

Your correspondents warn that 'great danger indeed exists in combining the tricyclic drugs and MAOI's in large dosage', and then go on to report in support of this statement a case where a patient has received not a large dose but a gross overdose of one of the drugs.

Their patient received up to 180 mg. of phenelzine daily for ten days; a large enough dose to account for all of the adverse effects observed even if the patient were not receiving any other drug. Of course the addition of 75 mg. of amitriptyline daily no doubt ensured the certainty of disaster.

180 mg. of phenelzine daily is more than twice the maximum recommended dose and four times the usual starting dose even when used alone. It is twelve times the reasonable starting dose when used in combination with amitriptyline (1) (2).

This case adds no new information to our knowledge of the dangers of any of the drugs involved, but it does underline the importance of ensuring that there is careful medical management and control when powerful and potentially lethal drugs of any nature are prescribed.

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REFERENCES

1. WINSTON, F. (1971). 'Combined antidepressant therapy.' *Brit. J. Psychiat.*, **118**, 301.
2. SCHUCKIT, M. *et al.* (1971). 'Tricyclic antidepressants, and monoamine oxidase inhibitors.' *Arch. gen. Psych.*, **24**, 6.

THE CONTINUUM MODEL OF MANIC-DEPRESSIVE PSYCHOSIS

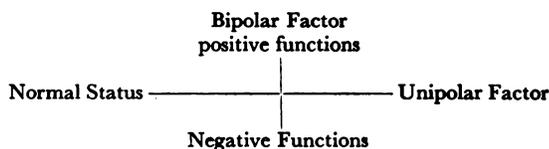
DEAR SIR,

J. H. Court (*Journal*, February 1972, p. 133-41) has sought to reaffirm that a continuum model explains manic-depressive psychosis better than the bipolar Kraepelinian model. He draws attention in the continuum to a number of unipolar variables, namely, progressive failure of the sodium pump, increasing reaction time and insomnia; one might add increasing distractibility and psychoticism. The correlation of these variables with mood is a valuable concept. However, it is inherently unlikely that so complex an illness could be modelled, as he suggests, on a single dimension. (Following his argument *ad absurdum*, minimal hypomania is predicted to alternate with maximal depression.)

The unipolar variables from which he constructs the continuum are, with the exception of mood, primarily physiological, relating to underlying biochemistry and brain function. Conversely the bipolar

model is a clinical or behavioural model. These may not be mutually exclusive, but rather complementary.

Bipolar changes from one clinical state to the other are the characteristic of this illness. They are obvious in mood, activity and speech, but also apparent in changes in superego pressures, in the tendency to take percepts from internal or external cues and the alterations in the balance of intra/extrapunitive hostility. At the risk of creating a fourth 'model', I would suggest that these two sets of functions, the unipolar and bipolar, seem likely to subservise different physiological (and/or psychic) phenomena. They might usefully be investigated against a combined axis as under:



It is likely that a complete statement to account for the wealth of clinical presentations would involve many more variables than these two groups.

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BRAIN HYPOXIA

DEAR SIR,

I am writing to you, as one of the Assessors for your *Journal*, to protest about the review of *Brain Hypoxia*, edited by Brierley and Meldrum, in the February 1972 issue (p. 239).

The review is not only unfair to contributors, editors and publishers of the symposium, but is singularly unhelpful to your readers who may want to know whether to get the book from a library or even whether to buy it. Your reviewer's criticism that in this volume '... results can be made public for a second (or even a third) time...' is unjustified. The book, in fact, contains an unusual amount of new information; for example in the section on the physiological and neuropathological effects of hypoglycaemia in adult and new-born animals, in the section on the physiology of induced seizures, or in that on continuous monitoring of intracranial pressure during and after exposure to hypoxia. And I for one shall certainly refer students to this book, if they want to find out how these problems and many others are being tackled experimentally and clinically.

As far as I know there has been no Symposium on cerebral hypoxia and related subjects for ten years. These well edited and illustrated Proceedings would

have been most welcome even if the contributions had not been original.

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SABINA J. STRICH.

DEAR SIR,

All serious physicians and research workers will regret the publication of your peevis and highly misleading review of *Brain Hypoxia*, edited by J. B. Brierley and B. S. Meldrum.

I did not have the good fortune to attend the conference, but I have bought and studied this major and important work. It represents a landmark in our understanding of the way in which the insult of anoxia can damage the brain. Those who did attend the conference tell me that it was one of the most exciting and stimulating meetings of its kind. The delegates were all of international repute and their contributions are of the highest quality.

Serious psychiatrists are daily faced with the tragic results of our inadequate knowledge of brain hypoxia and its prevention. Workers in many other disciplines are also involved, particularly those concerned with obstetrics, perinatal paediatrics, neurosurgery and space travel. Epileptologists have an immediate and practical involvement.

The volume concerned is most beautifully edited and presented. Its publication is subsidized so that the price is remarkably low. This is a work that can be warmly and confidently commended to all your readers, except for those whose interests are limited to laboratory trivia.

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DEAR SIR,

I agree with both your correspondents that *Brain Hypoxia* is an excellent example of a book which is a collection of separate papers given at a symposium. It is well edited and printed, many of the papers are of a high standard, and Drs. Brierley and Meldrum can be congratulated on their production of a most attractive work. Nevertheless, it is only in the most unusual circumstances that the proceedings of a conference should be given wider circulation than among those who were actually present at it.

In general terms, the aims of scientific publication are: (1) to present the results of new research work and to discuss them, (2) to review the work done in a given field, and (3) to instruct students, other research workers, or a wider audience in a scientific

subject. It is unlikely that the proceedings of a symposium will contain any worthwhile new information, because the majority of authors always first publish their results in reputable scientific journals. It is unusual for reviews to be given at such meetings, nor can it be maintained that a collection of papers from a symposium is an adequate substitute for a properly ordered textbook.

In the list of periodicals which have been more than once quoted between 1950 and 1960 in papers published by the *Journal of Physiology*, no less than 383 journals would be expected to accept papers dealing with aspects of brain hypoxia. Why add to this disastrous state of affairs by printing at length all the papers read at meetings?

The aim of my review was to discourage the unfettered proliferation of books of this kind; I do not regard it as either 'unhelpful' or 'highly misleading'. Most libraries, for instance, will feel compelled to buy these works. I, for one, object to their resources being squandered in this way.

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THE TREATMENT OF OBSESSIONAL NEUROSIS

DEAR SIR,

Obsessional states constitute one of the most disabling forms of neurotic illness (1, 2). Consequently, the observation that tricyclic antidepressants in high dosage may bring relief to a proportion of patients suffering from these disorders (3)—a view which has received support from uncontrolled studies (4, 5)—is of special interest. In this communication we report a retrospective investigation into the long-term effects of this form of treatment undertaken to determine whether the findings justified carrying out a prospective controlled trial.

A search of the case records of in-patients and out-patients attending the Professorial Unit of this Department over the past six years revealed that only 16 patients could be confidently diagnosed as suffering from an obsessional neurosis. The criterion for inclusion were that:

- (1) ideas, images or impulses recognized as morbid and obtruding repeatedly into the individual's thought processes formed the central feature of the disorder;
- (2) there was a sustained resistance against the ideas and the acts towards which the individual was compelled;
- (3) they were not secondary to other conditions such