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TARDIVE DYSKINESIA

DEAR SIR,

I am writing in reference to the article on Tardive Dyskinesia published in your December 1972 issue (**121**, 605–12). This is a subject which continues to command interest. Part of the problem is that dyskinetic phenomena may occur even early in treatment. I have seen them within six months of instituting neuroleptic treatment. However, in these instances it disappears with the discontinuation of medication. The question of the incidence of permanent neurological impairment remains an unanswered one, as does the treatment. Three points seem to be of importance in this area:

(i) Tardive dyskinesia disappeared in three manic patients (erroneously diagnosed as schizophrenics and treated with neuroleptics) when they were changed to lithium therapy. In other non-manic cases, improvement of symptoms took place when the patients were given lithium. These have been uncontrolled studies, but are of sufficient interest to warrant further investigation.

(ii) In the past year I have seen several other patients, not schizophrenics, who presented with severe tardive dyskinesia when receiving neuroleptics. One was a patient who had received 100 mg. of thioridazine daily for 10 years. He had a well-marked bucco-lingo-masticatory syndrome which disappeared when medication was withdrawn. More important was the appearance of a crippling tardive dyskinesia in a neurotic patient who had received no more than 10 mg. of trifluperazine daily for less than a year. It did not remit upon withdrawal of medication. I have also seen two involutional depressives who were treated with neuroleptics, both of whom were unable to dress themselves or function in the outside world because of their dyskinesias. These observations have important implications in terms of the use of neuroleptics for treating neurotic conditions and particularly for treating depressions. There is a considerable literature suggesting that neuroleptics are a good treatment for certain types of depression, and it has also been stated that neuroleptics are the treatment of choice for 'agitated depression' (1). On the basis of the above-listed cases, I would say that neuroleptics are contraindicated in the treatment of such patients, since there are no data to suggest that these are better than antidepressant medication, which certainly does not produce tardive dyskinesia.

(iii) The third and last point relates to why some patients on small dosages of medication should develop this syndrome. The last three of the patients mentioned above received medication for a short period of time only, and certainly did not ingest anything like the large amounts that chronic schizophrenics may have received. All were female and all were of Eastern European Jewish background. The question therefore, arises as to whether, as with the congenital dystonias, Eastern European Jews may have an increased susceptibility to this syndrome. It has even been suggested that the administration of small amounts of neuroleptics may be a suitable way to detect the heterozygous carriers of congential dystonias (2).

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BENIGN MYALGIC ENCEPHALOMYELITIS

DEAR SIR,

May I be permitted to draw attention to the fact that the paper entitled 'A controlled follow-up of cases involved in an epidemic of Benign Myalgic Encephalomyelitis' by Drs. McEvedy and Beard (*Brit. J. Psychiat.* (Feb. 1973), **122**, 141), is merely a prolongation of the thesis they submitted through the columns of the *British Medical Journal* of 3 January 1970. In a letter to the *B.M.J.* (1970, **1**, 362) Drs. N. Compston, H. Dimsdale, A. T. Richardson and myself pointed out that while a diagnosis of hysteria had been seriously considered at the time of the outbreak, the occurrence of fever in 89 per cent, of lymphadenopathy in 79 per cent, and of ocular palsy in 19 per cent, rendered it quite untenable. In the same issue Dr. E. D. Acheson, who had personal experience of cases at the Middlesex Hospital, stated that he too had considered a possible diagnosis of hysteria but for similar reasons had ruled it out. Most important evidence favouring our view that the condition was infective in origin was the occur-

rence of sporadic cases in the general population of North-West London for two months before and for at least two years after the outbreak in the hospital. A considerable proportion of these patients were extravert types of stable personality with no history of previous illness of any kind. A further outbreak of 370 cases of the disease seen by Dr. Betty Scott in Finchley between 1964 and the summer of 1966 was described in the *British Medical Journal* (1970, **i**, 170).

The totally irreconcilable line of cleavage from the opinions expressed by Drs. McEvedy and Beard lies in the fact that all the physicians of the Royal Free Hospital who had care of cases in 1955 were in no doubt that the symptoms were organically determined and could not possibly be regarded as 'pure' hysteria. Final and complete refutation of the McEvedy and Beard hypothesis was advanced by Dr. David C. Poskanzer (*B.M.J.* 1970, **ii**, 420), who reminded us that in an outbreak in New York he and his colleagues (Albrecht, R. M., Oliver, V. L., and Poskanzer, D. C. (1964) *Journal of the American Medical Association*, **187**, 904) demonstrated a considerable increase in creatinuria and an increase in the creatine/creatinine ratio, suggesting an abnormality of muscle; on recovery this disappeared. He makes the very intriguing suggestion that 'instead of ascribing benign myalgic encephalomyelitis to mass hysteria or psychoneurosis' Drs. McEvedy and Beard might 'consider the possibility that all psychoneurosis is a residual deficit from epidemic or sporadic cases of benign myalgic encephalomyelitis'. I trust that all fair-minded psychiatrists would agree that this view should be accorded serious consideration before consigning these unfortunate patients with their functional prolongation and tendency to relapse to the implied stigma of 'pure hysteria'.

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VIOLENCE AMONG ATTENDERS AT A LONDON DRUG CLINIC

Dear Sir,

Gordon (1973) finds that clinic attenders have more violent-crime convictions after first drug use than before. However, the *per year* rate for violence convictions before and after drug use is about the same—and larceny lower! Even with liberal assumptions (e.g. that the pre-drug period should exclude only ages 13 and younger) the data corrected for time fail to show a statistically significant increase.

Moreover, the post-drug conviction rate is almost certainly enlarged because the sample patients are subject to a very high rate of police surveillance. And even if offences did increase following drug use, non-drug events could be responsible. In fact a non-opiate sample having the same early conviction records might well have much higher rates of later convictions than the clinic sample.

Gordon elaborates only slightly on the actual behaviours that constitute 'indictable offences of violence'. His examples of 'more serious' crimes of violence include six items—among them, dangerous driving and malicious damage.

He states, 'There is little to suggest that the findings would be specific to this clinic . . .'. However, the proportion of people without criminal convictions in Gordon's sample is less than half of the corresponding proportion for people approaching all of London's drug clinics the following year.

Finally, it should be made explicit that the results seem in no way attributable to clinic treatment itself, as most of the 'post-drug' period precedes clinic attendance.

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BEHAVIOUR THERAPY IN MENTAL DISORDERS

Dear Sir,

Our booklet, *Behaviour Therapy in Mental Disorders*, was reviewed in your *Journal* in February this year (**122**, 229), and we were not surprised that such a parochial publication should have been criticised in the way it was.

Nevertheless it seems unfortunate that the reviewer should pick out one case of marital disharmony which was in fact successfully treated. It seems to us that it is more important for the patient that his problems are solved than that they should be pedantically classified to satisfy the doctor's rigid requirements.

General practitioners are not inclined to read psychiatric books, even paperbacks, and anything that makes them aware of progress in the available treatments must be of some use.

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