The Dexamethasone Suppression Test in Depressed Kuwaiti Patients

SIR: We read with interest the multicentre WHO collaborative study of the response to the dexamethasone suppression test (DST) (*Journal*, April 1987, **150**, 459–462). The only Arab centre involved in this study was in Casablanca, Morocco (12 patients). In Kuwait, we recently carried out a similar study on 19 Kuwaiti depressed in-patients (11 males and 8 females; mean age = 34.3 ± 2.2 years) and 23 healthy controls.

Our study supported the view that post-dexamethasone plasma cortisol concentration is higher among depressed patients than among controls. The mean post-dexamethasone plasma cortisol concentration was 95.5 ng/ml, which is higher than the Casablanca results (37.4 ng/ml) and shows that variations are present even between the two Arab centres.

The percentage of abnormal responses was 32%, which is higher than at the Casablanca centre (25%). The range of variation in the WHO study was from 15% to 71%. DST response was normal in the controls.

The absolute value for serum cortisol decrease between 8.00 h and 16.00 h could not be used to predict and separate patients who were suppressors from those who were non-suppressors.

There was no statistically significant difference between suppressors and non-suppressors in the Hamilton Rating Scale for Depression (HRSD) scores before and after treatment. No differences were detected in total HRSD scores between males and females, either before or after treatment.

No significant correlation was elicited between initial serum cortisol levels and HRSD scores before or after treatment.

Comparing two types of antidepressants (amitriptyline and mianserin), it was found that the former was a more effective treatment in both suppressors and non-suppressors, while the latter was clearly ineffective as a treatment in suppressors. It seems that the question of possible involvement of factors such as geographical location and climate has to be considered when interpreting differences between centres, especially those with similar ethnic features.

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Reference

HAMILTON, M. (1967) Development of a rating scale for primary depressive illness. *British Journal of Clinical and Social Psychology*, **6**, 278–296.

Chronic Schizophrenia and Long-term Hospitalisation

SIR: In Dr Abrahamson's first letter about 'institutionalisation' (*Journal*, September 1986, **149**, 382), he appeared to argue that negative symptoms in schizophrenia were immutable. This premise seemed to be based only on the fact that the relationship between negative symptoms and length of stay often found in cross-sectional studies (he quoted a study by George Brown and myself in this context) could be explained by an accumulation of more severe cases in hospital as the less severe were discharged. In my reply (Journal, January 1987, **150**, 129–133), I pointed out that we had specifically argued against any cause and effect relationship and, instead, had provided evidence, in a comparative longitudinal study, that 'poverty of the social environment' did seem to increase 'clinical poverty', at least in a proportion of cases. More specifically designed experiments could not disprove the hypothesis.

In his second letter (Journal, November 1987, **151**, 708), Dr Abrahamson said that he had intended to agree with our point, and I am grateful for the assurance. However, what he gave with one hand he took away with the other, since he went on to juxtapose two quotations (100 pages apart) that he thought demonstrated that we had, after all, claimed that length of stay in hospital caused deterioration. What we in fact suggested was that social conditions in the three hospitals were responsible, acting through a biological vulnerability, for part of the deterioration *or lack of deterioration* found in their respective residents. The final sentence quoted by Dr Abrahamson illustrates the point. One fifth of the patients who remained in hospital during 8 years of the study (excluding those discharged) actually improved with time. If social conditions change with time, so will the negative symptoms of some patients. Length of stay, as such, is not a key factor.

That my interpretation of Dr Abrahamson's first letter was close to the mark is also suggested by his new statement that attitudes to discharge from hospital are entirely explained in schizophrenic patients by the severity of their disorder. We found that moderately impaired patients were more likely to have favourable attitudes than the severely impaired, but that controlling for severity, for example of blunting of affect or poverty of speech, did not remove the relationship to length of stay. Many people with