British Journal of Psychiatry (1995), **166,** 537–544

Correspondence

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Neuroleptic malignant syndrome

SIR: While discussing the relationship between the neuroleptic malignant syndrome (NMS) and lethal catatonia (*BJP*, October 1994, **165,** 548–550), Osman & Khurasani offer three provocative theses: (a) that the real incidence of lethal catatonia in developing countries has decreased, with a concomitant real increase in the incidence of NMS, which is "found only since the 1980s"; (b) that those few neuroleptic-treated patients who develop NMS may do so as a result of receptor sensitisation following neuroleptic exposure *in utero*; and (c) that "lethal catatonia represents an advanced stage in the progression of psychosis when a massive blockade of the then hyperstimulated receptors occurs [. . .] while NMS is caused by a similar blockade but initiated by neuroleptics".

A clinical picture seemingly identical to the entity we now label NMS was first reported by Ayd (1956), while disordered thermoregulation and unexpected deaths were recognised complications of phenothiazine therapy in the psychiatric textbooks of the 1960s; however, it is reasonable to assume that the *diagnosis* is now made more frequently, probably as a result of its recent attention in the literature. As for lethal catatonia, its true incidence in the West may well have decreased in the last 40 years, secondary to improved diagnosis and management of its myriad medical causes (cerebral malignancy, metabolic derangements, encephalitis lethargica, etc.), but the apparent decline in the incidence of its psychogenic subtype might simply be an artefact of changing diagnostic fashions. More importantly, any person presenting to a modern psychiatrist with agitation and an altered sensorium is likely to be prescribed neuroleptics, to which are then attributed such signs of lethal catatonia as pyrexia and rigidity – and another diagnosis of NMS is made (Lindesay, 1986).

NMS was certainly reported before the 1980s, and it does not appear to be disproportionately commoner in persons under the age of 40; thus, it seems unlikely that pre-natal exposure to neuroleptics might play a significant role in its pathogenesis. Indeed, if this were the case, and since the authors postulate a molecularly identical pathogenic mechanism for lethal catatonia, one would expect a coincident rise in the number of cases of catatonia. In addition, I wonder how frequently neuroleptics were prescribed to pregnant women, given that the much-publicised thalidomide tragedy occurred within a decade of the licensing of chlorpromazine, and that standard psychiatric textbooks cautioned against its use in women of child-bearing age without appropriate precautions as early as the mid-1960s (Redlich & Freedman, 1966). If pre-natal exposure is central, one might expect a striking cohort effect on the incidence of NMS in patients presently aged between approximately 28 and 42.

The authors appear, in their final argument, to be postulating the existence of a physiological dopamine receptor antagonist, which "blockade(s)" these receptors. A perhaps more appealing alternative is Friedhoff's (1983) 'restitutive hypothesis' of dopamine receptor activity, as modified by Friccione (1985). They postulate that the known plasticity of the mesostriatal-mesolimbic dopaminergic system is important in protecting the brain against severe biopsychosocial stressors (which are thought to cause mesolimbic hyperdopaminergia) by means of an appropriately timed physiological mesolimbic receptor down-regulation. In most people, this homeostatic mechanism is sufficient to protect against psychosis; those for whom it is not may be helped by the further decrease in dopaminergic receptor sensitivity produced by neuroleptics. For some patients, however, this further reduction in general dopaminergic tone will result in NMS. As a corollary, they suggest that the primary mesolimbic hyperdopaminergia might induce a homeostatic response, via GABAergic feedback from the

nucleus accumbens, consisting of down-regulation of dopamine receptors in the mesostriatum and hypothalamus. Such a response might result in a reduction in local dopaminergic tone sufficient to produce lethal catatonia, despite mesolimbic hyperdopaminergia simultaneously producing psychosis.

In spite of the great attention these two possibly related entities have received of late, we still known remarkably little about the pathophysiology of either.

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Predictors of psychiatric morbidity in cancer patients

SIR: Harrison & Maguire's enjoyable review (*BJP*, November 1994, **165**, 593–598) referred to the work of myself and colleagues (Hughson *et al*, 1986) showing that chemotherapy increased psychological morbidity after mastectomy; they state also that this paper showed that the psychological consequences of radiotherapy after mastectomy persisted beyond six months.

I have never thought that this paper demonstrated persisting psychological morbidity due to radiotherapy. While it compared chemotherapy with radiotherapy, it did not contain a comparison with patients receiving no further treatment after mastectomy. Hence it could not distinguish between the effects of radiotherapy and the effects of having a mastectomy for cancer. However, in a subsequent paper (Hughson *et al*, 1987) we did compare patients having radiotherapy after mastectomy with those receiving no further treatment. Somewhat to our surprise, we failed to demonstrate any significant excess of anxiety or depression in the patients treated with radiotherapy, although they did show a significant excess of somatic symptoms and social dysfunction.

Despite this negative result, having interviewed all the patients personally, I am in no doubt that a few of them experienced anxiety or even panic caused by going under the radiotherapy machine. But I think that this anxiety was offset by the reassurance other patients got from getting further treatment and from having regular contact with the radiotherapy staff. The control patients having no further treatment after mastectomy had little contact with staff and hence less opportunity to be reassured. The psychological effects of having radiotherapy are more subtle than first appears, perhaps also varying with dosage (our patients did not receive very high doses), and I agree entirely with Drs Harrison and Maguire that further work is needed to look at the effects of radiotherapy for specific disease groups.

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Worcester Development Project and Powick Hospital

SIR: Those who worked at Powick Hospital, Worcester (including at least one ex-President of the Royal College), those who took an interest in the Worcester Development Project, and those who happened to read Ian Brockington's and my own book (1991), may be interested to know that the Worcester Development Project story has now finally ended.

It began with the proposition in 1970 that the then DHSS would close a single large mental hospital and replace it with a diverse range of psychiatric facilities in the community as a model national project. The eventual sale of the old mental hospital site would repay the original pump-priming capital invested.

Due to the insistence of all the mental health professionals involved (academic, DHSS and local) the old mental hospital was not closed until the new day hospitals, acute in-patient unit, hostels, etc. had been up and running for some time. The result of that foresight is that – unlike less fortunate areas – the citizens of south and mid-Worcestershire have had a viable and modern community-based