusions, and feels that such unsubstantiated findings should no longer be published in this *Journal*.

He argues that the following methodological flaws have been committed, and that the case made for the use of antidepressants in the treatment of phobic states is therefore not proven by this investigation. Briefly: the data were bias-prone since they were derived from the clinical records of the doctors prescribing treatment, the data were assessed retrospectively, there having been no previously established standardization of data collection, and the appropriate corrective procedures such as independent rating of clinical records and the use of control groups were not undertaken.

It would have been reasonable to expect that the authors, in their reply, would argue that the faults had either not been committed or did not imply that their conclusions were unwarranted. Sadly, they have done neither. They merely reiterate the findings of their investigations, and state that the potential value of their treatment regime has been established to their satisfaction.

The point of interest is that the effects of attention and placebo reaction are generally assumed to be present in all treatment regimes and are not, in these studies, the object of investigation. Therefore, what matter if the first uncontrolled study yields favourable results? This may be only the result of the above factors. A proper investigation will have to be done anyway (N.B. the prospective study being carried out by Kelly), and could profitably have been done in the first place.

The Editors themselves raise a number of equally interesting points, among them that in their view Dr. Mawson 'expects too much', not only of their *Journal*, but of psychiatry as a whole.

The first point, by implication, is that because criticisms can be made of any work, no one piece of work is better than any other. Their lament: 'Even controlled drug trials contain a large make-believe element, since serum levels of the drug are not monitored over the trial period', prompts the reply 'Monitor the serum levels'. One does not say that a clearly malfunctioning watch is, after all, a reliable timepiece simply because even a more precise one has its own, far smaller, error. Implicit in their comments is in fact the view that some investigations *are* better than others (witness the initial 'Even' above) and of course the whole point of Mawson's criticisms is that some procedures make findings move further up into the regions of comparative acceptability than do others.

And further, the Editors then reiterate precisely what is in question: 'It is not possible to get, by giving standardized doses at set intervals over a fixed length of time to an arbitrarily selected group of patients, the same results from a psychotropic drug as can be obtained by a clinical expert sensitively selecting his patients and dosages, individual by individual, on a basis of experience'. The point of interest here is that despite the undisputed non-comparability of the results, all the adjectives would suggest that the results of the clinician are 'better' (along an impermissible scale). The factors which make such comparisons dubious have been set out in Mawson's letter.

That these confusions are not confined to the authors of the papers and the editorial board is shown by Dr. Freeman's (*Journal*, September 1970) misunderstanding of the whole problem, since he takes as demonstrated precisely what is at issue, the value of M.A.O.'s in phobic states, and then berates Mawson for 'the neglect of practical and humane considerations,' namely suggesting that the supportive evidence is unsatisfactory. At this level it is intellectually arrogant to refuse to dispense imaginary goods.

Finally, one regrets the publishing of methodologically flawed investigations in any journals, but since standards in these matters are open to vigorous discussion, perhaps correspondence rather than static editorial policy will provide the necessary corrective.

Above all, we must move away from the position in which it would seem that the ultimate criterion of the veracity of psychiatric findings is that they should arouse in the investigators a feeling of satisfaction.

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DEAR SIR,

My departure to work overseas for a few months has prevented me from replying earlier to Dr. Mawson's latest letter. However, as well as giving time for further reflection, this interval has provided me with a completely new dimension of psychiatric experience, which has very much reinforced the views I expressed in your September issue.

The most important of the 'real issues at stake' was Dr. Mawson's castigation of your own editorial