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RAYMOND TRAVERS

Olive Mount Hospital
Old Mill Lane
Wavertree
Liverpool L15 8LW

AUTHORS' REPLY: Dr Travers raises questions but offers few solutions. He is of course correct that there are many influences on the opiate withdrawal syndrome in addition to that described in our paper (*Journal*, May 1991, **158**, 697–699). However, while clinical practice might necessarily involve juggling with all of these simultaneously, the systematic study of these possible modifying factors requires a different strategy. We have previously reported on the differences between 10-day and 21-day withdrawal regimens (Gossop *et al*, 1989) and on the influences of different reduction curves (Strang & Gossop, 1988). Dr Travers rightly draws attention to the difficulty in identifying the relevant measures for the study of the opiate withdrawal syndrome. We are of the view that the SOWS (for development and validation see Gossop, 1990) and the actual measure of completion rates provide two differing and valuable perspectives. When all the criticism is said and done, what does Dr Travers actually suggest?

The most interesting points of the criticism are in the last paragraph where consideration is given to the possible different influence of methadone on heroin or methadone addicts. As we explained in the paper, virtually all of our subjects had originally been using heroin, and were assigned to the two groups according to their most recent opiate of use before admission. Thus if drug cues are being considered, it is unlikely that they would be substantially different across the two groups. Perhaps it is being suggested that the drug cues may relate to the preparation being used – the linctus – but this is unlikely as we use a differently coloured and specially prepared linctus on our inpatient programme. There are also important cognitive cues that influence withdrawal responses. Some of these have been considered by Wikler (1980) as well as in our own previous research.

Finally, Dr Travers questions whether the findings are of any relevance in view of the similar discharge rates. From a clinical management perspective there may be some legitimacy to this point of view, although we would have thought that reducing the levels of within-treatment distress remained a legitimate clinical goal, notwithstanding the similar completion rates. However, we would also draw attention to a more general point: for too long the treatment of addicts has muddled along, driven by chemical whim, with insufficient investment in the systematic study of the treatment being delivered.

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JOHN STRANG
MICHAEL GOSSOP

Drug Dependence Clinical Research and Treatment Unit
The Bethlem Royal Hospital
Monks Orchard Road
Beckenham
Kent BR3 3BX

Cerebral ventricular size and dyskinesia

SIR: McClelland and colleagues (*Journal*, May 1991, **158**, 691–696) report an interesting association between cerebral ventricular size, at the end of 16-years follow-up and the development of facial dyskinesia during the period in a group of 23 patients with functional psychosis. This data adds to 12 previous studies, the results of which divide evenly between supporting and refuting an association between current ventricle:brain ratio (VBR) and current dyskinesia. A smaller group of studies are divided over the importance of cortical atrophy. A confounding problem in McClelland *et al*'s findings may be the effect of ageing on brain structure. While the 18 years from first assessment for dyskinesia to computerised tomography (CT) scan would be accompanied by some ventricular changes, it would also be accompanied by some development of cortical atrophy. This is particularly likely in those patients who converted from dyskinesia negative to positive as their mean age at first assessment was 53 years. Thus it may be equally possible that the development of dyskinesia in this group could be associated with cortical atrophy.

Recent findings of our own would support this view (Cooper *et al*, 1991). We have found no relationship between VBR and the presence or absence of

dyskinesia in a group of 79 schizophrenic patents studied cross-sectionally. However, we do find that those with dyskinesia have greater cortical atrophy. Indeed the association with cortical atrophy seems due particularly to greater frontal atrophy (assessed separately) in patients over 55 years of age with dyskinesia. While our current study provides a more detailed assessment of CT variables in a larger sample than many previous studies, it also supports a further hypothesis in relation to the development of dyskinesia.

We would suggest that the integrity of fronto-striatal pathways may be relevant to whether or not dyskinesia is evident in patients. It is well recognised that dyskinesia becomes more prevalent with age, and the development of increasing frontal atrophy with age, from the sixth decade onwards, may explain this. Normally, fronto-striatal pathways would suppress some aspects of striatal outflow. Animal models of dyskinesia suggest that neuroleptic drugs may sensitise the striatum to producing dyskinetic movements. However, in humans these movements may not become evident in many cases until fronto-striatal suppression is lost. This would not only help to explain the increasing prevalence with age but also a variety of other observations such as: (a) greater cognitive impairment in dyskinetic versus non-dyskinetic patients may be due to cortical atrophy; (b) association of dyskinesia with soft neurological signs (King *et al*, 1991); (c) the lack of consistency in demonstrating a clear relationship of dyskinesia to prior neuroleptic treatment might be expected if another pathological process is necessary for full expression of the drug effect. Such a hypothesis would also be consistent with suggested frontal lobe deficits in schizophrenia itself (Morihisa & Weinberger, 1986). A few patients display dyskinesias at an early stage of illness, even in the absence of drug treatment. They may be those with more marked frontal impairment.

It would be interesting to know if further examination of the CT scans from the study of McClelland *et al*, would lend support to this hypothesis.

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STEPHEN J. COOPER
Department of Mental Health
The Queen's University of Belfast
The Whitla Medical Building
97 Lisburn Road
Belfast, Northern Ireland

MICHAEL M. DOHERTY
Belfast City Hospital

JOHN WADDINGTON
Royal College of Surgeons in Ireland

Verapamil in major depression?

SIR: Jacques & Cox (*Journal*, January 1991, **158**, 124–125) reported the case of an 82-year-old woman who suffered from what they diagnosed as a major (psychotic) depression, who dramatically and accidentally responded to treatment with verapamil. As a consequence, they suggest a possible aetiological role for calcium in affective disorders.

However, if the case is carefully examined *as described in the article*, the diagnosis must be in question. It is more likely to have been an organic affective syndrome of vascular aetiology since:

(a) The patient had no personal or family history of psychiatric disorders. The depressive syndrome appeared at an advanced age which means that an organic aetiology should be considered.
(b) The patient suffered hypertension of 20 years duration which lately was poorly controlled (210/100 mmHg).
(c) The onset of symptoms coincided with a stressful life event, but psychiatric symptoms of vascular aetiology can be precipitated by stressful events.
(d) The development of a supraventricular tachycardia and cardiac failure two days after the second electroconvulsive therapy (ECT) confirms the previous poor cardiovascular state.
(e) Mental symptoms worsened with an antidepressant medication (fluvoxamine, 300 mg daily) which would not be expected in a primary depression.
(f) The quick, complete and simultaneous recovery of her cardiac and psychiatric symptoms after treatment with verapamil and frusemide.
(g) The absence of depressive relapse in the four-month follow-up on no antidepressant medication.

All these suggest that the diagnosis of an affective organic disorder of vascular aetiology should be considered before thinking of major depression. In