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SUSTAINED RELEASE AMITRIPTYLINE (LENTIZOL) IN DEPRESSIVE ILLNESS

DEAR SIR,

Dr. McGilchrist's response to our letter (*Journal*, January, 1973, **122**, 119–120), concerning Dr. Haider's study of sustained release amitriptyline versus placebo in depressive illness (*Journal*, May, 1972, **120**, 521–522) misses our point. We did not assert that single daily dose ordinary tricyclic medication was proven to be as effective as multiple doses, but rather that this seemed quite likely. Therefore, before a new 'long-acting' drug preparation is manufactured it seems reasonable first to ascertain if available ordinary drugs can serve the long-acting purpose.

Dr. McGilchrist states that 'My company is, of course, aware that both forms of amitriptyline should be compared in a once-daily dosage, and are [*sic*] at present conducting such clinical studies.' We would suggest that the first study to be done is comparing ordinary amitriptyline in divided and single doses. If single dose ordinary amitriptyline is as effective as divided dose and is well tolerated, there would be no need to produce a sustained release product. Other issues, such as decreased total daily dosage, might also be secondary to the single vs. multiple dose issue rather than due to the sustained-release dosage form. Dr. McGilchrist's statement that the two preparations have different physical characteristics does not establish therapeutic differences.

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NEGATORS IN THE SPEECH OF DEPRESSED PATIENTS

DEAR SIR,

I feel Brahm Norwich's letter (*Brit. J. Psychiat.* February 1973, **122**, 244), requires, rather than deserves, a reply. His most serious misunderstanding of my paper is reflected in his comment that I did not offer subjects 'alternatives between words and their opposites'; this reveals that he has not understood even the basic aim of the procedure, which was certainly not to provide opposites but indeed *semantic synonymities* both with and without negators. A more careful reading would have obviated this spurious criticism. Norwich simply brushes aside all my methodological criticisms of the original paper by Hinchcliffe *et al.* (*Brit. J. Psychiat.*, **118**, 471–472), seemingly as if the right level of significance in the end justifies any unsatisfactory means of achieving it. Perhaps he might anyway be interested in a very large study by Pylyshyn (1970) which showed that when corrected for sample size there was no significant difference between negation in speech of depressives and other diagnostic groups. This latter study demonstrates even more the need for great care in technique, as before sample size had been corrected depressives showed a small excess in negation ($P < \cdot 05$), although neurotics showed an even greater excess ($P < \cdot 01$). The critical zeal of Norwich leads him even to carp at my preference for the Wakefield Self Assessment Depression Inventory over the Zung Scale. The former is in fact a truncated form of the latter, well validated against the Hamilton Depression Scale (the reference was afforded and this was explained), and since these scales were only being used to dichotomise between depressed and non-depressed subjects the criterion is truly grasping at trivialities. At the end of my paper I made a plea for rigorous methodology in psycholinguistics applied to psychiatry. Sadly, Brahm Norwich complains of my 'over-constricted theoretical framework' and further states 'what passes for "linguistic theory" in his behaviourist scheme of things is only a simplistic version of a possible linguistic theory'; he has sadly misconstrued me, I think, as a Skinnerian linguist, which I am not, and he seems to be saying, in essence, that you don't have to believe the world is round providing you aren't so particular about admitting the existence of an horizon. His point about presence of anxiety or threat as a case against the original Hinchcliffe *et al.* paper, I fail to comprehend (though perhaps *they* do); further, his comment 'it is conceivable that the significant use of negators represents a cognitive construct system-processing information in a negative form' has the sound of fine words clothing little sense. Finally, Norwich says that there is much scope for further research using recorded verbal samples; he is right, and I would refer him to recent papers by myself using just this technique (Silverman, 1972; 1973).

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MYASTHENIA GRAVIS AND SCHIZOPHRENIA

DEAR SIR,

Having read the interesting paper by Drs. Gittleson and Richardson (*Journal*, March 1973, 122, 343–4) I thought it might be worth while to report another such case.

Mrs. I.P. was born in Dortmund, Germany, in 1937.

