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Correspondence

Letters for publication in the Correspondence columns should be addressed to:

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HALOPERIDOL IN THE TREATMENT OF STUTTERERS

DEAR SIR,

The reported success of haloperidol in the treatment of stutterers by Wells and Malcolm (1971) raises two issues. Namely, is haloperidol capable of consistently producing substantial improvement in the speech of stutterers; and, if so, is this exceptional and felicitous effect attributable solely to haloperidol's anxiolytic potency or to some other pharmacological action?

On the first issue, the literature is divided. Whereas Wells and Malcolm found that 10 out of 12 stutterers improved on haloperidol, Tapia (1967), while reporting definite improvement in five out of six tiqueurs, found definite improvement in only two out of 12 stutterers. In a recent study in this laboratory, conducted on the same general lines as the Wells-Malcolm investigation, the speech of 18 typical stutterers was measured before and on the 21st day of haloperidol administration. Speech samples were a formally rated 1000 syllable sample of spontaneous speech, and a covertly obtained sample of informal conversational speech, summated to a total of three minutes, taken when the stutterer was unaware that his speech was subject to scrutiny.

The scores of four subjects showed substantial improvement (i.e. a reduction in the frequency of stuttering of greater than 50%), those of six other subjects showed lesser degrees of improvement, and eight actually deteriorated. There was a tendency for stutterers to show less improvement in casual conversational speech than in formal laboratory measurement sessions. Overall, the mean frequency of stuttering in the group fell from 16·7 per cent to 12·3 per cent of syllables spoken.

One factor that might account for the discrepancy between these results and those of Wells and Malcolm is the apparently greater initial severity of stuttering in their subjects. In our series it was noted that the more severe stutterers did tend to show disproportionately greater improvement than the others.

Another difference between these two series was the higher average dose of haloperidol maintained in the Wells and Malcolm series (2·25–4·5 mg per day), despite the necessity to reduce the dose of six out of 12 subjects because of side effects. In our series, 15 of the 18 subjects were unwilling to tolerate doses of 4·5 mg/day, and the mean dose stabilized at 2·5 mg/day. As in the Wells-Malcolm and Tapia series, drowsiness was a common complaint, and many subjects also complained of poor concentration at their work. This resistance to what is usually considered to be a moderate dose of haloperidol was somewhat unexpected.

Concerning the second issue, the mode of action of haloperidol in stutterers, there is general agreement among speech pathologists that other potent anxiolytics have failed to improve the speech of stutterers significantly (Perkins, 1971). Furthermore, the reported success of haloperidol in controlling tics, including the Gilles de la Tourette syndrome (Connell *et al.*, 1967) has led some authors to propose that the drug also possesses a non-anxiolytic potency (Connell *et al.*, 1967). So this issue too would appear to be unresolved.

Encouraging reports based on the Wells and Malcolm series have appeared in the daily press. In the absence of an established treatment for stuttering, it is reasonable to anticipate yet another round of enthusiastic drug prescribing for stutterers, for which there are precedents in the 1950s. This communication wishes to point out that in two series only a minority of stutterers showed a substantial improvement, and would enjoin caution until the role of haloperidol in stuttering is more clearly established.

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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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