

WELLS, P. G., and MALCOLM, M. T. (1971). 'Controlled trial of the treatment of 36 stutterers'.—*British Journal of Psychiatry*, 119, 603-4.

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.

ARTHUR E. RIFKIN,
DONALD F. KLEIN,
FREDERIC M. QUITKIN.

Hillside Hospital,
75-59 263rd Street,
Glen Oaks, N.Y. 11004, U.S.A.

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 ($P < .05$), although neurotics showed an even greater excess ($P < .01$). 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).

G. SILVERMAN.

University of Sheffield Department of Psychiatry,
Whiteley Wood Clinic,
Woolfenden Road, Sheffield, S10 3TL.