Family history. Her father died during the war. He suffered from a 'nervous illness' of which no details are known. Her mother has been well; she remarried and the patient has two step-sisters.

Personal history. She had an uneventful childhood and left school at 15. She worked as a shop assistant and married, aged 19, a British national serviceman stationed in Germany. They came to England in 1957, separated in 1962, and later divorced. There were no children.

Past psychiatric history. In 1954 at the age of 16 the patient was admitted to a psychiatric hospital in Germany suffering from auditory hallucinations and paranoid delusions and was diagnosed as having paranoid schizophrenia. She was treated with ECT, drugs and 'fever therapy' and recovered.

History of present illness. In December 1963 she began to notice nasal regurgitation, slurring of speech and general weakness. In March 1964 she was admitted to St. Mary Abbots Hospital, Kensington London, diagnosed as having myasthenia gravis and was treated with neostigmine and pyridostigmine. She was also noted to be pregnant. Soon after her discharge she was readmitted with a spontaneous abortion and within a week she became auditorily hallucinated. She was transferred to the National Hospital, Queen Square, where she expressed the belief that people were trying to control her and were able to read her thoughts. She was investigated, and LE cells were found on one occasion, but this was not confirmed. She was treated with phenothiazines and recovered in the course of a month.

In July 1964 she had a thymectomy at the Middlesex Hospital but continued to require neostigmine and pyridostigmine. From then until 1970, when she returned to Germany, she was admitted to the Middlesex Hospital rather more than once a year because of her severe myasthenic symptoms, which were poorly controlled. These were difficulty in chewing and swallowing; slurring of speech; impaired grip with a tendency to drop things; weakness of the back and legs; and back pain. Signs noted were bilateral ptosis, weakness of palate, face and jaw and general wasting and weakness of the musculature of trunk, arms and legs.

In 1967, 1968 and 1970 she was admitted to the Middlesex Psychiatric Unit at Woodside Hospital with florid psychotic symptoms. On the first two occasions she was transferred from the neurological ward, where she had been admitted because of an exacerbation of her myasthenic symptoms. In 1967 she was hallucinated, with accusatory voices; she felt that electricity was playing on her and she misidentified people. In 1968 she was restless, agitated and at times disorientated; she was deluded and auditorily hallucinated and her mood was labile and incongruous. In 1970 she was found to be disturbed, thought-disordered and expressing delusional ideas. On each occasion she was treated with chlorpromazine and trifluoperazine and she settled down after periods in hospital of 2 months, 4 months and 3 months. After her recovery in 1967 she again became pregnant and she had an uneventful therapeutic abortion.

It is of interest that after her mental state had improved in 1970 she developed an arthropathy and a pericardial rub indicating active disseminated lupus erythematosus, and she was treated with azothiaprine.

In Germany her psychotic symptoms have recently been attributed to an ephedrine psychosis. In 1967 and 1968 she was taking ephedrine 30 mg. t.d.s. and atropine 0·6 mg. t.d.s. but she was not taking any in 1964 or 1970 and her first psychotic illness occurred ten years before the onset of myasthenia.

I do not think there can be much doubt that the psychiatric diagnosis was a recurrent schizophrenic reaction to the stress of severe myasthenia (associated here with DLE) in an individual shown by her illness at 16 to be predisposed to this form of psychosis.

I should like to thank Dr. Michael Kremer and Dr. J. A. Hobson for permission to report this case.

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DICHOTOMOUS THOUGHT PROCESSES IN ACCIDENT-PRONE DRIVERS

DEAR SIR,

I read the paper on accident-prone drivers by Plummer and Das (*Journal*, March 1973, 122, 289), with considerable interest, but doubt whether this study supports their conclusions. My main criticism rests on the composition of their groups and the concept of accident proneness.

It is well known that young drivers aged between 17 and 25 have higher than average accident rates; that men greatly outnumber women in this kind of misfortune; and that the hazards for young motor cyclists are very much greater than those to which car drivers are exposed. The control group in this study contained rather more women and had a mean age